THE INSTITUTION OF AUTOMOBILE ENGINEERS

PROCEEDINGS SESSION 1920-21

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ERRATA.

PROCEEDINGS—VOL. XV.

P. 598, line 23, $N(R/(R_o + R_i)$ should read $N(R_o/(R_o + R_i)$.

P. 643, seventh line from bottom, 6·3 inch-lb. should read 76 inch-lb.

Fourth line from bottom, 6·3 should read 76.

Third line from bottom, 12·6 lb. should read 152.

Second line from bottom, 126 lb. should read 1520.

Last line, "more" should read "less."

THE

Institution of Automobile Engineers.

PROCEEDINGS

OF THE

SESSION 1920-21.

VOLUME XV.

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THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

Founded 1899. | Incorporated 1907.

Past Presidents.

1907-08. Col. R. E. B. Crompton, C.B., M.Inst.C.E.
1908-09. Dugald Clerk, D.Sc., F.R.S., M.Inst.C.E.
1909-10. H. S. Hele-Shaw, D.Sc., F.R.S., M.Inst.C.E.
1910-11. F. W. Lanchester, M.Inst.C.E.
1911-12. L. A. Legros, M.Inst.C.E.
1912-13. T. B. Browne, M.I.Mech.E.
1913-14. J. S. Critchley, M.I.Mech.E.
1914-15. J. S. Critchley, M.I.Mech.E.—Acting President.*
1915-16. L. A. Legros, M.Inst.C.E.—Acting President.*
1916-17. L. A. Legros, M.Inst.C.E.
1917-18. Brig.-Gen. R. K. Bagnall-Wild.
1918-19. A. A. Remington.
1919-20. Thos. Clarkson, M.Inst.C.E.
1920-21. Sir Henry Fowler, K.B.E., M.I.Mech.E.

* Col. Sir Capel Holden, K.C.B., was elected President for the Session 1914-15 and again for the Session 1915-16, but owing to his duties at the War Office could not take up office, and Mr. Critchley remained in office till the re-election of Mr. Legros in 1916.

The Crompton Medal.

The Medal that is now known as the Crompton Medal, and which was kindly presented to the Institution by Mr. L. A. Legros in 1912 for the best Paper read during the Session, has been awarded to the following members :—

1912–13. To Mr. G. H. Baillie for his Paper on "Springs."

1913–14. To Mr. L. H. Pomeroy for his Paper on "The Elements of High Speed Engine Design."

1914–15. To Dr. W. Watson for his Paper on "Benzol, Alcohol and Mixtures of these Liquids with Petrol as Fuel for Internal Combustion Engines."

1915–16. To Mr. J. H. S. Dickenson for his Paper on "The Choice of Steel in Automobile Construction."

1916–17. To Brig.-Gen. R. K. Bagnall-Wild for his Paper on "The Use and Abuse of Steel."

1917–18. To Major H. P. Philpott for his Paper on "Some Experiments on Notched Bars."

1918–19. To Mr. A. E. Berriman for his Paper on "An Analysis of Test Records of some Petrol Engines."

1919–20. To Mr. A. P. Young and Mr. H. Warren for their Paper on "The Process of Ignition."

The Graduates' Prize.

The Prize awarded annually by the Council for the best Paper read by a Graduate before any of the Centres has been won by the following :—

1908-09. To Mr. F. Thomas (London Centre) for his Paper on "Change Speed Gears, Transmission and Reduction of Speed."

1909-10. To Mr. A. E. Plant (Birmingham Centre) for his Paper on "Jigs and Tools for Motor Car Work."

1910-11. To Mr. R. K. Hubbard (London Centre) for his Paper on "Agricultural Motors."

1911-12. To Mr. T. E. B. Chalmers (Coventry) for his Paper on "Carburation."

1912-13. To Mr. W. Betterton (Coventry Centre) for his Paper on "Processes in the Production of Gears."

1912-13 (second prize). To Mr. G. M. Junner (London Centre) for his Paper on "Carburation."

1913-14. To Mr. A. Cooke (London Centre) for his Paper on "Grinding."

1913-14 (second prize). To Mr. R. Coulson (Coventry Centre) for his Paper on "Chain Driving."

1914-15. To Mr. M. Farnell (Coventry Centre) for his Paper on "Lubrication."

1915-16. To Mr. H. Warren (Coventry Centre) for his Paper on "Electrical Equipment of Automobiles."

1916-17. To Mr. C. Pole Wedmore (Coventry Centre) for his Paper on "Valve Timing."

1916-17 (second prize). To Mr. H. Towns (Coventry Centre) for his Paper on "Welding."

1917-18. To Mr. C. Pole Wedmore (Coventry Centre) for his Paper on "Pistons."

1917-18 (second prize). To Mr. D. S. Heather (Coventry Centre) for his Paper on "Brakes."

1918-19. To Mr. C. Pole Wedmore (Coventry Centre) for his Paper on "Frames."
To Mr. A. J. Pendrell (Coventry Centre) for his Paper on "Design versus Production."

1919-20. To Mr. D. L. Prior (Coventry Centre) for his Paper on "Epicyclic Gearing in Automobile Practice."

1919-20 (second prize). To Mr. W. E. Benbow (London Centre) for his Paper on "Chemical and Physical Properties of Iron and Steel."

The Daimler Premium.

The "Daimler Premium" of £25 placed at the disposal of the Council for award to the Graduate who submits the best Paper on an appropriate subject in any Session has been awarded to the following Graduates :—

1917-18. To Mr. H. Warren (Coventry Centre) for his Paper on "The Operation and Design of Sparking Plugs."

1918-19. To Mr. L. Griffiths (Coventry Centre) for his Paper on "The Testing of Magnetos."

1919-20. To Mr. D. L. Prior (Coventry Centre) for his Paper on "Epicyclic Gearing in Automobile Practice."

CONTENTS.

PAGE

List of Past Presidents ii

Winners of Crompton Medal iii

Winners of Graduates' Prize iv

Winners of the Daimler Premium iv

References to Dimensions, &c. of 1921 Cars, Commercial Vehicles and Cycles viii

Index to Speakers and Contributors ix

"Some Notes on Production" (Presidential Address), Sir H. Fowler 6

"The Springing of Motor Cycles," G. H. Savage 35

"Some Notes on the Report of the Steel Research Committee," J. H. S. Dickenson 99

"Roads and Vehicle Maintenance," Col. R. E. B. Crompton 167

"The Combustion of Naphthalene Solutions in Internal Combustion Engines," L. S. Palmer 195

"Motor Bicycle Frame Design," G. D. Leechman 219

"Suggested Lines of Development in Transmission," Capt. S. Bramley-Moore 261

PAGE

ANNUAL DINNER 373

EXTRAORDINARY GENERAL MEETING 375

"THE DESIGN AND MANUFACTURE OF PNEUMATIC MOTOR TYRES,"
Colin Macbeth (London) 379

"THE DESIGN AND MANUFACTURE OF PNEUMATIC MOTOR TYRES,"
Colin Macbeth (Birmingham—Discussion only) 455

EXTRAORDINARY GENERAL MEETING 461

ANNUAL GENERAL MEETING 462

"FURTHER NOTES ON AUTOMOBILE STEELS,"
Dr. W. H. Hatfield 465

"A SUGGESTED PROGRAMME FOR AUTOMOBILE RESEARCH,"
Geo. Watson 511

"THE BALL BEARING, IN THE MAKING, UNDER TEST AND ON SERVICE,"
H. L. Heathcote 569

"COMPARATIVE MOTOR CYCLE PERFORMANCES,"
D. S. Heather (Birmingham Discussion only) 705

"SOME EXPERIMENTS ON SUPERCHARGING IN A HIGH SPEED ENGINE,"
H. R. Ricardo 717

SCOTTISH CENTRE,

"Roads and Vehicle Maintenance," Col. R. E. Crompton (Discussion only) 777

"Suggested Lines of Development in Transmission," Capt. S. Bramley-Moore (Discussion only) 791

"The Design and Manufacture of Pneumatic Motor Tyres," Colin Macbeth (Discussion only) 805

"Producer Gas Practice," John Stewart 813

"Comparative Motor Cycle Performances," D. S. Heather 839

WOLVERHAMPTON CENTRE 883

PAGE

"Epicyclic Gearing in Automobile Practice" (Daimler Premium Paper), by D. L. Prior (Coventry Centre). 887

Graduates' Section 919

Summer Visit 927

Library Catalogue 931

Annual Report and Accounts 943

List of Members and Committees 959

Topographical List of Members 1011

Memorandum and Articles of Association 1033

Index to Proceedings 1059

Subject Matter Index, Vols. XI.—XV. 1077

Plates 1101

REFERENCES TO DETAILS OF CARS.

Dimensions and fairly complete particulars of 1921 Motor Cars, Commercial Motor Vehicles and Motor Cycles are to be found in the following publications:—

"Light Car and Cycle Car."—February 19th, 1921.

"Automobile Industries."—February 17th, 1921.

"Motor Cycle."—March 17th, 1921.

"Autocar."—April 16th, 1921.

"Motor Transport."—October 3rd, 1921.

"Motor, Marine and Aircraft Red Book."—Technical Publishing Co.

ERRATA.

P. 206, line 13.—For page 1, read page 195.

Plate VI.—For Fig. 54, read Fig. 58.

SPEAKERS AND CONTRIBUTORS.

PAGE

Acres, F. A. S. 246, 249, 455, 707
Aitchison, Dr. L. 495, 539
Allan, R. 668, 680
Armstrong, P. A. E. 560
Arter, A. M. 687
Aveling, T. C. 708

Bagnall-Wild, Brig.-General R. K. 547
Beaumont, W. Worby 186
Berriman, A. E. 547, 754
Berry, J. F. 249, 709
Bramley-Moore, Captain S. **261**, 333, 364, 798
Brayshaw, S. N. 499
Brearley, Hy. 134
Brook, Major R. V. 75, 708, 709
Bunn, G. 555
Burgoine, Captain A. C. 213
Burt, Captain S. J. 76

Caudwell, E. 254, 707, 709
Chalmers, Captain T. E. B. 83
Clarkson, T. 31, 181, 675
Cooper, T. W. 663
Cree, — 805
Crompton, Colonel R. E. 28, **167**, 187, 193, 784

Daniels, W. 781
Davidson, Lieut.-Colonel A. E. 90, 441
Dickenson, J. H. S. **99**, 146, 150, 160
Drummond, D. 777, 784
Duffy, J. 457

PAGE

Elvidge, H. 781
Evans, A. F. 213, 549

Fowler, Sir Hy.....**6**, 145, 213, 249, 457, 550, 709, 757
Fulton, N. O. 791

Garrard, C. R. 678
Goodman, Prof. J. 689
Greenwood, J. E. 73
Guest, J. J. 329

Hadfield, Sir Robt. 130
Hatfield, Dr. W. H. 157, **465**, 503, 507
Heathcote, H. L. 182, **569**, 684, 692
Heather, D. S. 710, **839**, 878
Henderson, J. F. 791, 805, 828, 872
Higgs, Colonel H. J. 672
Higham, W. H. M. 81
Hingston, W. H. 326
Holroyd, M. 84, 247
Hounsfield, L. H. 144
House, C. E. G. 557
Hutchinson, Lieut.-Colonel T. M. 142, 558, 670

Invernairn, Lord 158

James, — 143
Joyce, P. J. 247, 256, 456, 707
Junner, G. M. 679

Keachie, D. 782, 796, 806, 831

Lanchester, Dr. F. W. 185, 309
Lanchester, G. H. 346
Lawrence, Max R. 324, 548, 676
Leechman, G. D. **219**, 250, 257
Legros, L. A. 331, 498, 559, 673

PAGE

MACBETH, COLIN 379, 445, 457, 807
MACDONALD, — 805
MARTIN, J. A. 875
MAXWELL, R. M. 554
MCCAW, G. E. 778, 828
MCGILLIVRAY, J. A. 875
MERCER, T. S. 683
MILLS, J. W. 793
MORGAN, R. T. 79, 89
MORGAN, PROF. W. 210
MUIRHEAD, D. P. 553
MURRAY, G. 158

NORTH, O. D. 332, 442, 549, 677
NORTON, J. L. 243

ORMANDY, DR. W. R. 180, 211, 439, 540, 550, 747
OWEN, MAJOR A. D. 545

PALMER, L. S. 195, 214
PARKES, J. D. 795, 829, 878
PARNACOTT, A. E. 191, 329, 544, 667
PEARSALL, R. H. 80, 705, 708
PHILPOT, MAJOR H. P. 490
PRIOR, D. L. 887
PUGH, J. 680

REMINGTON, A. A. 132, 330, 490, 752
RENWICK, W. S. 753
RICARDO, H. R. 717, 757, 767
RICHARDSON, J. 793, 828, 874
RICHARDSON, J. J. 84
RITCHIE, E. G. 764
RODWAY, G. H. 682
ROLFE, R. T. 140
ROSENHAIN, DR. W. 136, 487
ROWELL, H. S. 556
ROWLEDGE, A. J. 550, 687
RYAN, D. V. 457

Roderick Henry Boyd (London).
Henry John Bretton (London).
William Richard Buckingham (Feltham).
Hubert Noel Charles (London).
Joseph Cross (Manchester).
John Currie (Wolverhampton).
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EUGENE OSCAR WILLIAM LA VALETTE PARISOT (Whyteleafe), Probationary Graduate to Graduate.
STANLEY RUSSELL PHILPOT (Coventry), Probationary Graduate to Graduate.
WILLIAM HAROLD WARDALL (Slough), Probationary Graduate to Graduate.
BASIL TOM GILL WILLIAMS (Taplow), Probationary Graduate to Graduate.

Mr. T. CLARKSON: It is my privilege to-night to induct my successor, Sir Henry Fowler, into the Presidential Chair, and I feel I am leaving the destinies of the Institution in very good hands. All who know anything of automobile engineering history know that Sir Henry Fowler is one of the early pioneers, and he has always been a very keen observer of the performances in

various automobile trials. As long as twenty years ago he was acting as official observer in the famous Liverpool trials, and I remember on one occasion, when I was trying, somewhat unsuccessfully, to manœuvre a lorry in one of the hill-climbing competitions, Mr. Fowler, as he was then, very narrowly escaped a serious accident in his efforts to observe more closely what was going on. On yet another occasion, as showing Sir Henry Fowler's keenness in automobile engineering, some fifteen years ago, Sir Henry followed a steam 'bus which I was driving all the way from Derby nearly to Matlock on a push bicycle in order to be in the best position to make his observations. The roads then had not been tarred or prepared in any dustless fashion, and I am afraid he had a pretty bad time following that 'bus. I would like to express my pleasure at being in the fortunate position of inducting Sir Henry Fowler into the Presidential Chair.

Sir Henry Fowler then took the Chair and delivered his Presidential Address, "Some Notes on Production."

There were present 120 Members (including seven Past Presidents) and visitors.

SOME NOTES ON PRODUCTION.

Presidential Address.

By Sir Henry Fowler, K.B.E.

I am very much honoured in having been elected President of this Institution, especially when I remember the eminence in the engineering profession of so many of my predecessors in the Chair. I appreciate, too, the fact that the coming year may be an important one in the history of the industry which this Institution represents and of the Institution itself. We have never had such an opportunity of drawing into the Institution so many of the young men who have recently joined the engineering side of the industry, and I trust that by the individual efforts of the members this coming session may see a record increase in our numbers. This is essential from two points of view. In the first place it will put the Institution more firmly in the position it should occupy as being absolutely representative of the engineering side of the industry; secondly, it is essential from a personal standpoint, for there is no doubt but that membership, especially at an early age, is of the greatest benefit to the member himself if he will utilise the opportunities which offer. In automobile engineering, perhaps more than in any other branch of engineering, it is beneficial to meet and mix with others who are working in the same direction, and to have the advantage of free and frank discussion. This need not always be of the formal type which takes place after the reading of a paper on a definite subject, but those personal discussions between two or three which always take place after the meetings. I would urge on the older members especially the desirability of extending every possible encouragement to the juniors of the Institution who attend the meetings, etc. This intercourse is one of the greatest benefits of summer meetings,

and successful as our recent one was, it is to be hoped that in the future even more Members, and Graduates too, may attend all such functions. It is a very pleasant way of carrying out what is strictly business.

Before proceeding to the main theme of my Address, I feel I must refer briefly to the recognition which the work of the Institution has received during the past year from the two great motor trade bodies of this country—The Society of Motor Manufacturers and Traders and The Association of British Motor and Allied Manufacturers. Both these bodies now look upon the Institution as their representative on the technical side, and as responsible for assisting them in the production of the vehicles which it is their particular province to deal with from the commercial point of view. The accord which has been brought about has already led, among other things, to the granting by the Council of the S. M. M. T. of a most valuable privilege to members of the Institution in that they will be admitted free to the Motor Shows at Olympia on the special view days before the opening of the Shows to the public. As admission on these days will be strictly limited, all will have proper facilities for really seeing the Shows and studying as they ought to do the technical features of the various vehicles exhibited, and so keeping themselves up to date with current practice. The value of this privilege will be fully appreciated by those who have endeavoured to gain information among the seething crowds of idle spectators at Shows in the past.

I believe that it is generally recognised that at the present there is no more urgent engineering necessity in this country than the production of saleable articles. During the late war and under the incentive of patriotism this country reached a state of production—in articles of destruction it is true—which six years ago we should have looked upon as beyond the dreams of possibility. It is just as essential to-day that we should produce, but the incentive seems to have disappeared.

I propose to deal briefly with this point, although many pens have been writing and some voices raised on the subject. I do not know that I can add materially to what those who have issued a message before have said, but I will ask your forbearance on a matter which I feel appeals to each one of us now as it has done during the past few years. As a rule in speaking on such subjects it is usual to deal first with the machine and then with the man.

I think this is reversing the importance of the subject, and so propose first of all to deal with the human side.

It is not necessary to explain why it is needful to deal with this side, although a comparatively few years ago it was of secondary importance. The whole trend of the movement of the world is in the direction of study of the individual and individuals, and Chairs of psychology are springing up at various Universities. Associations, such as the Industrial League and the Industrial Council, now happily amalgamating I believe, are giving serious attention to the various phases of psychological study. This joint Association is, I think, one that should receive every encouragement. Lately the League and the Council have considerably extended their scope by forming branches in various towns, and this gives an opportunity of meeting local labour leaders outside the Conference Rooms and of discussing views at more leisure than there is opportunity for when some particular case is being dealt with.

I do not, however, propose to go deeply into the theoretical side of the question, but to deal with it chiefly as it appeals and appears to a practical engineer dealing day by day with men and materials. As an old chief of mine so truly said when he was giving me my first charge of men after having had charge of the testing and inspection with a large firm: "In the past you have dealt with many intricate questions in connection with material, now I am going to give you a much more complicated job—I am putting you in charge of men." We have all of us given—if often unconsciously—some attention to the subject, for when serving our time we were always considering and studying the state of mind of our "mate" or our labourer, and this was probably the greatest education we had in our shop experience. If we benefited by it as we should have done, we appreciate to some degree the thoughts and aims of those we are now in charge of. I say to some degree advisedly, for changes take place in the ideas and ideals of labour, and probably no period has seen so great a general change as that which has taken place in the past six years.

Originally we bargained or dealt with individuals, then with fairly small groups, but now we are often only a unit in a body which discusses matters with the representatives of very large bodies of men, oftentimes of very diverse trades and occupations. I think, however, that it may truly be said that the greatest asset which a man can have in these dealings, whether with an individual or with the whole of a trade, is to have a reputation for fair dealing.

This may seem to be a truism, but there are times when an oversight gives him an advantage, if he desire to take the word rather than the spirit as the proper interpretation of an agreement. I do not think that labour disputes the right of the other side to fight hard for what they believe are their rights, and they expect the same consideration to be given to them.

There have arisen during the past few years various systems of getting in touch with labour, and although the general methods are still to some extent heterogeneous, they approximate broadly in the engineering trade to a huge Whitley Council. The organisation of this latter is probably well known to all, and in certain specified trades it is said to be working well in its entirety. I do not wish to deal with it or with the larger organisations in our own industry, but with matters which affect our own individual works, and then to deal with a few other matters which affect labour and work in our shops, which will perhaps be looked upon as details.

I must first of all deal with Shop or Works Committees. The constitution of these is not standardised, and it is questionable if it is really desirable that this should be done. Every shop varies in its physical geography and in the distribution of its labour; the main point is that every employee may be fairly and truly represented, and that the Committees are not only representative but have the confidence and respect of those they represent. As a rule all questions of rates are settled for the firm and the employees by conferences covering larger bodies, but points of classification are sure to crop up from time to time, and it is well if these are, as far as may be, settled for every man on the ground. It is doubtful if it is desirable for any definite intervals to be fixed as the maximum for meetings between the management and the committee, the better arrangement seems to be that they shall be called at any time either side desires. In many cases the whole of the Committee need not attend, but possibly the Chairman or Deputy Chairman, the Secretary and one or two of the members concerned, together with any individual who has special knowledge of the matter under discussion, may meet one or more members of the management.

Some may argue that there is nothing left nowadays for such a Committee to deal with, as settlements are so general, but I believe there never was a time when they were so necessary. Questions with regard to the interpretation of agreements and the putting into operation of awards are constantly cropping up.

Points dealing with classification of men and the hundred and one little matters which occur in a works have also to be dealt with. It will be realised that if there is any cause of dissatisfaction in a works on the part of a group of men or even of an individual, there is nothing much worse from a production standpoint, and the time for dealing with it is as near its inception as possible. It may be argued that many points are trivial and will finally clear themselves up, but even if there were not the chance that an apparent triviality might grow into a serious trouble, this is surely not a sound argument when dealing with a bit of grit in the machinery of a works organisation. Not only so, but it can be said from experience that a properly organised Committee will frequently help the management by clearing away misconceptions which will always arise. In this, too, they are infinitely better placed than any management can possibly be. As was put to me at a works I had charge of a few years ago, not a tenth of the complaints raised with the Committee ever came forward, most of them were founded on mistaken ideas which the Committee were able to clear away without any representation to the management. The Committee, too, can be useful in disseminating information with regard to various matters which the firm wishes its employees to be acquainted with. Rumours of change of design, of trouble with material, of cancellation of orders, etc., are always unsettling, and a frank talk with the members will be helpful. My friend and late colleague, Mr. Webster Jenkinson, C.B.E., F.C.A., read on January 28th, 1919, before a Conference of the Industrial Reconstruction Council, a most interesting paper on "The Worker's Interest in Costing (a Factor in Industrial Reconstruction)." In this he dealt fully not only with the necessity of costing, of which I shall have something to say later, but also with the desirability of enlightening labour more than has been the case in the past as to the state of affairs generally. In times past when shops were so small that the works manager was in the happy position of being able to address each employee by his Christian name, this could be done, and probably was done, by a word dropped here and there. We often sigh for those days when personal contact was possible, and it was felt that each one was a necessary integral part of the concern. Now with our huge shops and systems in management such a personal touch is impossible, but we may keep in contact with this side of our work through our Committees if we will only impress them and through

them the other workers that we look upon them as men to be dealt with as men by men, and, as I have said before, that we are out to do a straight deal with them not only when we want to be pleasant but on every occasion. It is so very difficult to get at the point of view of the other fellow, and we must make every effort to do so whenever possible. We are so apt to persuade ourselves that, because we once worked at the bench for a very few years a good many years ago, we understand what the mass of men feel to-day on every conceivable subject. I must confess that I was utterly unable to understand the workers' objection to payment by results until I read Mr. Sydney Webb's "The Works Manager To-day" a few years ago. One of my shop friends tells me it is already out of date, but if it is, it makes me more than ever ashamed at having to receive belated enlightenment from one who, according to his own statement, has never worked in a shop in his life.

I would like to refer, too, to the paper given by Mr. F. S. Button, A.S.E., Member of the Industrial Court, at the Manchester Convention of the Industrial League on June 16th last, on "Payment by Results." (Industrial League Journal, July, 1920, page 310.) The discussion on this paper, as well as the paper itself, is extremely instructive. I may be an exception, but I sometimes think that there are many others who would be glad to get an insight into the mind of labour. If we are to do this by direct contact, we must be willing to reciprocate in every way we can, and we must honestly endeavour to place ourselves in their position. In many cases this may be difficult, for the matter may seem to be so clear to ourselves, that we doubt that there can be any other view of it. Sometimes we read an article which on the face of it seems to be written in all sincerity and which yet seems to be based on a foundation for which there is absolutely no justification. We could in fact probably think of technical articles to which this would apply. If we agree that the writer is sincere, these views, however much we may disagree with them, represent the matter as it appears to him, and are therefore of value as showing his mind.

There is a question which naturally at once arises when considering the matters already referred to, and that is, who is to deal with this particular side of labour work? In the past Shop Committees have in their inception proved very successful as long as the heads of the firm have given them their personal attention.

In some cases they have failed because for various reasons it has been impossible for this attention to continue with a large concern. In cases even to-day we see the engineering head spending so much of his time on these matters that the saying that "we are ceasing to be engineers and becoming labour administrators" seems to be justified in some instances. In America an effort is apparently being made by the appointment of employment managers of a higher status than some who are working under that name in this country. It has been stated that they should rank at least with the works manager, and it must be self-evident that if they are to deal with all employment questions, they must hold a high position on the staff, for the questions they deal with are of vital interest and importance. They must be in a position to deal with matters directly, unless they involve important principles, or the men will realise that it is futile to discuss matters which have to go to someone else for final settlement. There is in a works of any size plenty for such an individual to do, for besides dealing with the Shop Committee he would look after employment generally, Welfare work in its widest sense, the training and education of apprentices, and would be the firm's representative at conferences of employers on labour matters, and at joint meetings between employers and labour. One difficulty that is met with at present is that of finding suitable men for such a post. It goes without saying that they should be engineers well conversant with shop work and shop life. Beyond this it is largely a question of personality and character, but having these three great and essential qualifications, they are all the better for training, and here perhaps the schools and courses of psychology, to which reference has already been made, might help. The whole subject is obviously at present in its infancy, but I cannot help referring to the work which Mr. B. Seebohm Rowntree is doing in instituting lecture courses for managers, foremen, etc., to meet and to hear and discuss various industrial labour problems.

One of the points which has a very direct bearing on the production question is that of "labour turnover." Although in automobile work there is, perhaps more than in any other large engineering industry, a similarity of the operations in various works, yet every man who is employed does not at once come on to full production. The result is that the less labour turnover there is the better. I have recently seen it stated in the American Machinist of July 24th, 1920, page 1041, English

Edition, that by studying this question the "turnover" per annum has been reduced by the White Motor Co., of Cleveland, U.S.A., from 63 per cent to 24·28 per cent, which latter figure compares with an average of 300 per cent for Cleveland generally. These figures may all seem high to us at the present time. It is true that the automobile industry is largely grouped in centres, but I think that there is no doubt that owing to the housing difficulty there is at present less movement of labour than there was before the war and than there may be again when the housing problem has been solved.

It may be a small matter, but it is often found that in a works covering a large area, many of the employees even have only a vague idea of what takes place in other shops than their own. Their wives and children are absolutely ignorant as to the place where the head of the family spends an appreciable portion of his life. Many works could arrange that at holidays their shops should be open to any of their employees and their families who would care to visit them. This was carried out lately in the largest of the works I have charge of in conjunction with the Shop Committee, and great interest was shown.

Another point on which the employment manager will prove useful is in dealing with what is broadly called "Safety First." I think many of us whose works, under section 7 (1) of the Police, Factories, etc. (Miscellaneous Provisions) Act, 1916, which came into force on 1st December, 1917, and who now have a regular ambulance room and attendant are astonished at the number of accidents which occur and require treatment each day, even if they are mostly trivial ones. These trivial cases if not treated at once may lead to serious ones, and should undoubtedly be dealt with at once, but it is naturally still better if they are prevented altogether. The "Safety First" Council is pushing the question of "Industrial Safety First," and here the co-operation and assistance of a Shop Committee is very necessary.

The subject of the education and training of apprentices has been dealt with from this Chair previously in a masterly manner, but I feel that I cannot refrain from pointing out that the new Act of 1918 imposes on us conditions which require the closest attention if they are not to interfere with the progress of our work. If we are to use labour of 15 and 16, and shortly up to 18, to the same extent as previously, it means that we shall have to increase the number of youths of those

ages by about 20 per cent, and this means re-adjustment with ages above those mentioned. The matter is, we know, receiving the attention of the education authorities and of certain groups of employers, and doubtless of individual firms.

There is at present one point with regard to education which is in certain quarters a very serious one. This is the large number of young men who are desirous of entering the industry, and for whom suitable training facilities cannot be found. This condition of things obtained in the Electrical Trade some years ago, but not, I think, to so great an extent for a well marked reason. The motor industry resembles the electrical, as it was, in that it is practically new, and as such offers considerable attractions. The idea of speed also has a fascination for so many young men, and on the top of these attractions we have the results of the war. So many youths upon becoming 18 joined up, and at ages from 18 to 23 have left the army and are looking for training; they probably know how to drive a motor bicycle or a car, and think that this will be a good start towards becoming an automobile engineer. Some had before joining up already started training, and are anxious to go back to their shop. The result of this is that there is over-crowding, and many Appointment Boards, I believe, will not consider applications for training in Automobile Engineering unless the trainee has already obtained a trainer himself. It is difficult to find a solution, for most sides of the engineering profession are in the same state. Were this not so, it might be suggested to anyone desirous of becoming an automobile engineer that little if anything is lost in getting a training in a general engineering works, and entering the automobile side later on. This was done in electrical work years ago, and oftentimes successfully. There are numerous instances of some of the greatest advances in engineering being made by men who have come in with a fresh, clear mind from some other profession or changed from one section to another of engineering work. The over-crowding is so great as to be a trouble, and the only solution would seem to be to stretch the capacity to the utmost in all cases. It may be said that we are likely to get more young men trained than can possibly find positions. We must remember the great wastage of this particular type of young man during the war, and although the competition for places will be keen, it is to be feared that it will be some time before we can ever make up our proper complement of well trained men of character, and without these the country cannot progress.

I do not propose to deal with the organisation or staff generally, but cannot pass from the question of labour without touching on the question of foremen. Many are inclined to think that with the specialisation of so many details of organisation the position of foreman is becoming gradually eliminated. I do not think this, but feel that this position was never so important and essential as it is at the present day. They are, or should be, the men who hold the shop together, and whose character and tact keep the shop as well as the wheels running smoothly, and tend to production. Living their working life in one shop as they do, they know instinctively its character and capacity if they have been chosen properly. I must say that it is oftentimes difficult to pick the right stamp of man out of the men generally, but holidays or any cause of absence of the supervisory grades affords an opportunity of trying a possible successor without unduly upsetting a man who, although perhaps a most excellent workman, does not rise to the occasion as had been hoped and who automatically returns to his old job. Knowledge is of course essential up to a certain point, but character and tact in this position, like so many others, are the great points to be associated with it. It is most necessary to have periodic meetings of the supervisory and technical staff concerned with production at which every encouragement should be given to frank and free discussion of the difficulties which crop up, and in which all hindrances to steady production are discussed. These meetings, perhaps more than any other means, give an insight also into the character of the supervisory staff.

One difficulty which I must admit I see no clear way out of is that of the under-foremen. In my preceding remarks I have dealt with means of getting in touch with the employees generally, and with the main supervisory staff. At the meetings of the latter, the head of the shop attends and deals with the matters affecting his section, but there is no room for the assistants; yet they must be made to feel that the part they play in the whole machine is appreciated. This, as I say, is a difficult matter in most shops, and can perhaps only be met by informal meetings and by acknowledgment by the high management when seen in the shops. It must however be recognised that they have their position as well as those above and below them, and that like all other parts it is one which cannot be dispensed with without the whole suffering.

Whilst on this point of supervisory staff, it may perhaps be

as well to speak of the whole of the labour in a works. It may be compared with any organisation in which all parts are essential and interdependent, and as we know it will only function properly if each portion is taking its share, fulfilling its function properly, and working sweetly. Each part must in fact be loyally carrying out its work if the production is to be what it should be, and it is on this word "loyalty" that I wish to speak briefly. We are, I think, too apt to look upon it as something which we expect to be given to us by those under us, and which we ought to give to those above. This is all right as far as it goes, but is only half of the matter. Loyalty is a virtue which works both ways, and must be displayed not only to those above, but to those below, if we are to get a return to the feeling of fellowship and pride in our concern, which is one of the greatest incentives to that production which means so much to us at the present day, not only individually, but nationally.

It is not necessary for me to go generally into the question of costing or into any of the systems, except to emphasise its importance to the management generally. I would, however, like to refer to the question of the division and classification of expenses and the dissemination of this information. This has, it is true, only an indirect bearing on the quantity of production, but a very great one on its cost. The indirect bearing is that by knowing exactly where all labour is going, i.e., what it is chargeable to, it can be seen whether the proper proportion is productive in the sense that it is being employed on the output of saleable articles. Labour and material go in shop expenses, which it is true may often be most wisely spent from a production standpoint, but which also may not be. It is very desirable that an account of these should be kept for each shop or section, and an example of how this may be laid out is given in Table I., the figures in which are, of course, fictitious and only used to illustrate the system. This particular form is drawn up especially with a view to being handed to the foreman of the particular shop to which it applies, duplicate copies being, of course, supplied to the works office, through which the copies are issued to the individual shops. The form is to a large extent self explanatory; it will be seen that the whole of the amounts incurred in workshop expenses by the particular shop and all other shops on its behalf are shown. The "details of principal items of material" is a very useful schedule, as it shows the chief articles charged against workshop expenses such as chisels, files,

Table I.

MIDLAND RAILWAY.—CHIEF MECHANICAL ENGINEER'S DEPARTMENT, DERBY.

Wages and Workshop Expenses Return. Four Weeks to August 28th, 1920. Shop No. 23.

Average No. of Staff.		Hours Paid For.					Wages.		Lodging and Travelling Expenses.	Total as per Pay Bill.	Average Wages earned per actual hour worked.	Average Wages paid for 47 hrs. at ordinary rate.	Approximate Cost of Overtime and Night duty in excess of ordinary Pay.
On Books.	At Work.	Man Hours actually worked.	Added for Overtime.	Added for Nights.	Premium.	Total.	Directly Appropriated.	Own Shop Expenses.					
		Hours.	Hours.	Hours.	Hrs.	Hours.	£ s. d.	£ s. d.	£ s. d.	£ s. d.	s. d.	s. d.	£ s. d.
170	168	31,420	245	605	9,644	41,914	2,150 14 10	687 14 6	1 0 0	2,839 9 4	1 9 7	63 8	57 11 8

Workshop Expenses.

	Supervision.	Plant and Machinery.	Tools.	General.	Total.
	£ s. d.	£ s. d.	£ s. d.	£ s. d.	£ s. d.
Salaries (Own Shop)	65 3 8	—	—	—	65 3 8
Wages ,,	183 7 11	56 1 1	9 4 7	440 2 4	688 15 11
,, (Other Shops)	9 10	420 3 7	82 5 11	41 9 0	544 8 4
Materials	7 2	105 8 11	56 2 2	98 12 0	260 10 3
Gas, Water, and Electricity	—	—	—	129 19 2	129 19
Share of Stores Expenses, Yard Costs, &c.	—	—	—	534 17 10	534 17 10
Total	249 8 7	581 13 7	147 12 8	1,245 0 4	2,223 15 2
Percentage on Directly Appropriated Wages	11·6	27·0	6·9	57·9	103·4

Details of Principal Items of Material.

Article.	Qty.	Amount. £	s.	d.
Steel.				
Files	59	5	12	11
Saw Blades	72		13	6
Cart Wheels	2	4	5	11
Spanners	2		5	6
Chisels	150	17	8	0
Giant Drill	1	42	0	0
Twist Drills	149	129	15	2
Taps	139	61	2	1
Ball Bearings	—	9	13	5

Article.	Qty.	Amount. £	s.	d.
Wrot. Iron.				
Nuts			9	8
Bolts	—		19	3
Locks	1		2	9
Padlocks	2		2	4
Hinges	16	1	6	0
Screws	—		19	11

Article.	Qty.	Amount. £	s.	d.
Electrical.				
Motor Parts	—	8	5	8
Switches	2		4	5
Cable	—		10	10
Carbon Brushes	12		10	9
Lamps	16	3	6	6
Conduits	—		10	1
Insulating Material.	—	3	5	4

Article.	Qty.	Amount. £	s.	d.
Miscellaneous.				
Emery Cloth	—	5	19	11
Oil	—	10	3	3
Gas Fittings	—	9	4	0
Emery Grinding Wheels.	3	8	12	8
Belting	—	41	5	4
Belt Laces	292	5	16	8
Sponge Cloths	—	33	12	0
File Handles	—		11	0
Hammer Shafts	30		9	6
Yard Brushes	—	1	8	0
Coal	—	153	1	8
Leather Gloves.	-		3	9
Manilla Rope	—	16	18	8

sponge cloths, etc. This information it is very desirable that a foreman should have, as it allows him to see at once what is happening in his shop. Incidentally such a sheet when first issued is often an education in both quantities and values. It also shows the actual cost which extra wages, overtime and night work involve, and its relation to the cost of ordinary day work. As month follows month a comparison can very conveniently be made between the various items, while a summary sheet can be compiled for the higher management.

The question of design in relation to production is of too great a magnitude to be dealt with fully in an Address like this, and yet it must be mentioned, for it undoubtedly is one of the greatest importance. There is now perhaps in these days, when output means so much, greater co-operation between drawing office and shop than has always been the case in the past, and very rightly so too. That it is not always considered as it should be was brought home to me very fully during the war, as it doubtless was to so many of you. I may perhaps be allowed to say that I am not only thinking of aeronautical work, where more than in any instance lightness is most desirable, and where it helped to give a much-sought-after superiority over the enemy's products. It was not only in this country or in the countries of its allies that the difficulties I speak of arose, as will be appreciated by any who visit the War Museum and study the munitions of war put out by Germany. In other directions, besides that of aeroplanes, there was not I think in the early days of the war the close co-operation that was essential, and I feel that one of the reasons which led to the huge production of repetition articles was the conferences between those responsible for design and the members of the Ministry of Munitions who dealt with production, and between the latter and the manufacturers themselves. The matter is not always a simple one, for I well remember a recognised leader of engine design in the United States suggesting to me that he should re-design a piece of engine mechanism to get over a production difficulty, who, after a few minutes' discussion, admitted that it would entail a complete re-design of the engine itself. The whole matter is one very similar to the question which often arises as to whether a bolt is to be made out of the bar or from a forging, i.e., it must be discussed on its own merits, and with the full facts it can usually be readily decided which is the most profitable way of dealing with the

matter. There must in any case be machinery for dealing with such situations, which after all are bound to arise particularly when the staff are keen, although this may at first sound absurd. If, however, a man, be he designer or foreman, is enthusiastic he is likely to look at things chiefly, if not wholly, from his own standpoint, and to forget the "other fellow" in his desire to have his own point dealt with in the best possible manner. The designer wants the part to fulfil its functional duties in the best possible way, whilst the foreman, not primarily concerned in this, is anxious that the time on the job shall be low and the output as large as his machinery will allow. The designer in his eagerness does not worry too much about the available machinery, and the thing to do seems to be to bring the two together as early in the design as possible.

There is in connection with this matter, and it may perhaps be dealt with here, the question of special or single purpose machines. In this country in the past our machinery has undoubtedly been of a general type, due probably to the fact that the amount of repetition work has not been large enough to encourage the design of special machines, and in addition to this in most cases designs have changed before any really large quantities of an article have been made. When in the United States in 1918, one of my objects was to find a factory where, without a considerable outlay on machinery, a British aeronautical engine could be put on production early. This could have been done in practically any British motor car works without difficulty, but I soon realised that it was impracticable in nearly all of the large automobile shops in the States. Their machines were not "general" in any sense, but highly specialised for the particular work they were to undertake, the design of which was settled with a view to a large number being made without change. The Liberty engine, in fact, was chiefly made in shops either entirely new or in sections of shops in which special new machinery had been provided. This system of manufacture gives very much greater freedom of design, but must have with it confidence in the design and of the possibility of selling sufficient cars to justify the outlay on new and special machines. This latter should be helped by the knowledge that such special machines will reduce the labour costs, and should allow of the products being sold at a much lower price than if manufactured in what may be called a general shop.

In design not only has the design itself to be considered, but also the material to be employed, and here the Institution is, I think, to be heartily congratulated on the work it has done, due largely to the energy and initiation of a few of its members. This is particularly the case with regard to steel, which is, of course, the chief material to be handled. It is to be hoped that to complete this side of our work we shall before long deal with the few non-ferrous metals which are used in a car. At first sight this may seem to be almost unnecessary, but not only would it round off a very great work, but might be found useful even from a production standpoint. In the early stages of the war, certain parts of transport vehicles were of bronze to a certain tensile specification and approximate composition. It was found in one shop that the specification could be met, but that the parts were so difficult to machine that with the existing machines only about half of the promised output could be looked for. The matter was put into the hands of the metallurgist, who in a day or two was able, without dropping the specification, to get the material into such a state that the machining time was halved and the promised output easily obtained. The British Engineering Standards Association have recently formed a Committee on non-ferrous alloys, and it would perhaps be advisable to deal with the matter in conjunction with them.

The progress of the work in an automobile shop is, owing to its regularity, perhaps easier to trace than in a general engineering works. It is none the less necessary to see that the department dealing with this is alive in every way. There are many systems of routing or following progress, and all of them, if they achieve their object, are good. They have a certain family likeness, and the peculiarities among them are generally the result of the varied physical features of the shops. By a little thought a system can be worked out applicable to the conditions to be dealt with. Here, however, as elsewhere, every effort should be made to reduce the actual writing required by shopmen to the lowest possible point. It is too often forgotten that writing is not the chief object we employ either a foreman or a craftsman for. The burdening of the shops with another form should be looked upon as a very serious matter, and one not to be embarked upon without having been very maturely considered.

The ordering and supply of materials has a marked bearing on production, and has been recently one of the most serious problems

which an establishment has had to face. Under normal conditions with an accepted programme of output and with the detailed design well in hand, the matter is not a difficult one. With the uncertain delivery, which has been so common lately, and with the great fluctuation in values, very different conditions have arisen, for nothing is more tantalising than to find an article held up because of a shortage of material to make one small part. It is to be hoped that more stable conditions may before long again be reached.

The question of jig and tool design has been dealt with elsewhere quite recently. Nowhere perhaps is the provision of a

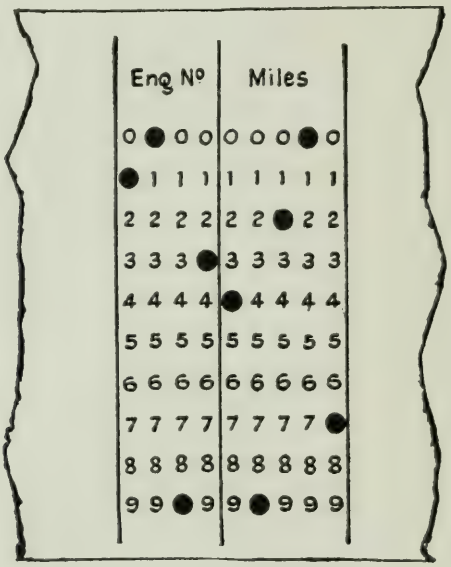


Fig. 1.

well organised department to deal with these more strongly felt than in a motor car works, and probably nowhere does the matter receive more attention.

I have already spoken of costing, and I think it may be of interest to speak, too, of a method of recording costs which allows of these costs and many others being obtained readily. The system is an adaptation of the method employed in taking the U.S.A. census some years ago, and for a considerable time has been used by various railway companies for obtaining statistics relating to locomotive mileage, consumption of coal, oil, etc. The whole

principle of the system is the use of definitely placed holes in lieu of figures, and in the additions of the figures mechanically. Naturally, arrangements have to be made for differentiating be-

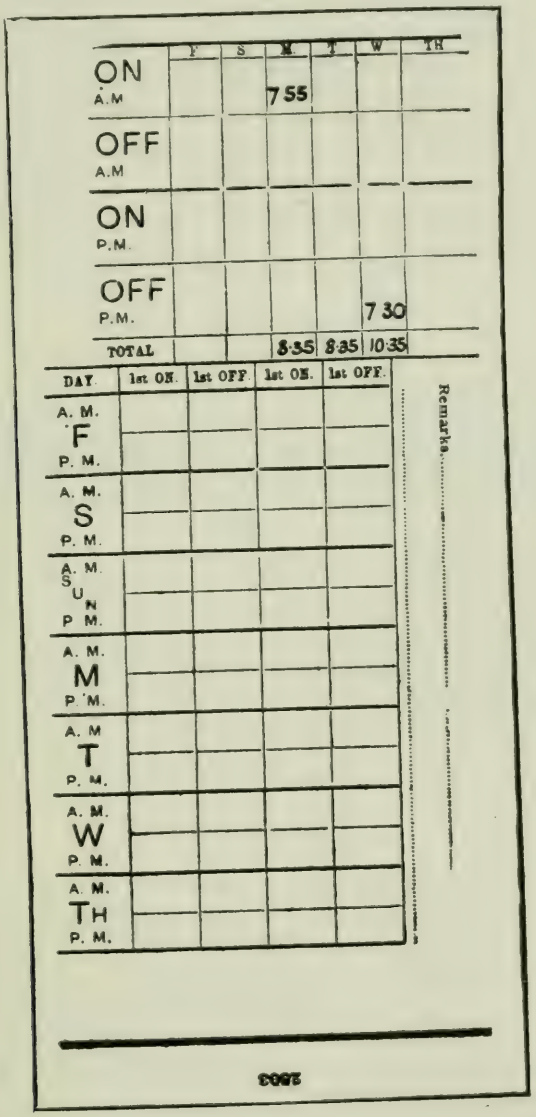


Fig. 2.

tween the various digits. This is done for any number by having 10 positions vertically in which the hole representing it may be placed. If the number be 1509, the hole representing the first digit is punched in the top position but one, the top position being

reserved for the cypher. The second digit is punched in the sixth position down, the third in the top and the fourth in the bottom position. The cards employed are about $7\frac{1}{2}$ in. by $3\frac{1}{4}$ in. Fig. 1 shows a card which would be used if it were desired to show the mileage of a vehicle only. The whole ten lines of positions are shown, and it will be seen that the Engine No. columns are punched so as to show its number as being 1093, whilst the columns relating to mileage show this to be 49207. The holes themselves are punched in a separate small machine, and in one system gang punching can be resorted to where this is advantageous. The cards having been punched, a sorting machine will sort out the cards so that all those relating to any particular

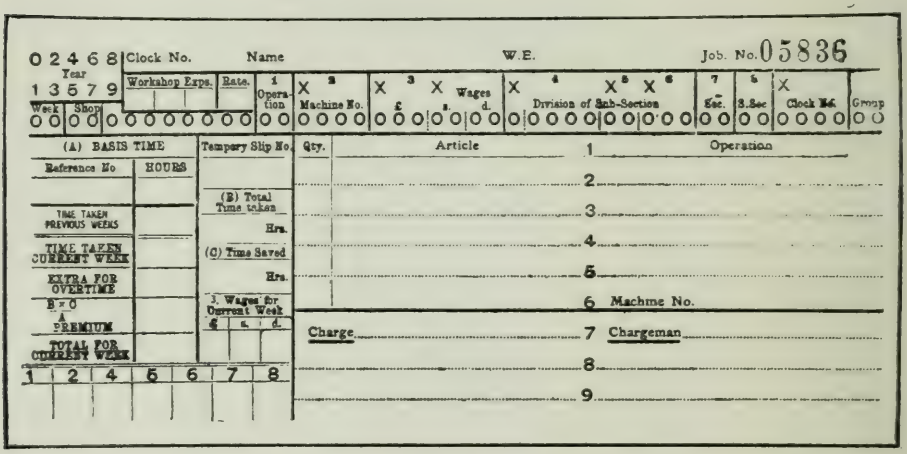


Fig. 3.

engine are collected together. These are then placed in another machine which will add the mileages of each engine together, and if necessary print each one as it passes through the machine on a roll and add up each set of cards giving the total in a separate column, all that is necessary being the insertion of a special blank card between the set of cards it is desired to total.

This, shortly, is the method which can readily be applied to dealing with shop "job tickets." It is an advantage to utilise the card as the job and time ticket itself, as the whole of the information can then be embodied on one card. To do this, a card of the size already mentioned can be employed and will suit practically any type of time clock. A typical example will perhaps help to explain what I mean. One side of the card is used for

"clocking" purposes only, and Fig. 2 shows one applicable to two types of clock, and will in fact deal with most of those in general use. In this case the top portion is used, and the time when the man "clocked on" and "clocked off" this particular job is shown. It is desirable to have the clock fitted with a bi-coloured ribbon so that by a switch in the main office all the clocks may, when desired, be made to stamp the time in red figures in place of the ordinary blue. This switching over should be made at a time after which time worked receives special allowance, as it calls attention in the office to the fact that something out of the ordinary has to be allowed for.

The reverse of the card is shown in Fig. 3, and is the job ticket proper. The series of numbers designating the position for the

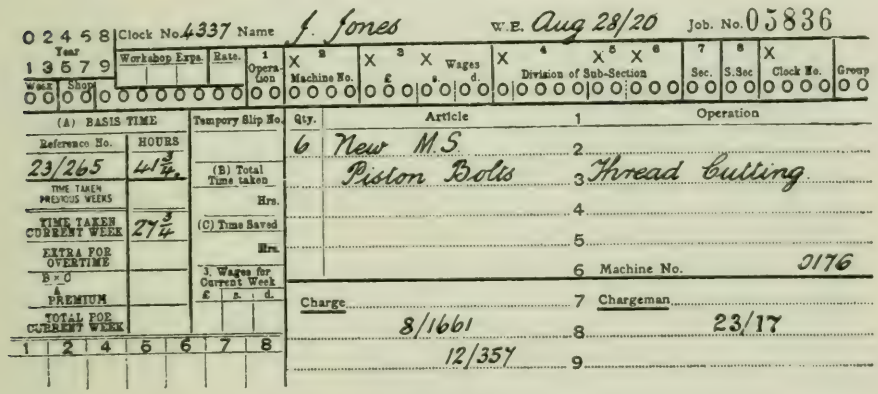


Fig. 4.

digits in the various columns are not shown right across the card as in Fig. 1, but their position can be judged by the lines and the vertical row of numbers a little to the right hand of the centre of the card, and the top line is fixed by the row of cyphers across the card. The particular card shown is one applicable to the premium bonus systems, as will be seen from the printing on the left hand of the card, but this is only a matter of detail. The card, when issued, is not intended to last longer than the end of the job or of the working week, whichever of these is the shorter. The cards are filled in in the Central Order Office or in the Shop Office, and given to the charge hand for distribution to his men as they require work. The workman upon starting a job goes to a clock and "clocks on," this being registered on the card as shown in Fig. 2. The "job ticket" side is shown as

handed to the man by Fig. 4, and it will be seen that it has on it particulars of the work to be carried out, the number of the man and the necessary information for charging the labour expended on the job. Upon completion of the work the man "clocks off" and "clocks on" to his new job; the completed card is collected and forwarded to the Accounts Office. Here the record of the job is completed and the various items filled in, this being shown in Fig. 5. It will be noted that directly under the top long line are a series of small numbers running up to 8, and it will also be seen that each one refers to a group of columns of figures beneath them. The information which should go in the columns and which is not already shown in the card is entered in the office in the corresponding short columns in the left hand

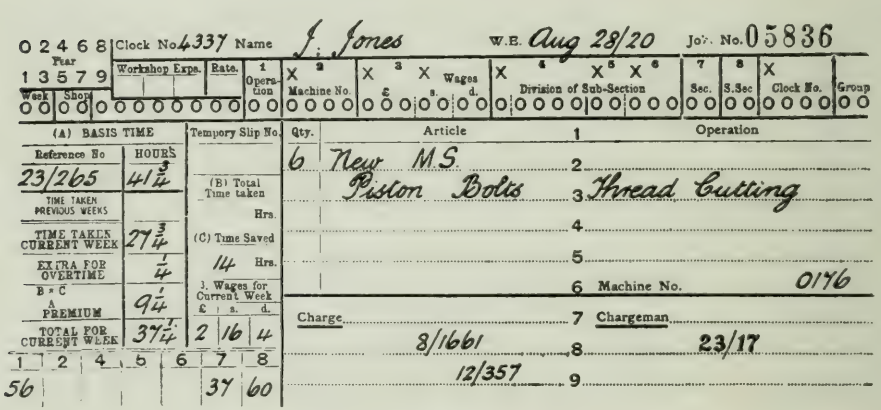


Fig. 5.

bottom corner of the card. This being done and the other particulars having been filled in, everything is now ready for what may be called the mechanical side of accounting. I would, however, call attention to the fact that the card or "job ticket" now has on it all that can possibly be required to be known about the job, and that it is the actual card on which the man clocked the time he has been on the job. The card is now ready for punching, and whilst being punched moves ordinarily one space from left to right; if, however, there is nothing to be inserted in a particular group of columns, a hole is punched in the x which lies on the left of the group directly under the top longitudinal line. Punching this hole causes the card to move forward automatically to the beginning of the next group of columns. The

job ticket fully punched is shown by Fig. 6. In the top left hand corner is punched the year, which is shown by its last digit (0), the week in the year (in this case the 26th), and the number of the shop (23) in which the job has been carried out. In the first of the numbered columns which lies just to the left of the heavy vertical line is shown the operation number (56), and just to the right of this line is the machine number (0176) on which the work was carried out. Now comes in column (3) the wages (£2 16*s.* 4*d.*) to be paid on the job, and this is followed by the Order No. (8 1661) to which this is to be charged, and the section (37) and sub-section (60). Under the clock number is punched 4337, giving the identity of the man, whilst the last column gives the group (17) that he is associated with. The card is now complete,

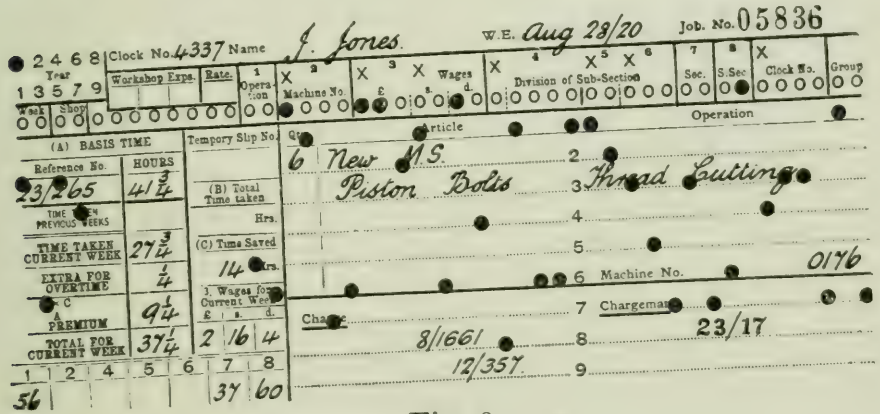


Fig. 6.

and can be sorted out so that all the cards of this man (No. 4337) can be collected together and added mechanically for the pay bill. Later they are re-sorted so that all the charges to the Order No. may be added and charged up, whilst if it is desired the money spent in that particular week in that particular shop by the particular group may be obtained readily. The amount earned by the machine or by the group of men may also be ascertained.

I have given this somewhat in detail as I can imagine that some may think that the showing of detail of costs is an expensive matter, and this will explain that much of it is obtained very readily. Other machines may be utilised for ascertaining quickly the hours shown on Table I.

The tabulating machines, the work of which has just been described, are manufactured by two firms, who hire them out and

keep them in repair. The description does not deal with either in its entirety, but is intended to show how they may be applied.

I am afraid this address has been of a somewhat rambling nature, and I realise that there are very many matters of great importance which I have either only mentioned very casually or have omitted altogether. Most of them are, as are many of those dealt with, of sufficient importance for a whole address, but I have endeavoured to touch on those which are perhaps not so universally known. Might I make the suggestion that there are many points which concern production, such as progress work and routing, jig and tool work, costing, speed and feed work, etc., which would be admirable subjects for some senior members to give addresses on to the Graduate Sections with great benefit to our younger members, who are the hope of the industry.

Colonel R. E. Crompton: I am afraid "Anno Domini" must be my name, for I happen to be the oldest of the seven Past Presidents assembled here to hear what our new President has had to say. It is a difficult task for me to do adequate justice to the Address, but it is not a difficult task to do justice to the man. The man is a man of character. He asked for a definition of character, and I should say a man of character is a man who holds strong clear views, and is not afraid of telling other people what those views are, and that is our President. Again, I am an old man; I cannot live very many years, but I want to live happily during those years, and I have come to the conclusion that there is only one way of doing it, and that is the way laid down very clearly in this Address. Do everything that you can for the younger men. You will never regret it. Everything that I have done in the past for the young men of my profession has come back to me many fold, and has made the later years of my life happy years, whereas we see so many people at my age doing nothing but grouse and sit and hope for the termination of their sufferings. I want to live on and see what the younger ones are going to do, and how far they have justified what I have tried to do for them; so far, I may say, in every case it has been satisfactory.

Turning to the Address, I feel that it is a carefully thought-out one on the question of the day. Who is going to get at the mind of the working man of the present day? Which working man's mind is it that we are to get at? Is it that of the trained careful married workman who has settled down for life, or is it

that of the youth of twenty who lost the control by his parents owing to war conditions, who bullied his mother, who drank and smoked and did every evil thing possible for the last six years and is now the principal voting agent in these rows? Is that the working man's mind that we have to diagnose? Well, I think that it is a by-product—it is a by-product of the war, and one of the worst by-products that were produced by the war. I agree wholly with the President, that we have to try and find out a suitable method of getting into touch and making friends with those we work with in the shops. Whether we can ever do it on the gigantic shop scale of the present time or not I do not know, but I do think that the ideal shop of the future will employ very much fewer hands but more machinery, will have a sufficiently large turn-out for a reduced percentage of labour, and will consequently enable the shop manager, whose business it is, to understand the feelings of the employees, as he will have a reasonable number of men to deal with. During my thirty years as manager at Crompton's I came to the conclusion that no man could know more than 500 men, that is, to recognise their faces and know what they were worth and what they were thinking of and whether things were going on smoothly. To me it is unthinkable that these huge concerns at the present time, with the number of men that come in from other shops, can be managed on any principle of knowing the feelings of the men. I think that really we have to devise some system of making friends with our men outside the shops. I have always thought the ideal thing would be smaller towns, each town having one or two smaller factories, everyone living as neighbours in one district, the employers, the foremen and the men, having their houses and their gardens near to one another, all interested in everything that their neighbours are doing. That feeling cannot exist when the men are herded together in a huge machine shop, and their living or houses being all alike, as dreary and commonplace as those in Battersea. At present no employer can know what his employees are like either at their work or in their homes.

I have taken the opportunity of making a political speech, but I think it will be agreed that, although we are not allowed to discuss a President's Address, we are allowed to touch upon those points which appear to accord with our own opinions, and which we thoroughly believe in ourselves. I do believe that the President has, in his Address, given us food for thought, and we must

congratulate ourselves on the great union that is to-day taking place, the great marriage between the rail and the road. This is the first time that we have had a President from the railway side. I have always considered the railway people to be nice, kindly, good old-fashioned, slow-going people, whose ideas were not to be followed by any means, particularly when they use mild steel for piston bolts!* I think, gentlemen, you really do feel that I have got at the mind of your President, and I hope you will see that he is really a man of character, and that he will rule you in the way you deserve to be ruled, to your advantage, during his forthcoming Presidential year, and I move the heartiest vote of thanks to him.

Sir Henry Fowler: I thank you very much indeed for the cordial way in which you have received what I know is a somewhat scrappy Address. With regard to Colonel Crompton's point of the piston bolt, I do not know what a piston bolt is in locomotives, as I only used the phrase in order to illustrate my point!

I do appreciate very much the opportunity of speaking to you, and I appreciate also very much indeed the kind way in which you have received the Address.

The first official duty I have to perform is one of the most pleasant things, and that is to propose a hearty vote of thanks, the heartiest vote of thanks we can give, to Mr. Thomas Clarkson for his conduct in the Chair during the year in which he has been President. I feel I am competent to do this for a good many reasons. As he has reminded you, I followed him in automobilism! I think it is now some six years ago that I followed him as one directly in front of me, to receive a medal for a paper he read before the Institution of Civil Engineers. I was very proud then, and I shall still be more proud if I could think that at the end of the coming year I had followed him worthily as President of this Institution. I am speaking so far entirely for myself, but at the same time you will, I am sure, all appreciate what we owe to Mr. Clarkson for the manner in which he has carried out the business of the Institution during the past year. Especially was this the case with regard to the Summer Meeting during his year of office. I think we had one of the most successful Summer Meetings ever held by an Institution, and the genial way in which

* See Fig. 4.

the President carried matters through tended to make the visit so delightful as it proved to be. We know, too, how good the discussions have been during his year of office, and you will appreciate this when you get the volume of the proceedings, which is sufficient evidence of the energy and the capacity with which the President carried out his duties. It is for these reasons I want you to show in the heartiest manner you can by acclamation what you really feel with regard to Mr. Clarkson.

Mr. T. Clarkson: I can only say that it has been a privilege to occupy the Presidential Chair during the past year, particularly as I have been so well supported by every Member of the Council and by every Member of the Institution. I feel that the Institution of Automobile Engineers is really one of the most important of the engineering bodies of this country, and my small efforts have been directed mainly to endeavouring to secure proper recognition from the powers that be, and particularly from the financial powers that be, because the money activities of the Institution are necessarily very restricted. As I mentioned at the outset this evening, I feel particularly happy in retiring from the Presidential Chair knowing that Sir Henry Fowler has taken my place, because I am sure that the Institution could not be piloted by a better man than by Sir Henry Fowler, who has by sheer merit and ability won for himself a very high place in British engineering, and he occupies the position, as you know, of Chief Mechanical Engineer to the Midland Railway Company. I think it was a very good augury, as Col. Crompton said, that there should be this close alliance between the railway man and the automobile engineer. I thank you most heartily for the manner in which you have responded to this vote.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

OCTOBER, 1920.

The Second Ordinary General Meeting of the Session was held at the Chamber of Commerce, New Street, Birmingham, on Thursday, October 28th, 1920, at 7.30 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"The Springing of Motor Cycles," by G. H. SAVAGE.

There were present 93 Members and visitors.

THE SPRINGING OF MOTOR CYCLES.

By G. H. SAVAGE.

A GREAT part of the author's experience with motor cycles was while he was with the Mechanical Transport in France, an experience which was perhaps unique, as it was the first time that the springing of motor cycles had been tested on a large scale, and he had some exceptional opportunities of seeing the springing at work and under repair, both up the line and at the base.

At different periods of his work up the line, he had attached to him for repair the machines of certain of the Signal Companies, Motor Machine Gun Batteries and R.E. Field Companies, all these being units whose machines had the maximum amount of hard work. Later on in 1917, he was sent down to the 3rd Heavy Repair Shop, where he took over the motor cycle repairing section, the work consisting of thorough overhauls of the cycles that were sent down from the line as complete casualties. The 2nd Heavy Repair Shop under Col. T. B. Browne (then Works Manager), and the 3rd Heavy Repair Shop, with Major B. H. Thomas as Works Manager, had to endeavour to make their combined output and the casualties balance. There was always a little friendly rivalry between the two Shops, and at the end the combined output of the 2nd and 3rd Heavy Repair Shops was, as far as the author can remember, over 100 solo and sidecar machines per week for re-issue.

He hopes that these experiences and deductions which are now put forward will be of some interest, and will help in solving, to some extent, the problem of motor cycle springing. He also desires to say that he is in no way interested in the motor cycle industry, and is not in consequence prejudiced in favour of any

particular design. The paper is in fact an account of some of the experiences of the Mechanical Transport, Motor Cycle Section, in France, with his own deductions therefrom.

That the problem of the springing of motor cycles is an important one there can be no doubt, and the author would go so far as to say that it is one of the most important on a motor cycle, as the severest strains that a machine has to withstand are from the road shocks. If the road shocks can be lessened, and they can be very considerably with efficient springing, the design of the motor cycle should undergo some radical changes. Designers in the past have been working on the problem of building a very nearly unsprung machine for the average rider, to withstand the road shocks when ridden at speeds up to 40 or 50 miles per hour. It is difficult to understand why they have not long ago developed front and rear springing for motor cycles, as it very much simplifies their work.

Very often it is remarked that some part of a machine has almost attained perfection, and it is difficult to see how it can be developed further. It cannot be said that the motor cycle has arrived at this stage yet, but efficient springing certainly opens up a vast field of possibilities which has hardly been touched on at present. Apart, too, from that side of the question, which only affects the manufacturer and designer, the range of the motor cycle would be in every sense much widened by good springing. At present it is no unusual thing to feel fatigued after riding, say, 60 miles over medium roads, which is in a great measure due to the shaking that the rider receives. With well sprung machines, the halt and the lame would soon be riding motor cycles, while the range would be considerably increased for the hale and hearty.

Besides the fact that the war period in France was the first time that the springing on motor cycles had been tested on a large scale, it was the first time that sprung machines had been tried against unsprung machines on exactly the same work. It is true that the springing had been tested in England, but the machines had never been subjected to the prolonged tests that they had to undergo in France, such as, in extreme cases, being ridden over corduroy roads, roads with every sort of obstacle and pothole that can be imagined, all ridden at the highest possible speed, for day after day, by riders whose instructions were to get to their destination as quickly as possible. In the more normal cases, they were ridden over roads with surfaces which in England

would be called bad indeed, but which in France were only regarded as average; ridden over these roads very often in heavy rain, with the holes filled with water, so that if a rider wished to pick his road it was next to impossible for him to do so, being ridden at night, too, very often without lights.

This, then, was the work that the average machine had to stand up to, and, granted, it was a heavy one. It was the most valuable of tests, "a test to destruction" spread over varying periods of time, thus enabling the stages of wear and fracture to be noted in sequence as far as time and war permitted. It was also a test conducted on an enormous scale, which made it more possible to be certain in the deductions made, the errors in manufacture and occasional batches of faulty material being only a small percentage of the whole.

It would, perhaps, be as well to give some of the adverse conditions worked under besides the question of road surfaces. The lack of facilities for cleaning the machines up the line, of rain, mud and dust were only to be expected. In some areas at one time chalk was the only material at hand for filling up holes and generally repairing the roads, and in the hot weather it used to lie inches thick for miles, and chalk is not a good lubricant. It impressed itself especially on the author's mind, as he was unfortunate enough to work for some time in one of these areas.

Another difficulty was due to the absence of any facilities for getting the repairs done at once. This depended to a great extent on the efficiency of the workshop the machines were attached to for repair, and in the author's experience the mobile workshops up the line had very often more work than they could cope with, and they could not, therefore, do all they wished in this respect. In the case of small repairs, it depended on the unit the machine belonged to and on the individual rider, as they undertook these themselves. It meant, in consequence, that sometimes machines were ridden somewhat longer than they should have been in an indifferent state of repair, and these difficulties especially affected the mechanical details of the springing. As the war progressed, however, they considerably improved. The quality of the riders themselves was for the most part good; they varied of course, but indifferent riders were usually kept up to scratch by their officers and non-commissioned officers, and by inspections of the machines when possible.

We now come to an important question, and that is—"Is

springing necessary as shown by experiences in France, or merely an advantage?"

That springing is an advantage will probably not be disputed; that it is a necessity, if the motor cycle is to develop the author will endeavour to show. It should be pointed out that in order to realise the value of springing, the work got out of a well sprung and a badly sprung car should be compared, also the respective costs of upkeep.

The success or failure of springing on a cycle must not be judged only by the number of broken springs, but by how the various parts which take the road shock (wheels, head lugs, frame, etc.) stand up also. The author will therefore deal with some of the mechanical failures experienced with parts other than the spring details which in his opinion were due to bad springing, and which anyway would have been greatly lessened if the springing had been better. It would be unreasonable to say that these parts should have stood up to the work without failure, as it is palpable that the machines were not designed to stand up to some of the severe tests to which they were submitted; they should, however, have stood up better than they did, although, again, war material and workmanship probably had a hand in the failures.

In a number of cases, of course, accidents and collisions, etc. were the causes of the parts breaking, but the parts mentioned below are those that failed in the vast majority of cases under more or less average riding tests. A very considerable amount of trouble was experienced with the head lugs cracking and ultimately breaking, if not changed at once; they cracked in different places on different makes of machine, and were obviously not strong enough to withstand the heavy strains put upon them. The riders of the machines were encouraged to examine the head lugs every day for cracks, and when these were not seen in time the break usually took place when the machine was travelling at a fair speed and much damage was done to the motor cycle or combination as a whole, by badly straining the frame, etc. There were, of course, cases where the failure was due to faulty work, by munition workers possibly, such as the tube holes in the head lug casting not having been bored concentric with the outside, etc.

Owing to repeated representations, one maker strengthened the head lugs on his machine by putting on extra webs, and they stood up much better; another maker, the author believes, made

his head lugs of steel in place of the usual malleable iron casting. It was disheartening work, putting in new head lugs, knowing that they would come back again broken at probably no very distant date.

It may here be asked, was it known how much running a machine had done between its overhaul and issue by the 3rd Heavy Repair Shop and its return as a casualty? The system at the 3rd Heavy Repair Shop was to number all machines when re-issued, and these numbers were looked for on all casualties coming in, so that it could be ascertained how long the machine had been out; how much running it had done could only be told approximately by looking over the machine and judging from the wear of the units. This was the procedure at a base repair shop; up the line the machines were probably seen every day. Frames were a constant source of trouble through the tubes breaking, or the frames themselves distorting and twisting; the front down tube was the weakest place, and it usually broke just below the head lug. It is possible that the heating in the process of brazing would, by annealing the tube, destroy some of the extra tensile strength of the solid drawn tube, but at the same time it was without doubt too weak. It usually broke when the machine was travelling at a fair speed, and the result was that both the top tube over the petrol tank and the middle rail bent, necessitating in most cases a replacement; any amount of damage to the machine was of course possible; the above was what usually happened.

Makers later on made these front down tubes of a heavier gauge, and they stood up to their work much better. In the author's experience, if the brazing had been well done it never broke; he remembers one consignment, though, of new machines which came to the 3rd Heavy Repair Shop. A number of these were issued, and they all came back in a few days' time with the front down tubes either broken across the pinholes, or pulled out, and it was found on examination that they had been very badly brazed up in the first place, and in consequence the whole consignment had to be rebrazed. The frames used to break in other places, but the head lugs and the front down tubes were the worst troubles. The back forks used to twist rather badly, but this cannot be put down as due primarily to bad springing.

Very important parts on the motor cycle are, of course, the front fork stem and the top and bottom bearings in the head, as

they jointly control the steering to a very great extent. The front fork stems broke very seldom in the author's experience, and stood up comparatively well. It is regretted that the same cannot be said for the ball races in the head; these were very poor, and after every 200 or 300 miles riding play developed in the head and the races had to be adjusted. When this had been done several times, it would be found that all the side play could not be taken up and new races were necessary.

Alternatively, the races cracked or pitted and balls broke or chipped, and there was also to all intents and purposes no arrangement for lubricating and greasing them as they required. Very probably this play in the head race contributed in a large measure towards the breaking of the head lugs.

At the end of this last Auto Cycle Union trial, the author had the privilege of looking over a considerable number of the machines. It was interesting but regrettable to note that a great number of them had play in the head, showing that there is still very much to be desired in the design of these bearings.

In the earlier days of the war, a great many front forks broke, usually just below the point where the first cross bar is brazed in; as time went on, the section was made larger and heavier, which was a big improvement. It might be argued that the front forks hardly come into the discussion, as on the vast majority of spring frames they are and must be still unsprung. This would not be correct, however, as on an unsprung or badly sprung machine, the shocks that the front and rear forks receive and transmit are of necessity very much greater.

Repairing wheels and scrapping them kept a small army of men busy; here again a great many were damaged through collisions, but their number was small compared with those that had failed through average running. Practically every rim that came in was badly dented, the spokes were loose and often broken, which of course did not help the rim to stand up to its work, but both were at fault, as the spokes were too light a gauge. Later on they were made heavier by some makers.

The wheel spindle bearings had to be replaced very often, and here, to be fair, it should be realised that the rain, mud and dust had a hand in shortening their lives; it must be admitted, however, that the bearings were not of sufficiently ample dimensions to withstand the vibration for long; on the sidecar machines

they stood up better, as probably these machines were not ridden so fast.

Mudguards had to be replaced very often, as they either broke, or cracked and tore away at the point where they were attached to the frame and stays; the method of attachment was usually very poor, and does not seem to have received the attention it deserves at the hands of the designer. The mudguard should be thickened up by an extra plate inside, round the set screw that attaches it to the frame. In the author's opinion the use of the back stand as a stay on the rear does not help matters.

Handle bars proved weak at times and broke, but they were good on the whole, and it is not clear that bad springing was responsible for these failures. The breakages of carriers caused a lot of trouble. Here it may be suggested that they were overloaded; there were cases, of course, when this was so, but they were exceptional, and on the new machines coming out from England, boxes which were fitted on, in addition to the carrier bags, were practically always taken off. The carriers broke everywhere, and representations were made to the makers, and later Messrs. Douglas on their 4 h.p. sidecar combination fitted a re-designed and much stronger carrier, which gave very little trouble. On some of the overhauled machines from the 3rd Heavy Repair Shop, and probably from the 2nd Heavy Repair Shop too, the carriers were strengthened by brazing on extra stays, and these proved satisfactory, but owing to pressure of work it was not possible to do this on a large scale. It was difficult to effect a satisfactory and economical repair on a broken carrier, and in consequence a great many had to be scrapped.

The carrier bags gave a lot of trouble, and the large replacement demands which were sent in gave rise to several orders from the Director of Transport urging extra care. Even with extra care though, short of not using the bags, they fell to pieces almost at once. The clip arrangement for attaching the bags to the carrier was poor, and they tore away through the excessive vibration on the rear wheel; the bags themselves, too, were not strong enough and came to pieces, although the rain must have helped considerably to deteriorate the leather. At one time some enterprising English manufacturer must have heard how the Germans were making their sandbags of paper, and he evidently decided to try cardboard for carrier bags; the author expects that he is now a disappointed man, complaining of the narrow-mindedness

of people at the head of affairs, and how they refuse to encourage "initiative." The spare parts carried in these tool bags suffered considerably unless packed in very tightly; if packed in at all loosely, the bags simply acted as tumblers. The design of these carrier bags was considerably improved towards the end of the war; they were still made of leather, but were enclosed in a sheet metal casing on all sides except the front. Messrs. Douglas further improved the bags by letting them into the frame of the carrier, thus supporting them by tubes on the four sides, the bottom tube carrying the weight.

That bolts and nuts, etc. work loose if subjected to vibration is well known, and the bolts and nuts on the machines in France were no exception to this rule. It was only in the case of exceptional riders that they were tightened up at once; usually they were not noticed for some time, during which period parts worked that were intended to be bolted up tight, and holes were enlarged, etc., thus causing extra expense when the machine eventually came in for overhaul.

In France, all the motor cycles and sidecar combinations were fitted with acetylene lighting only. The effect of vibration and road shock on the lighting qualities was that if the lamp burner was at all furred up or the carbide was getting low, the road shocks would jar the light out; if, on the other hand, the generator was charged and working well, practically no trouble at all was experienced. In regard to the mechanical details, the nuts on the attachment used to work loose, and the lamp would swing over; even with the nuts dead tight in some cases the lamp would not keep straight owing to the frictional surface of the clamp being insufficient. On the lighting equipment as issued at the commencement of the war and for some time after, the lamp was fitted on the handlebars and the generator on the front down tube, the two being connected by a rubber tube. This meant that the generator was out of sight when the machine was being ridden, and there were countless cases where the generator was jarred loose, and finally jumped off its bracket and was lost.

Later on in the war, a much better bracket was designed, the lamp fitting on one end and the generator on the other, the bracket being bolted to the handle bars in the centre, so that the lamp and generator more or less balanced each other, thus taking a lot of strain off the clamps; another advantage was that the lamp and generator were directly in the rider's view.

One very important side of the question of bad springing has not been touched on yet, and that was its effect on the personnel. Riding the standard machines in France, over the bad roads prevalent there, was very tiring for two main reasons: firstly, because the rider was being shaken the whole time, and, secondly, because of the continual vigilance and concentration necessary to keep the machine on the road, and to counteract the effect of bad pot holes, etc. on the steering. The author contends that if the machines had been fitted with spring frames during the war it would have made a very great saving in personnel by increasing the despatch rider's range and by reducing sickness. On the standard machines in France, front fork springing only was fitted, and helical springs were used throughout to take the road shocks. Some were wound slightly conical or volute, in which case their action was more or less progressive, while on other machines the springs were parallel wound, acting either in compression or extension.

After having ridden practically all types of machines in France over all sorts of conditions of roads, the author's criticism is that the springs were not responsive at all to the small and average shock, though they came into action for hard shocks, and for the very severe shock the springs either came up solid, when acting in compression, and the frame took the final blow, or, in the case of the extension type, the springs were overstressed; if a stop was fitted on this latter type, the frame again took the final blow.

The allowable movement from the normal, under load, was on the average from 1 in. to $1\frac{1}{2}$ in. This seems to be the crux of the argument, as in the author's opinion it is impossible to design a spring with this allowable movement to be responsive to the range of shocks which it is known from experience it will be called upon to include. In consequence of the springs being too stiff, parts broke and shook loose, etc., as has been stated earlier in the paper, and the springs themselves broke frequently.

It is difficult to say why the Triumph springs broke, though probably it was partly due to their being overstressed and partly to the rebound. Earlier in the war the Triumph spring was made of rectangular section, but later this was changed to one with very round corners, the section being more or less elliptical, and there were fewer breakages with this later type. With this design of springing there was the same very limited movement, the springs being stiff with high periodicity, and consequent

vibration. In the author's opinion, this type of springing, where the fork is hinged from the bottom of the fork stem, means that much of the road shock is transmitted direct to the frame, especially when the spring is stiff.

The extension springs broke through being overstressed, and the author has often broken the springs when riding over one bad obstacle or hole, though it is quite probable that these particular springs had been weakened before by being stressed just up to their limit.

With the helical wound spring acting in extension, on one or two types of machines the top and bottom ends of the spring were bolted up solid. The front forks were connected to the head by links, the movement of the forks being thus circular, the radius of the circle being the length of the link. If the two ends of the spring are in line with the spring straight, when the spring is extended under load one end will move along an arc, and it must move out of line with its other end, thus bending or distorting the spring as well as extending it. The angular movement of the spring ends can be altered slightly by having the top and bottom links of different lengths and at different angles in relation to each other; but with these modifications the spring still distorts, and they have their own objections. In such cases it is usual to have the spring bent when in the normal position, so that it tends to straighten when further extended. It may possibly be said that the heads of the bolts securing the ends of the springs were so shaped that the spring could be free to find its own line, and this was possibly the wish of the designer; in practice, however, the spring distorts. Some people say that this was very detrimental to the springs, and was the cause of so many of them breaking; to what degree this is true, the author cannot say, but he thinks there is a considerable amount of truth in the argument, and this arrangement is without doubt unmechanical. It is surprising to note that on one or two of this year's front fork designs the springs are still bolted up solid, the ends not being allowed to swivel. The $2\frac{3}{4}$ h.p. Douglas machine was supplied with the spring ends bolted up solid during the war, but on the latest type this has been altered, and the spring ends now swivel on pins, Fig. 1.

These $2\frac{3}{4}$ h.p. Douglas extension springs broke (though by no means invariably) where the spring commenced to taper down at the ends, and where it became in fact a conical coil spring in

extension; it is well known that a conical coil spring in extension distorts, and this is the place that the author would have named as being the weakest in the spring, although he has not gone to the length of abstruse calculations to prove it. On the latest Douglas 4 h.p. machines the springs were all parallel, and the ends were allowed to swivel on pins; very little trouble was experienced with this type.

At one time Messrs. Douglas fitted an extra horizontal buffer

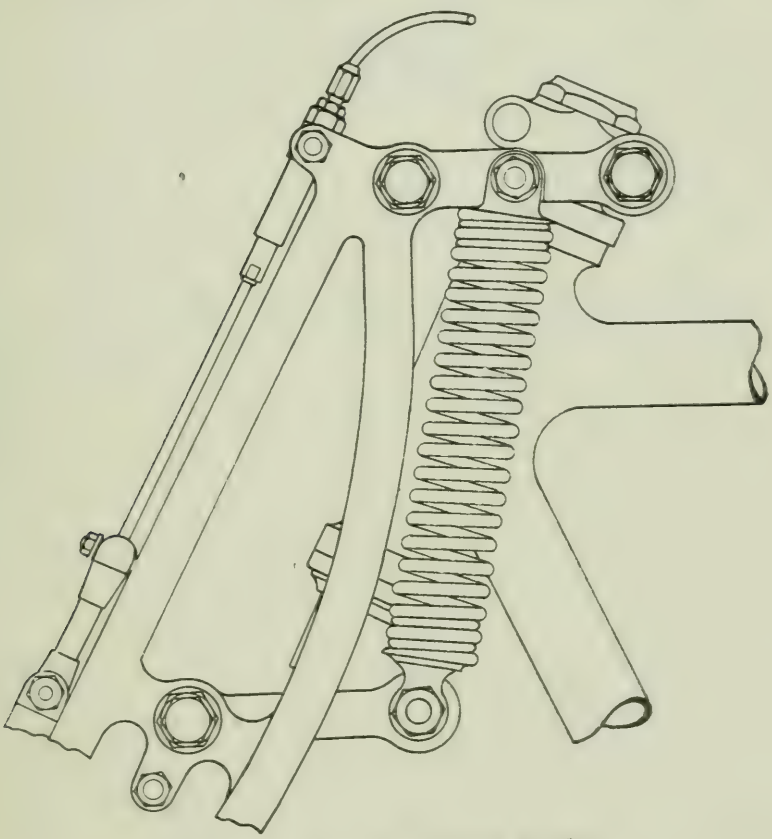


Fig. 1.—Douglas Front Forks.

spring box head clip, the allowable movement on which was small. The author does not think that it made any appreciable difference to the riding qualities of the machine, or to the number of broken springs, and on the latest of the firm's models it is not fitted. The link motion on the Douglas $2\frac{3}{4}$ was at first very light, but it was improved and made heavier. On all the makes, with the exception of the Triumph and Douglas 4 h.p., the link motion up to the last was too light, the chief faults

being the small diameter of the spindles and the lack of a proper method of lubricating them. The latter was ridiculous, usually consisting of a small hole, which no oilcan end would even look at, though occasionally toy grease cups were fitted, the cups being about 5/16 in. diameter. The author practically never saw a link motion taken to pieces but that the spindles were dry. It is probable that if well lubricated even the lighter spindles would have stood up much better.

On the later type Douglas 4 h.p. machine, the spindles were of larger diameter and drilled up, the end of the spindle being screwed, and on this a greaser cup fitted; this was a big advance on previous practice, and very little trouble was experienced with them.

On the Triumph machines the front fork was hinged at the bottom of the fork stem, a ball bearing being fitted, which stood up well.

Except on the very earliest types of Douglas $2\frac{3}{4}$ h.p. machines, the author saw practically no cases on any make where the links had bent during ordinary wear and tear, though in collisions and accidents, of course, they suffered considerably. The links used often to "work" where they were bolted on to the spindle ends, and where they should have been tight. Sometimes this was due to lack of lubrication facilities, so that the spindles became dry and partially seized up, in which case the links would start turning on the ends of the spindle. The diameter of the spindle end, the thickness of the link at the eye, and the face on the spindle which the link came up against were all too small, and once the links started working at the eyes it usually meant new links and spindles to make a satisfactory job. The surfaces for taking the side pressure were small, though any play developing could be taken up by washers, and later in the war some makers fitted hardened steel washers.

In the author's experience, when lateral play developed, a great part of it was usually, in addition to play in the head, play in the spindle bushes and sometimes the links working slightly on the ends of the spindles, which allowed them to rock. If these features were improved there should not be much trouble with lateral play developing. The fitting of double coil flat spring washers between the faces of the castings and of the links was a good arrangement.

Saddle springs stood up well, but chiefly because the saddles

bumped before the springs were overstressed; sometimes they broke, but it was unusual.

An account of two or three special models that were sent to France for test, and of some special machines built up in the R.A.S.C. Workshops there, should be of interest.

An early test machine that came out from England was a Douglas $2\frac{3}{4}$ h.p. rear spring model, Fig. 2. It was fitted with two

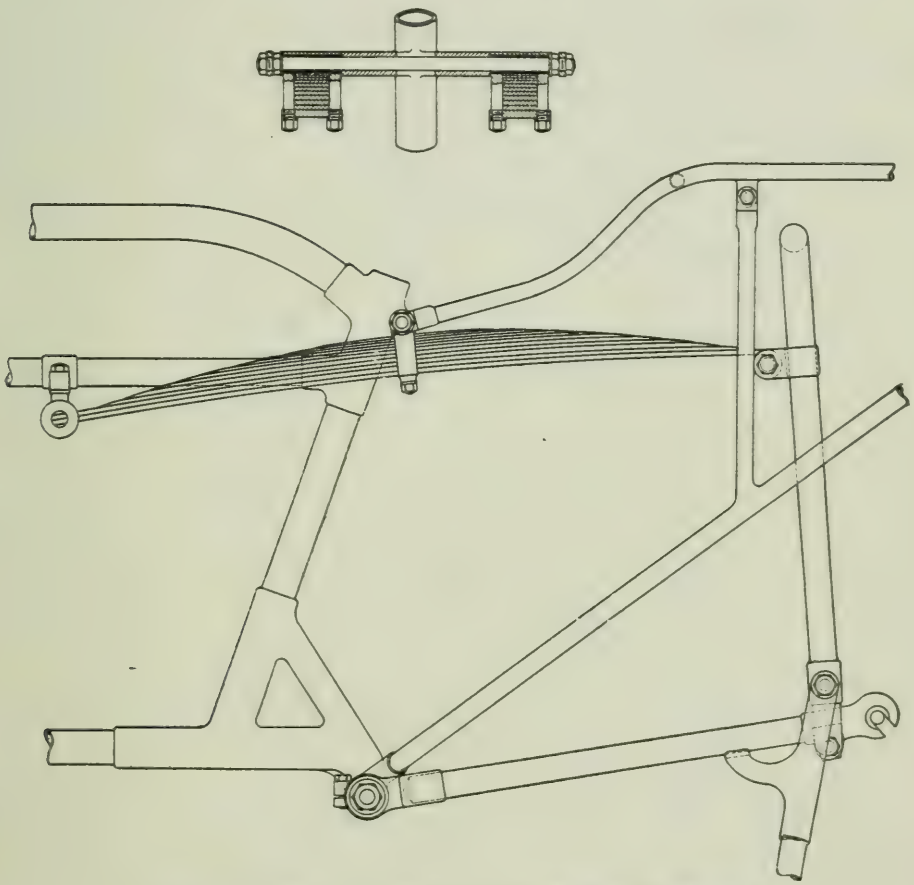


Fig. 2.—Douglas Laminated Spring Model.

cantilever springs, one on each side of the rear wheel. The forward ends of the springs rested on a bracket about 8 in. along the middle rail, the centre of the springs being hinged by the seat pillar lug, and the rear ends shackled on to a stay that went over the wheel. Unfortunately, the author did not test this machine himself; he has been told, though, that the early reports were good. The next experimental Douglas machines that came out were two $3\frac{1}{2}$ h.p. machines, one fitted with rear springing; the

machines came down to the 3rd Heavy Repair Shop from up the line, where they had evidently had hard work. Helical springs were fitted as shown in Fig. 3, which is self-explanatory. The bracket on the middle rail anchoring the forward end of the springs was broken, and this was repaired. The author rather believes that a new middle rail was also put in. The spring cages showed that the movement had been to the end of its travel fairly often and fairly hard. The machine was good and very comfortable to ride, but the springs were too stiff to be responsive to the light shocks. As far as the author remembers, there was no appreciable wear on the pins, or the main spindle bearings. He had, of course, no means of ascertaining how much running the machine had actually done.

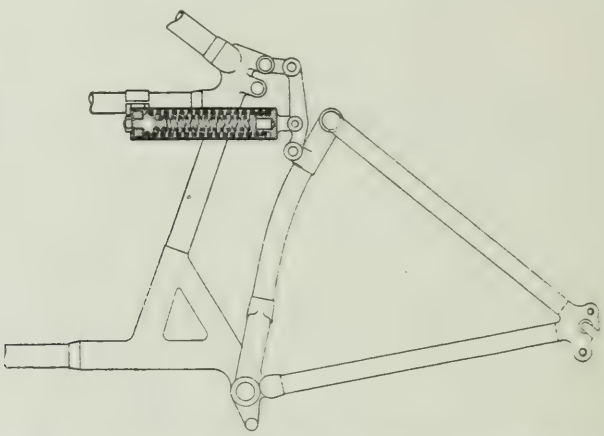


Fig. 3.—Douglas 3½ h.p. Helical Spring Model.

Another spring frame machine to come out for test was the Royal Ruby. This machine differed from the latest model shown in Fig. 4 chiefly in the position of the rear spring. It was more or less vertical then, it is now horizontal; the shackles also were different, with one or two other minor details, but the front spring was much the same. This machine, as it came to the author, had had some fairly severe work to do, but again it was impossible to say what mileage it had done. The only criticism was that the wheels were out of track a little, and care had to be exercised when riding the machine. The springing was very comfortable, and the machine was very much less tiring to ride than one with a solid frame. An objection to that design was the ragged appearance, but as far as the author can remember the spring details

were standing up to their work well, and showed practically no signs of wear. The makers informed the author that the reason

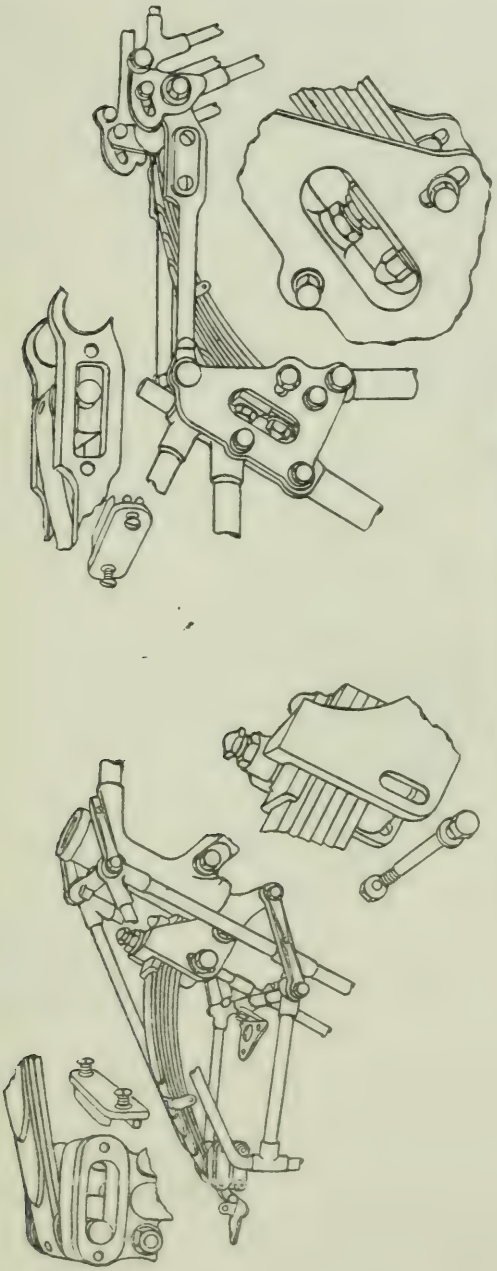


Fig. 4.—Royal Ruby Standard Model.

for the change in the position of the rear spring was that they got an improvement in springing and manufacture.

Of the machines built in France, the first was a Douglas $2\frac{3}{4}$

h.p. rear spring frame model designed by Capt. Francis and built at the 3rd Heavy Repair Shop. It had two leaf springs, one on each side of the rear wheel, level with the middle rail, the forward ends resting on a bracket bolted to the rail. After some running, this middle rail showed signs of sagging, and it was changed for one of a heavier gauge, after which it gave no trouble. The machine was, of course, only built up as an experiment. The deflection from the normal, as far as the author can remember, was 3 in. to 4 in., and it was a very comfortable machine to ride.

Another design, which was got out by the author, was the fitting

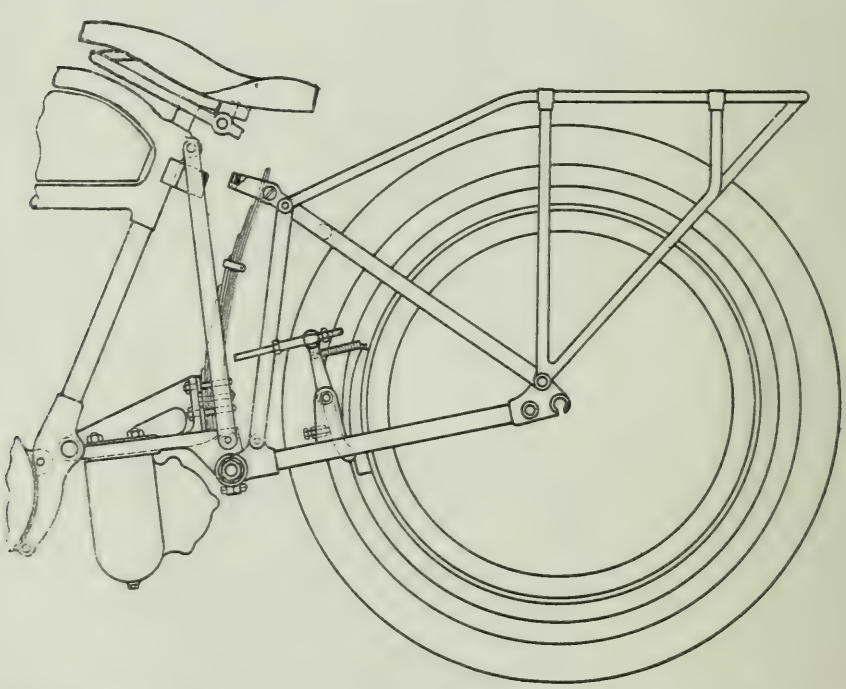


Fig. 5.—3rd H. R. Shop Spring Design on Triumph Machine.

of rear springing to a standard Triumph machine. There was nothing particularly original about the design, the general arrangement of which can be seen from Fig. 5. The carrier and rear mudguard were unsprung. The spring had a flat end, with no eye or shackle, but behind the end was a cross bar, which caught the spring on rebound or when the rear wheel dropped into a hole. The details of the main spindle bearing can be seen in Fig. 17. Grooves were turned just inside the bushes to collect any dust and mud, etc. that might work in. This design was simple and substantial; facilities and time did not allow

of the details being elaborated. It is regretted that the author has not got particulars of the strength of the spring, etc.;

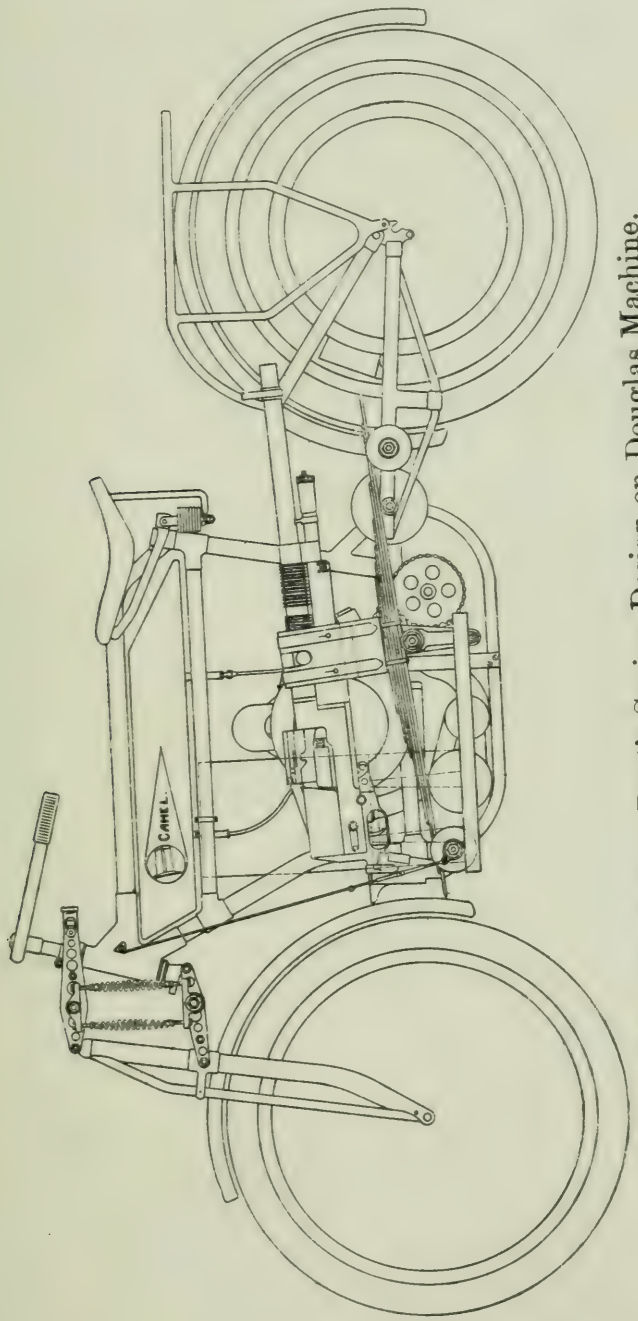


Fig. 6.—Capt. S: J. Burt's Spring Design on Douglas Machine.

from memory he would say that the allowable movement from the normal was about $3\frac{1}{2}$ in. The difference between the riding

qualities of this machine and those of a standard Triumph was enormous. One officer in the author's unit occasionally had to ride about 80 miles over bad roads, and he always complained of a very aching back on arrival at his destination. For one or two of his trips he took this spring frame machine, and he reported afterwards that at the finish of his run he arrived back comparatively fresh and a different man. The author himself, on a particular test run, covered approximately 90 miles over the Rouen St.-Omer road in well under 3 hours on this machine, and considering the very bad surface of the road the time was good. It would, in the author's opinion, have been practically impossible to do this time on a solid frame machine. It is interesting to note that after covering 15 to 20 miles of the return journey at quite a slow speed the front wheel fell to pieces. The wheel was a good one as it had been looked over before starting on this test, and it had simply shaken to pieces on the outward run; a standard Triumph front fork with front fork spring was fitted.

Another spring frame machine made in France was one designed by Capt. S. J. Burt of the 61 Aux. P. Coy., see Fig. 6, and some exceptional results were obtained with it. The machine was a standard Douglas $2\frac{3}{4}$ h.p., but different front fork springing was fitted, which gave much greater movement, and on the rear two cantilever leaf springs were fitted, one on each side of the wheel, also with a large range of movement. In consequence of this large movement and the comparatively small ground clearance of the machine for its particular work, a special skid was fitted under the engine. The extra and additional load for this machine was a Hotchkiss gun and 360 rounds of ammunition, which it used to carry over fields and the very bad roads prevalent in France. This machine was, the author believes, built for duty with Mobile Motor Machine Gun Batteries, and after inspection by H. Q. it was approved for this work.

This finishes the account of the machines in France, the author having touched on the main points which cropped up during his experience and work with the M.T. He thinks that it would not be out of place now to give some points which in his opinion it is desirable to bear in mind when considering the design for front and rear springing.

Some of the results of bad springing have already been mentioned, but in addition to these, as higher speeds become more common, the springing must be developed not only for comfort,

but to enable the mechanical details to stand up to their work better. It would not appear that this should add much to the cost of the machine, as there will certainly, after a time, be a saving by making the whole machine lighter; even if the first cost is greater, however, provided that the springing is well designed, it should be one of the best investments that the motor cycling public can make.

In considering the design for an efficient front and rear springing arrangement on a motor cycle, one of the main questions is, How much movement of the front and rear forks and wheels can be allowed? On a car, within limits, the greater the amount of relative movement it is possible to allow between the chassis and axle, the more flexible the spring may be. This means that the oscillations are large, and the period is in consequence low, and as the criterion of the stiffness of a suspension is the spring period, so this tends towards good springing of the car. If the relative movement is reduced, the weight remaining the same, the springs have to be made stiffer and their period goes up, and there comes a point when the spring is either no longer sensitive to the smaller shocks, or is sensitive to the smaller shocks and bumps for the heavier ones. The relation of sprung to unsprung weight is an important point. Taking the instance of an unsprung car, its inertia would tend to keep it moving in a straight line, but if it meets an obstacle, the car must move out of this straight line, and this imposes a great strain on the tyres; if the car is sprung, however, only the inertia of the unsprung parts, axle and wheels, etc. (which are much lighter), has to be overcome, and the consequent stress is proportionately lower. On all vehicles this is the same, though whether the relation of sprung to unsprung weight is more or less the same on a motor cycle as on a car the author does not know; he thinks it probably is. It is, however, desirable to keep unsprung weight as low as possible. A point in which the problem of the springing of the motor cycle is easier than that of a car is that the load carried never varies much, while on a car it may vary enormously. The above only holds good if the carrier be unsprung, the tendency at present being to carry an extra passenger seated on the back, a point which will be dealt with later. On a car, even with its greater possible relative movement, it is difficult with a non-progressive spring (that is, a spring of which the deflection under any load less than its maximum carrying capacity is directly proportional

to the deflection under full load), to design it to be responsive to the small as well as to the heaviest shocks. On a motor cycle where the allowable movement is more limited, the problem becomes more difficult still. The reason that supplementary springs, or shock absorbers as they are sometimes called, are often fitted to cars is because the laminated springs by themselves cannot be made to cover both the light and the heavy shocks. Often the allowable movement is not large, or the variation in load carried is considerable, in which case the springs have to be designed to carry the heaviest load. The alternative solutions seem to be to have either very flexible springs and buffers to take the heaviest shocks, or to have laminated springs with supplementary springs,

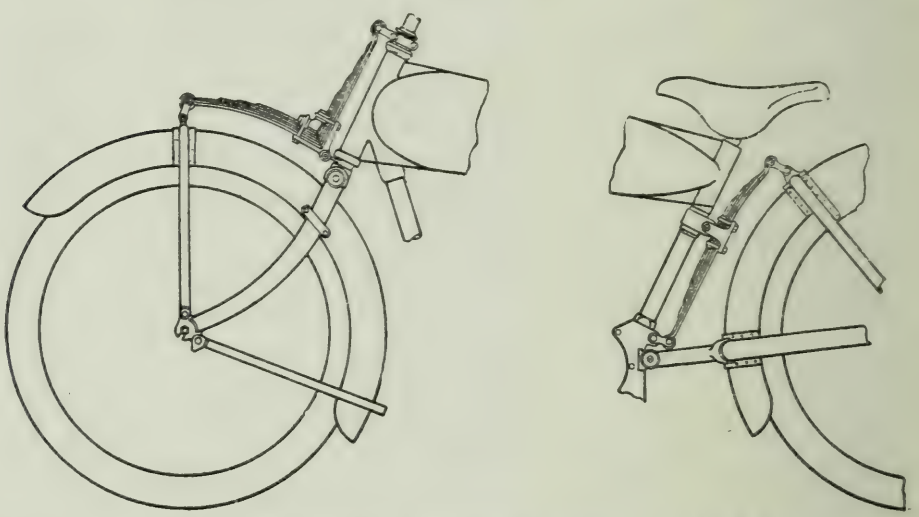


FIG. 7.—Beardmore Precision Standard Model.

or to have a combination of springs, helical, for instance, where, as the main spring is compressed, other springs come into action. It would also be possible to alter the leverages of the helical springs, as they are either compressed or extended, or to alter the rate at which they are compressed or extended, which would achieve the same results.

Before proceeding further, a desirable and practical permissible movement from the normal under load for the front and rear forks should be decided on. The author suggests $2\frac{1}{2}$ in. to 3 in. for the front forks and $3\frac{1}{2}$ in. to $4\frac{1}{2}$ in. for the rear. There will, of course, be very considerable differences of opinion on this point, though, that the mechanical difficulties can be overcome

comparatively easily will probably be agreed; the point in contention will be as to the amount of the movement. The Beardmore Precision Co., for instance, inform the author that they have experimented with larger deflections than they at present allow on their standard spring frame machine, Fig. 7, and have rejected them. They found that the movements were too great in their opinion for the short wheel base of a motor cycle, and that the periods were too slow and the springs heavy. On their standard machines they allow a maximum movement of about $1\frac{1}{2}$ in. for the front wheel and 2 in. for the back wheel, and this, in their experience, gives full relief from all road shocks.

The Royal Ruby Cycle Co. say that their maximum allowable movements are about 3 in. on front and rear wheels.

The author has been informed by the Beardmore Precision Co. that in their experience the steering is hardly affected by the large movements. This agrees with his own experiences on the 3rd Heavy Repair Shop spring frame Triumph machine already mentioned.

With even the $2\frac{1}{2}$ in. to 3 in. and a $3\frac{1}{2}$ in. to $4\frac{1}{2}$ in. movement from the normal, there is no doubt in the author's opinion that a non-progressive action spring cannot be got to include the range of light and heavy shocks. The possible solutions of this difficulty are, as mentioned before, firstly, a non-progressive action spring with a buffer; this is not good though, as it means that the heaviest shock is taken finally on the frame. The second is a laminated spring with supplementary springs, and this is a practicable solution. Care must be taken when designing the above to see that the supplementary springs are called upon to take direct thrust only, the point of contact between the end of the spring and the wheel usually changing as the spring is compressed. There are many ways of effecting this, and two suggested designs are shown, Figs. 8 and 9.

The third is springing with helical springs, where, as the main spring is compressed, other springs come into action; this is already used, but not with such large movements as are suggested here; the system has advantages but also many disadvantages. A helical spring in which the free height is approximately 3 in. over the solid height, would require for the average weight machine to be about 12 in. in length. This would have to be enclosed or to have a centre guide to prevent its buckling, and it would be somewhat difficult to fit in on the machine. On a design of this sort, with

several long springs working in combination, care would have to be taken to see that the springs did not foul each other, and numerous guides would be required, while there would also have to be many small pins and eyes, bolts, etc.

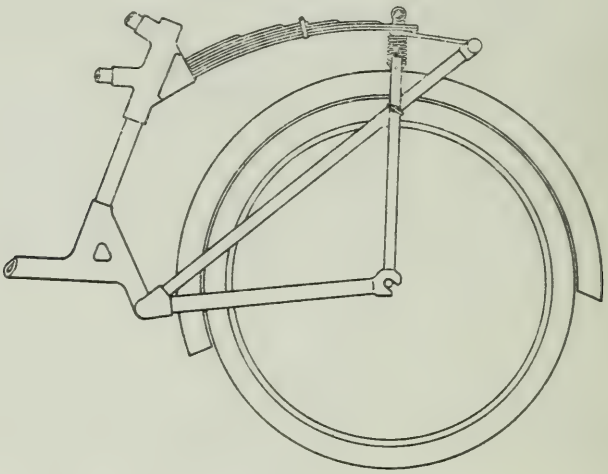


Fig. 8.

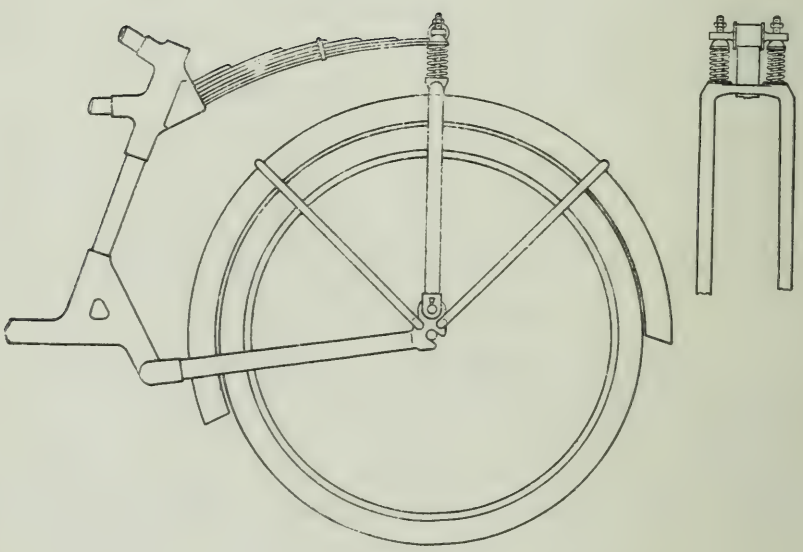


Fig. 9.

With extension springs it would be difficult to work in a combination type, and the only suggestion is to change the action from the non-progressive type to a resultant action in which the load was no longer directly proportional to the deflection. This

could be done without much difficulty by altering the leverage of the spring on the links as it was extended, or by increasing the rate at which it was extended for a given movement of the wheel. Experience has shown that in parallel wound extension springs, the safe value of the stress per sq. in. is only about two-thirds of that for open coiled compression springs of the same dimensions; other considerations may, however, outweigh this disadvantage. The author believes that pneumatic springs have been used on motor cycles years ago,* and they have much to recommend them if the mechanical difficulties can be overcome.

A last alternative is the volute or conical spring, which can be made to practically any rate of progression, and it would seem to be worth while experimenting with this type. In one type of front fork, conical springs acting in compression are already fitted, but they are short and are only very slightly taper. A conical spring is not satisfactory when used in extension, as sufficient load to fully stress the smaller or stiffer coils cannot be applied without distorting the larger coils.

Before leaving this side of the springing question, it would be as well to discuss the merits or demerits of friction, and this has lately been a much debated question. For perfection, a damping device which would come into action after the first big deflection either way, after the wheel has passed its normal position, is desirable. If the author could not have this, however, he would prefer to have friction in the springing of a motor cycle rather than none at all, chiefly to steady the springs and damp out oscillations, this especially when the machine is travelling over a road with more or less regular undulations which happen to fit in with the natural period of the motor cycle suspension. The objection to friction, as pointed out by Mr. G. H. Baillie in his paper before the Institution in 1913,† is that it increases the change of acceleration, and change of acceleration causes that jerk which is uncomfortable to the rider or passenger.

In the case of a car, as there is very little danger from the cumulative effect of regular undulations on the springing (which would be greater with no friction in the springs), comfort is the first consideration. On a motor cycle, however, the case is different; the machine is in a state of unstable equilibrium, and as

* See Proc. I.A.E. Vol. IV. p. 87.

† See Proc. I.A.E. Vol. VII. p. 451.

this cumulative effect on springing could be very dangerous, the extra or excessive comfort must be a very secondary consideration.

On a considerable number of machines now horizontal springs are also fitted on the front forks; the wheel is thus sprung in two directions, and that these springs do useful work can be appreciated by riding a machine so fitted. In the author's opinion, however, these are only useful when vertical springs are too stiff, and on a front fork fitted with reasonably flexible springs the horizontal springs would not be required, especially as the motor cycle front forks are already at an angle of about 30 degrees to the vertical in the direction of the shock.

A point that has been raised in objection to these horizontal springs is that the steering lead is continually changing, and that there would be danger when riding round a curve. A movement of about one inch is fairly usual on these horizontal springs, which would be $1\frac{1}{2}$ in. to 2 in. on the wheel. In the author's experience, these horizontal springs do not have any particular effect on the steering, although it is possible that they might at high speeds.

The question arises as to whether it should be arranged that the springs give lateral support to the forks. That the laminated springs do give lateral support on most makes of cars is well known; at the same time it should be remembered that a certain amount of side play on the axles of the ordinary touring car does not matter, while on a motor cycle practically no play at all can be allowed. The question is a compromise, and the author only says that if possible it would be better to arrange that suitable faces should give lateral support, and the springs take their direct load only. On the type of spring which is fitted with an eye and shackles, if the bearing in the spring eye is to work nicely, it must have some clearance on the sides, so that before the spring can help in the lateral stability the bearing in which the rear fork is hinged must have worn enough for the fork to rock over and take up this clearance. In the author's opinion, by the time this bearing is worn to that extent it requires renewing. In the A.B.C. type of springing, Fig. 10, the spring does give lateral support to the wheel, as it is bolted solidly at each end to the frame and the rear fork respectively.

Dealing with the eye of the leaf spring, on several makes shackle plates and pins of good proportions are employed, and on the Royal Ruby patented roller bearings as shown in the upper part of Fig. 4 are fitted to eliminate lateral play, and they should

work well. The author thinks, however, that these shackles are a refinement, and he would recommend in a great many cases simply a flat end to the spring and no eye. A roller for the spring to work against is unnecessary, it should simply work against a flat face; at the end of the back of the spring should be a crosspiece, with possibly a rubber buffer to catch the spring on rebound or when the wheel drops into a hole. An objection to this type is that there would be a tapping noise if the end of the spring left this face, but the author has ridden a machine so fitted, and he experienced no inconvenience. This type is much cheaper to make, and the possible noise could hardly be counted a serious objection until the motor cycle engine

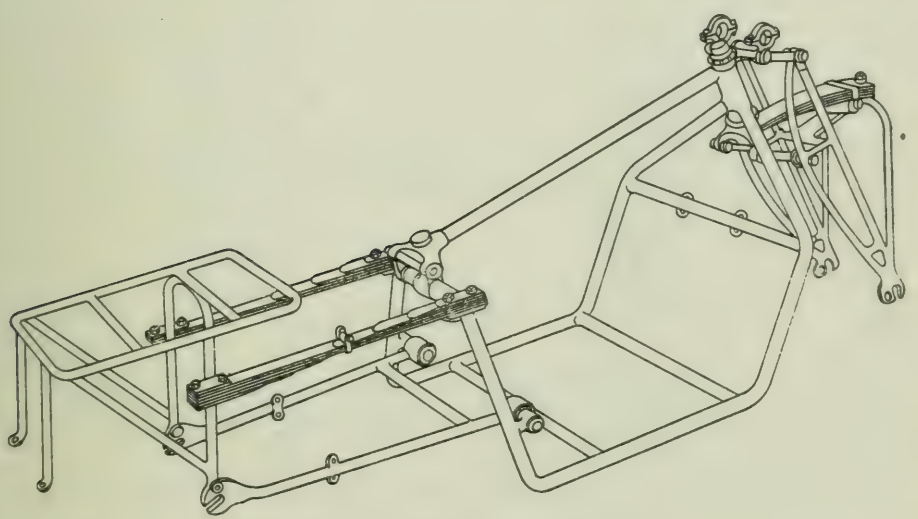


Fig. 10.—A.B.C. Spring Frame.

is more silent than it is at present. English designers throughout should remember Ford practice a little more—cheap and simple and doesn't look nice, but the public gets used to it, and it works —Ford taught the public, and didn't let the public teach him.

It would be as well to compare the different designs of front forks, of which there seem to be three distinct types. The most usual is the parallel link motion type, where the links connect from the top of the front forks to the head of the frame, then the type as on the Triumph and the Beardmore Precision, where the front fork is hinged at the crown, and lastly the design which seems to be popular on American machines, and which is also embodied in the Phelan and Moore front forks, where the con-

necting link comes straight off the wheel spindle. It would be hard to say which type is best, as they are, in common with most engineering problems, a compromise; of the three, the author prefers the first mentioned, where the links bridge from the top of the front forks to the head.

The bearings in this type are long, easy to lubricate, easy to adjust, accessible, simple and out of the dirt, and in practice they stand up quite well. The tendency is now to make the link spindles of larger diameter, as on the Douglas type shown in Fig. 11, which is a move in the right direction; they will, however, stand further increase still. Some makers are also providing an easy adjustment for taking up any side play that develops, which is satisfactory as long as it ensures that the links are still

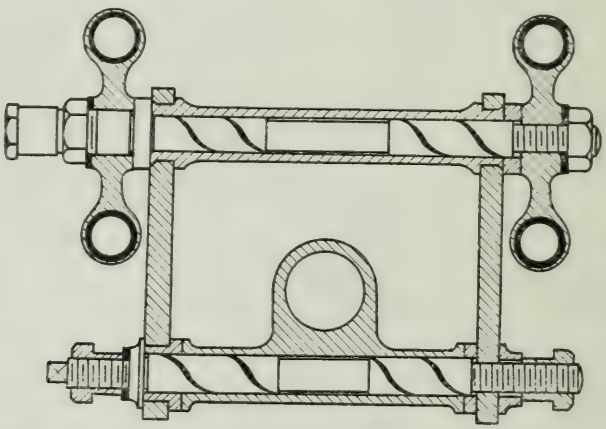


Fig. 11.—Douglas Front Fork Link Arrangement.

held solidly on the spindle ends, or on a connecting tube in some designs, while preferably the links should be screwed up against shoulders. The type of front fork as fitted by Triumphs has, the author knows, already been the subject of a considerable controversy, and personally he considers that on this type a lot of the road shock must be transmitted direct to the frame, or at any rate a great deal more than on the other two designs mentioned; the more flexible the spring, however, the less will this be felt. Where the front fork is hinged at the steering crown, there is the advantage of only one bearing against four, see Fig. 12, and this bearing can be made long and of large diameter.

On the P. & M. and American type, there is the advantage that when using helical springs, the angular movement of one

end of the spring in relation to the other when being compressed or extended is less than with the first mentioned link motion type, and another advantage is that there will be slightly less unsprung weight. When the wheel is in front of the bearing it hinges from,

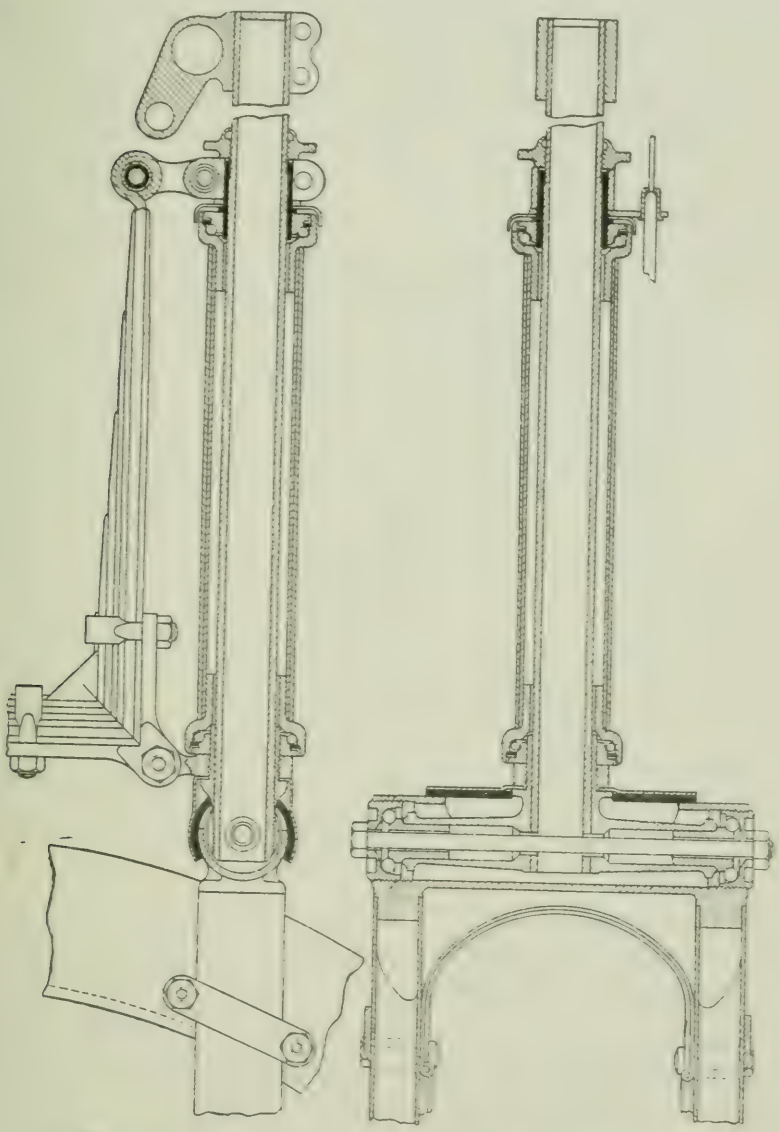


FIG. 12.—Beardmore Precision Front Fork.

it would seem that one advantage of this type is destroyed; if the wheel is behind the bearing, it is trailing, and its position could be such that it would be readily responsive to road shocks from a large range of directions. At the same time, however, the

author does not like this design, as it still has four bearings, which are and must of necessity be very short, and these bearings are working in a position where they are open to a maximum of wet and dust; it would also seem that even slight wear on these bearings would show itself as quite a considerable figure on the rim of the wheel.

With reference to the lubrication facilities on all these different types, some makers are regarding this question seriously; a great many, on the other hand, even on their latest standard models, fit designs which are useless. They do not seem to realise that unless it is possible to lubricate the part with absolute minimum of trouble to the rider, and unless the system is efficient, the part will go unlubricated. They may say that it is up to the rider, but it is also very much to their interests that the machine should wear well. The design in which there is simply a small oil-hole, covered by a pivoted flat spring, is bad in practice, as the oilcan end never fits the hole and the hole also holds practically no oil, and in consequence most of it simply runs over the outside.

Fitting absurdly small greasers is another favourite design. Personally, the author would either recommend the oiler with the hinged lid, which is ideal for the lazy man, or a greaser of reasonable dimensions.

Rear springing on a motor cycle is a comparatively new feature, and it would be as well to regard it separately. The considerations in the design seem to be accessibility for viewing, repair and lubricating, and efficiency, cleanliness of design and cheapness; it would in fact be a compromise as usual. The maximum allowable movement from the normal should, the author thinks, be from $3\frac{1}{2}$ to $4\frac{1}{2}$ inches.

Vitally connected with the problem of rear springing is the practice which is becoming more usual each day, that is, the carrying of an extra passenger on the carrier. It seems absolutely unreasonable to expect the manufacturer to design a machine to give good springing with one passenger, and also with two passengers, one sitting on the carrier; it is, as a matter of fact, impossible, and the public must not expect it. So long, however, as the extra passenger is being carried, the additional load must be unsprung, which could be arranged either by temporarily cutting out the entire rear springing, making it solid, or by having the carrier unsprung; this point of the carrier will be dealt with later. The comparative merits of helical and leaf

springs have been already discussed, and as the author has intimated, for this work he prefers the laminated leaf spring. Satisfactory designs have, however, been got out with helical springs for the rear springing, and there is no reason why they should not give very efficient results.

The Douglas $3\frac{1}{2}$ h.p. machine, which is sprung at the rear with helical springs and is a very original design, is shown in Fig. 3; the author is, however, unable to describe it fully as it is being experimented with further. The illustration is self-explanatory. The author has ridden one of these machines, which was sent over to France for test purposes; it was very comfortable to ride and

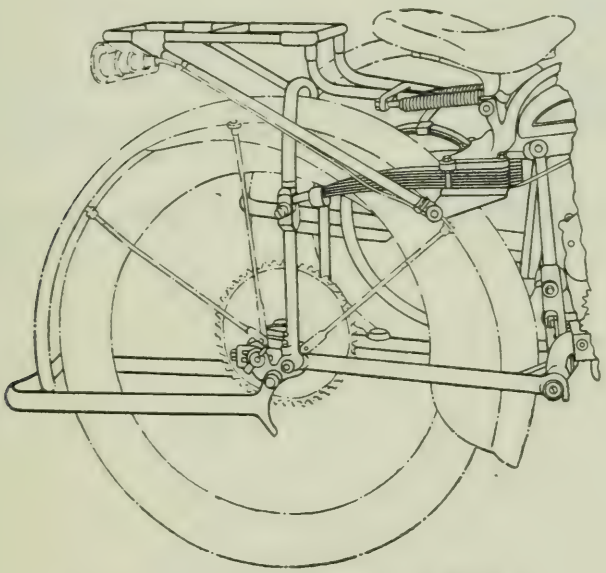


FIG. 14.—Clyno Standard Model.

was well made, the only criticism is that the springs were of necessity rather stiff, the allowable movement of the wheel being small.

Another well known machine which has helical springs for the rear springing is the Matchless, Fig. 13, Plate I—it is to be regretted that on this type at present the ends of the springs are held solidly, and as the spring is compressed they move out of line, or rather, in the normal position they are out of line, and as the spring is further compressed they move into line, which comes to the same thing, as at one position they are being distorted. The carrier is sprung on this machine, so that if an extra passenger

is carried, the springs on the rear wheel would have to support the extra weight.

In some machines a spring is fitted on each side of the wheel, an example of which is shown in Fig. 14. In this design, if one spring is made or becomes a little weaker, or has a different set to its fellow, or if the deflection ratios are different, it would put side strains on the main spindle bearing. One designer told the author that he had ridden such a machine for some way with a spring on one side only, and it had worked quite well; this would probably be so for short distances and while the bearing was in

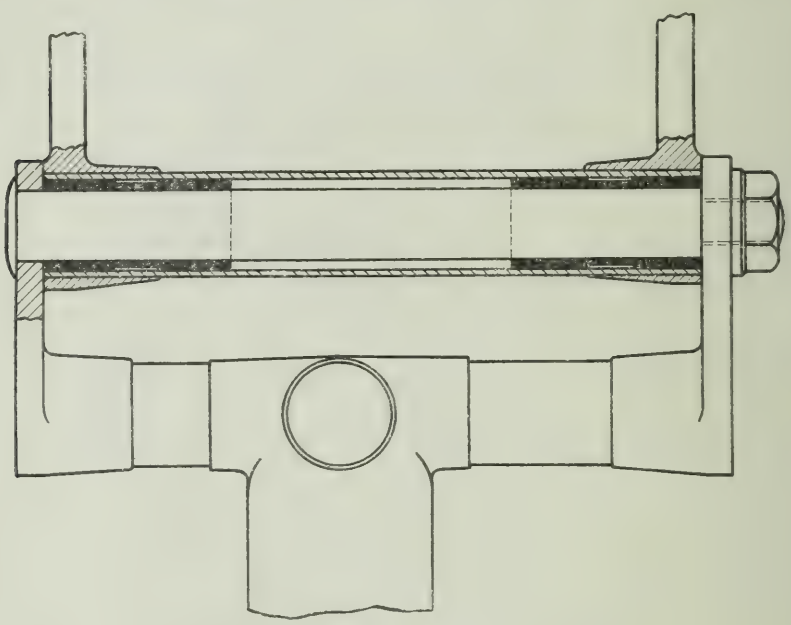


Fig. 15.

good order, but the twisting strains on the bearing would be severe. The position would be improved if the springs were bridged across, with a central point of contact for the springs and wheel.

On some designs where leaf springs are fitted, they are on the short side, which calls for care in seeing that the deflection per unit of length is not too great.

It is difficult to say what would be a best position for the spring, and from the point of efficiency in springing there are a number of positions which would be equally good; accessibility, cleanliness of design, working out of the dust and wet protected by the

mudguards, and cost of manufacture would be the next consideration.

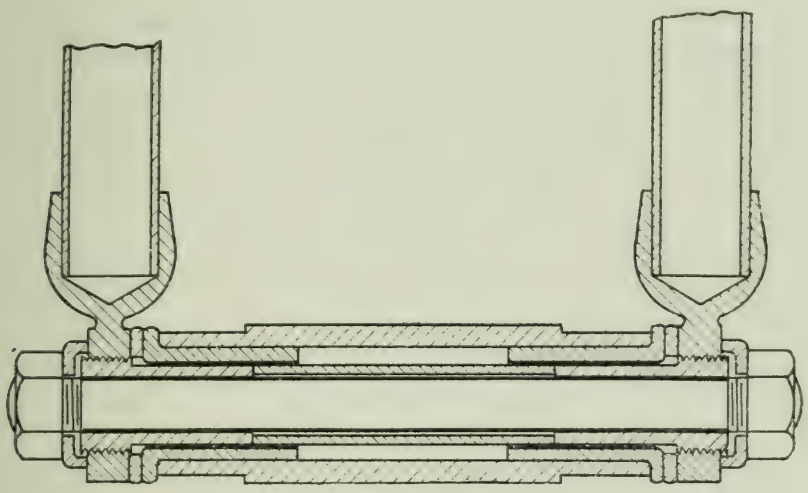


Fig. 16.

Saddle springs act as supplementary springs or shock absorbers for the weight of the rider only. As these saddle springs are part

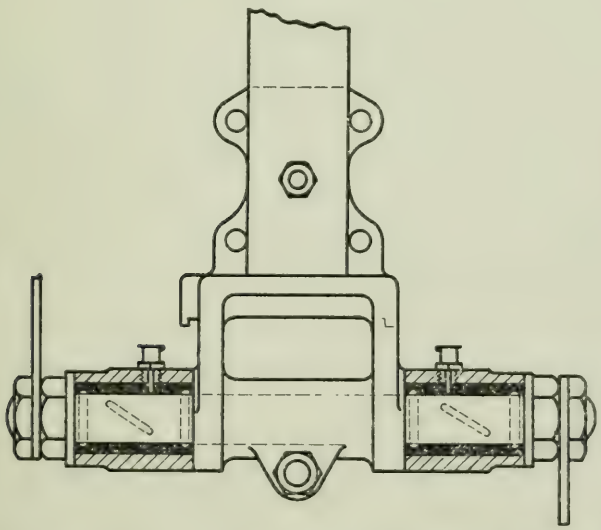


Fig. 17.—Main Spindle of Special Triumph Spring Frame Machine.

of the springing, it does not mean that supplementary springs need not be used in combination with the main spring, or that the main spring could be stiffer. If it did, the power unit, gear box, etc.,

which weigh about one and a half times as much as the average rider, would still be badly sprung, and apart from the bad effect of road shock to these units, the rider would feel the vibration. What should be aimed at is to spring the machine and rider as perfectly as possible, and simply look on the saddle springs as the equivalent of the cushion springs on a car.

We now come to the main spindle bearing design. On the majority of machines this is a plain bearing, examples of which are shown in Figs. 15, 16 and 17, though on at least one design ball bearings have been fitted. All these bearings, although sound, should be of considerably larger proportions, and as long as possible, ample faces for the side thrust should be allowed, and these should be adjustable. When these machines have been in the hands of the average rider for a longer period, there will be more particulars available as to how they wear. This bearing is an extremely important feature; if it develops wear soon (possibly through not being lubricated well, and through careless usage, from both of which it will suffer) slack will be set up, which will destroy the balance of the machine and prejudice may arise in the mind of the inexperienced rider. This it will be difficult to remove as the completely sprung frame is only comparatively in its infancy at present. On account of the under-lubrication which this main spindle bearing, in common with all others, will undoubtedly receive, the author rather favours ball bearings as on the Beardmore Precision machine. It is difficult to estimate the amount of journal and side thrust this bearing will be called upon to stand, and these figures can only be satisfactorily arrived at by test and experience.

It is therefore hard to say in the present stage of development how one bearing will stand up from the point of view of lateral stability, and an alternative is another stay higher up, which would approximate to the links on a front fork. This is already embodied in one or two designs, although judging from the size of the hinge pins, it cannot be with the idea of assisting lateral stability. In both types of bearing, plain and ball, great care must be taken to keep out dust and wet, as the bearing works in a very vulnerable position; on the plain bearing type, the author favours a groove turned on the inside diameter of the bushes on the extreme outside edge, where any foreign matter could collect and not work further in.

The position of this bearing, as is obvious, should be as near to the centre of the gear box pulley or chain wheel as possible. In the normal riding position, the centre of the wheel should be $1\frac{1}{2}$ in. to 2 in. below a line produced from the gear box pulley through the centre of the main spindle bearing. Actually, the shortening of the distance between the gear box pulley wheel and the wheel centre is practically negligible even with a 4 in. movement, and the author has never had trouble with a belt slipping on this account. With a belt there is the elasticity, and a chain never runs dead tight.

We now come to the carrier, and although it is more difficult to effect, it would undoubtedly be preferable to have it as sprung weight, providing the machine is ridden solo. If it is sprung, it saves the carrier bags and their contents considerably. While the present tendency of carrying an extra passenger continues, it must, however, undoubtedly be unsprung, as a protection to the manufacturer.

The back stand should if possible be sprung, thus doing away with the clip on the rear of the mudguard, which is intended to hold the stand up. This spring clip is now being replaced by a positive screw fixture, but this is a nuisance and is always exposed to the wet and dust. The best position for the back stand seems to be under the rear down tube, where it has been fitted on one or two machines already. For keeping the stand up, the design of a spring as on the Royal Ruby machine would seem to be simple and satisfactory. These stands should be made much stronger than they are on the average machine, and fitting them in the last mentioned position raises the objection that the crossbar by the feet has to be done away with. This could, however, be replaced by a circular cross stay fitted as far down the legs as possible. In France these back stands broke continually; they are always very badly used, often the machine is balanced on one leg of the stand when draining oil out of the engine, etc. When lifting the machine up, too, it is usually brought down on to the stand very hard, but this is all the more reason why the stands should be designed much stronger.

Whether to make the springs adjustable for different weights and for bad roads or not seems to be again a question of Rolls-Royce versus more or less Ford design. The Royal Ruby Co. embody this adjustment on their machine, as

shown in the lower part of Fig. 4, and it is without doubt a very pleasant refinement; if, however, a low-priced machine is being built, the author thinks it unnecessary. Adjusting the spring would not, of course, alter its stiffness; it would simply give larger clearances. On a machine with non-adjustable springs, larger clearances must be left to accommodate the increased movement when ridden by a heavier rider or over very bad road surfaces.

Before closing, a word should be put in for larger tyres. That it is more economical in the end and gives easier riding probably everyone agrees. The objection to this from the motor cycle manufacturer's point of view is, of course, the greater first cost of the machine, and at the present time this is a serious item.

The author wishes here to thank Messrs. Royal Ruby, Beardmore Precision, and Douglas Motors for their kindness in helping him to carry out his idea of showing drawings of some of the most progressive types and thus illustrating the general trend of spring frame design. There are, of course, other machines on the market with spring frames, the Coulson, for instance, which did very well in the recent A.C.U. trials, and other manufacturers are, no doubt, experimenting with spring frame machines, but have not yet put them on the market.

The author has not criticised any make of machine in particular, as his arguments for and against springing in general can be applied if desired. The main argument is that the springs must be more flexible, with greater permissible movement, which could be achieved with either helical or laminated springs, only, in the author's opinion, with helical springs the working parts are likely to be more in number, and it is more difficult to attain the desired objective. The flexibility of the springs, front and rear, and the permissible movements are increasing on the best machines, which is all to the good, and the machines are without doubt an improvement on any that have been made before; at the same time he would emphasise again his opinion that to obtain really good springing there must be some arrangement of supplementary springs or combination springs fitted, and this could be done comparatively cheaply.

The author, in closing, would like to say a word on the value of the experience gained in France for the motor cycle industry. It has been, in his opinion, with one or two exceptions, neglected or is

not realised. The desire of every manufacturer is, he imagines, to turn out a machine that will do its work and not break down even when very badly treated, and that there is plenty of scope in this direction has been indicated in the paper.

The period in France was, as he has said, a test to destruction spread over varying periods of time, and it was conducted on an enormous scale. Many of the faults which developed in France showed up for the first time, and in consequence a number of makers seemed hardly to believe the reports or thought they were exaggerated, and they seem to have disregarded the trouble and to have taken practically no steps to prevent its recurrence or to cure it.

They say now, Why should we go to the expense of altering the design to meet conditions which the machines will practically never be called upon to undergo again? At a time like the present, when output is large and export on a large scale must follow, this is an especially fallacious argument.

Although the author believes that the first more or less practical motor cycle was made by Daimler, yet to English manufacturers is due the credit of developing the motor cycle to its present state of high efficiency, and of putting England in undoubtedly the first position in the world for knowledge and manufacture of these machines. Many countries that six years ago took practically no interest in motor cycles realised during the war their usefulness, and to a certain extent the pleasure to be derived from riding them. This all means that England, if it is to keep its premier position, must be prepared to make machines that will stand up to the prolonged tests which they will receive on the Continent, tests that are much more severe than are found in England.

A great many of the failures that were experienced in France do occur in England, though much more seldom, and this test has shown that they were due to weakness in design and not to faulty material or extraordinary circumstances in those particular cases, as may have been imagined.

With the more prolonged test, in certain cases there has been also a very rapid rise in the breakage curve, showing that the factors of safety were small. It has shown, too, how by altering the design of some part, weight might be saved on parts that worked in relation to it, in fact that the weight and strength of the various units and structure of the machine can be distributed much more efficiently.

There are, however, a hundred and one arguments why the war experience was and is the most valuable this industry has ever had, and it will only be wearisome to reiterate them. There is no middle road, the author believes; it is either advance or go back, and that to work on the experience gained in France is the road of advance there can surely be no question.

THE DISCUSSION.

Captain H. D. Teage: The author questions whether rear springing in particular is an advantage or a necessity. Personally, I think that for our English roads springing is a luxury and not a necessity. On the other hand, for the appalling surfaces over which our present-day trials are held I should welcome any first-class forms of springing. I have had some experience of the three special machines mentioned in the paper; on the $3\frac{1}{2}$ h.p. Douglas I rode over 6,000 miles, and it gave admirable results for solo use. Both the forks and the frame were adjustable for side play. The forks were as comfortable as those on any machine I have ever ridden, and in this connection it may be pointed out that what is a reasonably good fork on a rigid type of frame may appear very bad when used with a spring frame. During the whole of the time I possessed that machine I adjusted the spring frame for side play once, and even then the slightest adjustment locked the frame almost rigid. I am inclined to doubt whether, especially with sidecars, the spring frame machine provides the same wheel grip on the road as the rigid machine. For solo riding, however, I do not think there is any difficulty. The design of the steering head races requires very much more attention than it has yet received, although a few manufacturers are now giving attention to it. Very few steering head races will stand up for 5,000 miles without constant adjustment, and it would appear that many of them are not designed to take any axial loading, but merely an end thrust. Springing has a bigger effect on the life of wheel spindles than is generally realised. The type of fork pivoted at the bottom of the head is notoriously hard on front wheel spindles unless the springs are extremely flexible. Another point which has been raised is the question of the increase of average speed which is possible with springing. I think that has to be experienced to be realised. The degree of movement to be allowed for springing is an important point, because it is, as the author states, quite impossible to get reasonable springing with very small movement, and a large movement complicates matters. It is undoubtedly desirable, but it means providing greater ground clearance and sometimes trouble is experienced in

(Captain H. D. Teage.)
providing clearance between the mudguard and the bottom of the steering head. Adjustment for the weight of the rider is probably purely a luxury, and it is quite obvious that rear springing is impossible if it is to do both for pillion riding and solo work. Perhaps rear sprung machines may do something to stop this practice.

A rather interesting spring fork was patented some years ago by a Mr. Waterlow, which appears to comply with the stated requirements very well, although, to the best of my knowledge, it has never been manufactured. It was a fork of the link type that is usually described as the Druid type, but the action was controlled entirely by a leaf spring lying parallel with the steering head and working through a link motion, on the lines of the Saxon. This fork should be admirably suited to present-day requirements. The author mentions that if proper springing were incorporated in a machine it could be made lighter. Certainly some parts of the machine could be made lighter, but if laminated springs are used, and this appears to be desirable, I rather doubt if the total weight of the machine could be reduced. Speaking of unsprung weight and forks, the great majority of British forks are of the link type, where the whole fork is unsprung, and from experience with many types of machines, I have come to the conclusion that the unsprung forks are apt to lead to more chatter than with the very light moving parts. A fork which has not been mentioned in the paper, which has particularly light moving parts and is very satisfactory in action, is that used on the Scott machine, and with this exception I think the P. and M. is the only unsprung fork of British manufacture. With regard to leaf springing, it might be possible to use a modification of the rear springing on the Marmon car, which provided, to all intents and purposes, a variable fulcrum for the base of the spring, i.e., transverse springs were inverted back to back, with hard wood blocks between, so shaped that the spring took up on the block as the load increased.

Recently I have had some experience with the Beardmore Precision machine and found it to be most admirably sprung, although the movement is rather larger than is provided in most spring frames. It is, however, extremely comfortable, and does not appear to have the disadvantage of giving undue side play. With regard to means for damping the action of the springs, many years ago a very admirable fork, the Trufault, was fitted to the Vindec machine; this was provided with a leather pad friction device,

and I still say it was the most comfortable fork I have ever ridden. It was, however, very ugly, and probably for that reason its use has been discontinued. Lubrication is a most necessary point, and has not received very full consideration, but during the last eighteen months one or two manufacturers have begun to adopt a grease gun with an end screwing into the parts to be lubricated. This is probably the best possible form of lubrication. It is absolutely fool-proof, and it is merely necessary to remove a screw and replace by the grease gun and inject. Spring carriers are obviously desirable, especially if the tool bag has to be carried on the carrier, but if the springing system makes it almost impossible to spring the carrier, a simple way out of the difficulty appears to be to carry the tool bag or tool box on the top of the tank, or somewhere near the centre of the machine, where the movement is less. The use of the stand as a stay for the rear mudguard was, I think, perhaps the most unsatisfactory point with the $3\frac{1}{2}$ h.p. Douglas machine, and in spite of strong stays I have broken up two mudguards. For machines which have no spring frames, the use of larger tyres makes an extraordinary difference in comfort, which must be tried to be realised. The machine which I possess at the present moment has 28 by 3 in. instead of 26 by $2\frac{3}{8}$ in. tyres, and the difference between the two is wonderful.

Mr. J. E. Greenwood: I think the paper rather leads us to suppose that very little has been done in this country with regard to experimental work on spring frames, but that is not so. Personally I have been carrying out experiments for five or six years, and I daresay I have ridden 20,000 to 25,000 miles on various experimental models. The first machine I tried was fitted with the Druid type of fork, with auxiliary springs at the top which gave a backward and forward movement. I fitted a leaf spring to the rear wheel of the machine with a 4-inch motion, but it straightway gave the impression that the front fork was rigid, and I had to start and design a front fork with at least 3 inches movement in order to get the full benefit of the rear springing. The rear spring part I fitted on plain bearings, and for nearly 6,000 miles they were satisfactory, but then they began to show signs of wear. This caused a whip in the frame, and the machine skidded in grease and on wet cobble stones.

I think a sidecar complicates the matter of springing very much indeed. There is a peculiar rolling motion on a spring frame

(Mr. J. E. Greenwood.)
with a sidecar which is very unnerving; in fact, the machine seems as if it would turn over at corners, due to the centrifugal force trying to throw the weight outwards, so that the outside spring goes down; it is very disconcerting at first. The bearings for the spring part of the frame should preferably be ball bearings. I have used these up to from 10,000 to 12,000 miles, and they show no signs of wear whatever. I do not think the round tubes with which the majority of spring frames are hinged behind the gear-box are sufficiently rigid to prevent deflection of the wheel from the vertical position. Some year or two ago I patented an idea to make the two side members triangles and connected them together behind the bearing with a large diameter tube. This takes up the strain in torsion, and that is the only stress to which it is subject, by trying to deflect the wheel from the vertical. A frame of this type is practically rigid without any outside help from the springs, although, of course, the leaf springs do help to support the wheel to a certain extent. The Matchless Co. have got over this rolling motion by connecting the spring part of the back wheel with the spring part of the sidecar wheel, and fitting a very rigid truss with pin joints across the back of the sidecar. Personally, I think there will be trouble with this, because, if the machine is being driven fast and the sidecar wheel or the back wheel jumps upwards very suddenly, it cannot possibly lift the other wheel up at the same time and to the same extent, which must throw a tremendous stress on this connecting strut. Mudguard stays and the mudguards themselves have given a great deal of trouble in connection with spring frames. If these are connected to the unsprung part of the machine they break up very quickly. I do not think there is very much difficulty in the sprung carrier to carry an extra passenger. The ideal springs would not carry the extra passenger, but springs can be made which will be quite comfortable on a solo machine, and also be capable of taking another passenger. I have proved this conclusively. I think the greatest requirement now is a light weight solo spring frame machine. For sidecar combinations at present I think the ideal is a rigid frame, with large tyres, with possibly a spring wheel to the sidecar, but not to the machine, and a good fork. This combination gives very comfortable riding. The Brooke's laminated spring saddle will give as comfortable riding on a rigid frame machine as a spring frame machine. I have ridden a machine with this saddle over

the tramway blocks in Wolverhampton, and with 2½-inch tyres the shocks on the saddle when going over these blocks cannot be felt: the machine only appears to jolt forward a little bit. If it is possible to make a saddle so good, and to retain the other advantages of the rigid frame, I think it will form an ideal combination with a sidecar.

Major R. V. C. Brook: The question of designing spring frames is not a simple one, as Mr. Greenwood and the author have told us. Yet I agree with the author when he says, "With well-sprung machines the halt and the lame would soon be riding motor cycles, while the range would be considerably increased for the hale and hearty." Many who are not motor cyclists still hold the perhaps mistaken belief that the motor cycle is a very tiring and vibratory machine to ride, and I believe that, by bringing out a well sprung machine, designers and manufacturers would tap a very much larger market than they can at present. The author also mentions the use of head lugs of steel in place of the usual malleable iron casting. I believe some designers to-day are considering the use of mild steel castings in place of malleable iron. The malleable iron of to-day is not so good as it was before the war, but is very much more liable to fracture. The majority of motor cycles have their front forks sprung, and yet the author points out that that is one of the places where there was most trouble, so that we must take it that even front springs are not as satisfactory as they should be, and we have not only to aim at rear springing but also better front springs. The author refers to bolts and nuts working loose. I often wonder why greater use is not made of helicoid or similar lock nuts, which, I believe, would be better than spring washers. Then with regard to the question of the lubrication of spindles for spring forks, I should like to know whether anyone has had any actual experience of graphite bushes after a year or so's running. It appears to me that if they will stand up to the work they would be ideal for motor cycles, because the average motor cyclist will not bother to lubricate his springing. I think we must agree with Mr. Greenwood, however, that ball bearings seem to be the real solution of the spindle lubrication question. Then there is the question of the effect of springing on the weight of the machine. To some extent weight to-day is due to the need for strength, largely on account of the fact that there is so much unsprung weight. A good system of springing would, I believe, tend to

(Major R. V. C. Brook.)
a reduction in weight, although, as has been pointed out, if laminated springs are used it is a question whether there would be any great saving.

I should like to add my experience with regard to the Beardmore Precision machine, which I rode in one of the recent Trials over very rough ground. I found it magnificently sprung. One objection that I have heard raised is that a machine sprung front and rear does not give the same wheel grip as a rigid machine. I can only say that I did not experience any ill-effects whatever in this direction. The author mentions that care must be taken to ensure that the links on sprung forks are held solidly together. I believe it was the usual practice to hold the links merely by means of a key or dowell in the spindle. I think a better result would be obtained by broaching a square hole in the links and squaring the spindle ends. I suggest that designers ought to consider the question of enclosing the springs. It is a practice which has not been widely enough adopted in car design, but I feel that it must come, and that the springs must be so enclosed that they can be continually immersed in oil. The author says that stands should be made much stronger, but my experience is that the stands have been strong enough but that more attention should be paid to the question of making them automatic in action. It does seem desirable to make frame springs adjustable as on the Royal Ruby, because the proportionate weight of the machine to that of the rider is very much more liable to variation than in the case of a car; the rider may weigh 7 or 8 stone in one case, and 16 stone in another. I am entirely in agreement with Capt. Teage with regard to larger tyres, if the cost will allow it, and I think it would be in the interests of the public that the Press should endeavour to educate the public up to the fact that larger tyres are better for the rider and for the machine, even though they cost a little bit more in the first instance. Engine vibration has a very large influence on the comfort or otherwise of the motor cyclist, and, whilst springing is a very important question, in my opinion the comfort of a two-cycle machine or a small twin-cylinder machine is very much greater, especially on long distances, than that of a single-cylinder four-cycle machine.

Captain S. J. Burt: I find that the spring frame machine evolved by me in France, and which is illustrated in Fig. 6, embodies, with one notable exception, almost every feature which

is advocated by the author. Before proceeding to describe the machine, I hope I may be allowed to endorse the appeal to manufacturers to profit by experience gained in France, and to find means of increasing the reliability of their machines in certain directions. During the four years I spent with the M.T. in France, I saw every one of the failures mentioned occur again and again. I can add to the information given by the author concerning two of these failures by giving a close idea of the frequency of the breakages in my unit. These two parts were the front fork springs and the front down tube of the frame on $2\frac{3}{4}$ h.p. Douglas machines. The machines on my unit were covering on an average about 25 miles a day on the very bad roads around the Souchez district, and it was found that the front fork springs lasted only about ten days, and that the life of the front down tubes was almost three months to the day. It was the constant failure of these parts which drew my attention to the matter of springing. I then re-designed the suspension of one of the Douglas machines, so that the front and rear forks were sprung as illustrated in Fig. 6. The object of the design was to allow very large movements of the wheels yet at the same time to call for the least possible deflection of the elastic media. To begin with the front springing, it will be seen that by means of longer links the permissible movement has been increased to $2\frac{1}{2}$ in. actually, and even this was readily adjustable to give a movement of $3\frac{1}{4}$ in. to $3\frac{1}{2}$ in. The links were fitted with tubular distance-pieces and bolts, which rendered them thoroughly stable laterally. The helical springs, of which there were four instead of two, were provided with an easily swivelling anchorage and so relieved of the distorting stresses mentioned by the author on page 44. To digress for a moment, the effect of fitting this improved front springing arrangement on a frame otherwise standard was to prolong the life of the springs something in excess of twelve times. It also greatly improved the comfort of riding and prolonged the life of the frame. When failure of these springs did eventually take place, it took the form of a gradual extension, a distortion of the coils or "permanent set" becoming noticeable about half-way in the length of the spring. The springing of the rear portion of the machine was carried out by two cantilever springs located just above the footboard. These were pivoted upon a spindle running athwart the frame under the main engine bearer. The front end of the

(Captain S. J. Burt.)
springs took their bearing upon aluminium rollers which were free to turn on a second transverse spindle. This spindle was located at the base of the front down tube. The rear end of the springs bore on aluminium rollers carried by a third spindle, which was fixed to the pivoted back forks and which controlled their movement in a vertical plane. The position of this spindle in relation to the back wheel will be noted, providing, as it does, the minimum movement of the springs with a very large back wheel motion. Concentric with the axis of the pivoting of the back forks, a frictional damping device, or shock absorber, as it is sometimes miscalled, was located. The design of this was simple, and such that, if it were so desired, a certain amount of fork movement could take place before the device came into operation, thus tending in the direction suggested by the author. Owing to the special nature of the work imposed on the machine, it was found desirable to fit a skid under the silencer and gear box. This skid was made reasonably strong and utilised to strengthen the main tube carrying the engine and the central spindle of the springs. It will be noted that the position of the swivelling point of the back forks is by no means ideal, but this position was the best that could be arranged with the facilities at hand. The fact that no trouble was experienced with the belt slipping or the fastener pulling out may be taken to prove that more latitude in the positioning of this point is permissible than might at first sight be supposed. The permissible movement of the back wheel was between $5\frac{1}{2}$ in. and 6 in. The displacement mentioned was utilised to the full under the conditions of use to which the machine was put. The low frequency of the springs gave great comfort in riding. In view of the author's remarks regarding the advantages of adjustability, it will no doubt be of interest to state that the substitution of a pair of rollers of $\frac{1}{4}$ in. greater diameter resulted in increasing the range of possible back wheel movement by 1 in. In view of the point raised in the paper on page 55, I should like to say that my experience was that the large movement in the suspension does not adversely affect the steering. The riding of the cycle was most luxurious, even on the worst roads. In fact, its remarkable efficiency in swallowing the worst road bumps was responsible for the search for some obstruction which would really test it, and it was its efficiency in dealing with these that suggested the possibility of taking a machine gun across country. During the search for suitable

obstacles to test the springing to its limits, the machine was repeatedly ridden both up and down a flight of steps. No difficulty was experienced in surmounting a full petrol can placed in the track of the machine. It was after these tests that the machine gun was mounted on the cycle, and tests were made over such obstacles as might be expected on a cross-country run. These tests proved satisfactory, and the cycle was brought to the notice of the First Army Commander, General Lord Horne. The machine was then demonstrated to the latter and his Staff at the review of the Guards' Machine Gun Brigade held in 1918 on the Arras—St. Pol road, and also later at our own testing ground. Some of the tests were as follows:—

The machine, complete with machine gun and 350 rounds of ammunition, was ridden at a reasonable pace over the following obstacles—

(1) Sandbags piled up to a height of about 18 in.
(2) A large diameter telegraph pole.
(3) A heap of stones 3 ft. high, the angle at the sides being at least 30 degrees.
(4) Down into a hole 3 ft. 6 in. deep and between 8 and 10 ft. wide, which was constructed to be a very realistic imitation of a shell crater.

After the ease with which the machine could deal with each obstacle had been demonstrated, General Horne said that he was astounded and more than favourably impressed with the possibilities of the machine as an instrument of war, and stated that if request for the trial of the machine were put through the usual channel he would forward it to G. H. Q. with a strong recommendation.

Mr. R. T. Morgan: Some time ago I had a machine in which the front forks were of the ordinary link pattern, and these links rapidly developed an angle of about 15 degrees slack, and consequently the machine steered itself within that range. I scrapped that fork and started again to design a fork which would not have any links at all. In this case the front wheel was carried on two laminated springs bolted to the crown at the bottom of the steering column. Each spring had three leaves, one of which was $\frac{1}{4}$ in. thick carried between the other two of $\frac{3}{16}$ in. thickness. To give proper stiffness, I provided very great compression between the springs, and in order to get friction without harshness I fitted perforated sheet brass plates charged with tallow

(Mr. R. T. Morgan.)
between the leaves, which gave sufficient friction to damp out excessive motion, the idea being that the springs should be auxiliary to the pneumatic tyre. The pneumatic tyre dissipates nothing. It takes the shock and gives it all back again, and it is necessary, in my opinion, to damp out these shocks by dissipation of the energy. This front fork worked extremely well, though it looked weak. The extreme range of movement was about 4 in. forward and 4 in. aft, and owing to small clearances it gradually hammered a large dent in the silencer. Its comfort by comparison gave me the impression that I had not got a tyre on the back wheel at all. One of the troubles of the steering column is with the ball races, which appear to be manufactured and fitted without any proper regard for squareness of the seatings of the races. In a great many cases the balls hammer in locally and make little depressions in the races, until each ball takes up a part of the load, by which time the races in many cases are split open and scrapped. The races should bed on spherical seatings in order to get equal distribution of the load amongst the balls. There is not sufficient metal put into the races, and, moreover, the metal is of a very poor quality. At any rate, that is my opinion of motor cycle front fork and other races. I cannot say anything about the springing of back wheels, but the springing of carriers is very important, and very bad at present. There is a particularly smart invention in regard to railroad traction which has eliminated all breakages of drawbars due to sudden snatches by the engine. It has been passed by the National Physical Laboratory as possessing shock absorbing qualities considerably in excess of rubber, and it might well be applied to spring forks. It consists of two links which are made convex, and the two convex surfaces are in rolling contact. The shock is applied so as to compress together one pair of ends, and a spring is applied to compress the other ends together, with a double see-saw effect, the convex surfaces giving a rolling fulcrum which reduces the leverage of large shocks and proportionately increases the leverage of the spring to overcome them, giving a continuous and progressive diminution of the initial deflection, so that quite a small spring can damp out a very large shock.

Mr. R. H. Pearsall: From the point of view of one whose business it is to produce a suitably sprung motor cycle, there arise two main questions. In the first place, variation in loading has to be encountered, and I disagree with the author when he

says this is small in a motor cycle. The rider's weight may vary from 7 to 17 stone, and to that may be added a passenger on the carrier, a sidecar, and three people in the sidecar. It is clearly difficult to provide for so great a variation. Secondly, as Mr. Greenwood points out, the spring frame cycle rolls badly with a sidecar. This, I believe, can be got used to, and if Mr. Greenwood can get the public used to it so much the better, but as the change from rigid to sprung frame is alarming at first, I feel the right course for the sidecar is the rigid frame. The general difficulty is that we are forced to provide motor cycles which will be ridden both solo and sidecar, and what is good for one is not good for the other.

It has been stated that spring frames add to the speed, and the author quotes experience in France, where speeds averaged perhaps six miles an hour. Over such roads I agree, but is it not a fact that professional riders in competitions frequently bind up their springs, when they have spring frames, to reduce their action? And if so, why? Watching the Six Days' Trials, I was impressed with the value of frame stiffness. The stiffer the frame, the better the machine held the bad roads, and I think there is no spring frame machine on the road to-day that does not suffer from loss of lateral stiffness.

Also, with a chain drive and a sprung back wheel there is continual snatch on the chain, causing either wheel slip or chain breakage, generally both. This is very serious, and apparently cannot be relieved by a cushion drive, and is probably the reason for the professional practice I have cited. With a belt, slip occurs every cycle of the engine and the effect is less marked.

Capt. Burt referred to springs with three or four inches motion in front and little more at the back. Such a combination will probably cause pitching; it certainly would on a car.

Mr. W. H. M. Higham: I can fully endorse the author's remarks on the weaknesses of the machines generally, especially breaking of head lugs and front down tubes, although I found that the great majority of these fractures occurred on Douglas machines. I can only remember one case of a Triumph frame breaking. Mudguards and carriers were also a source of trouble, especially on the lighter machines. With regard to dirt in bearings, I have fitted rings after the fashion of oil throwers with benefit. To return to the question of springing, the author does not mention the fact that most Triumph front forks were strapped up, it

(Mr. W. H. M. Higham.)

being suicidal to ride a machine without this. On my own machine they were strapped up as hard as I could get them by putting my full weight on the handle bars. By this means I appeared to obviate the usual effect on the steering due to the design of the Triumph fork. In fact, out of all the machines attached to my workshop, mine was noted for holding the road best at all speeds, from a walking pace up to the limit. Whether this was due to the strapped-up fork spring or just an inherent peculiarity I cannot say with any certainty, but perhaps the author could enlighten me on this point. Of course I lost in resilience, as I discovered when on Belgian pavé, so I coiled half an inner tube round each handlebar to form grips, and thereafter rode many miles in comfort. The rubber barrel-shaped buffer issued out to be tested did not find favour with my men as it was too solid. To test the relative merits of the two standard examples of springing, I fitted Rudge Whitworth forks to my Triumph, with great benefit to the steering at speed. From the point of view of springing, I preferred the Triumph fork, as there was a most unpleasant vibration in the handlebars noticeable with the Rudge fork. I transferred to the Tanks soon after, and was told that in the end the Rudge forks practically fell to pieces, really owing, I believe, to insufficient lubrication. Douglas fork springs were far too weak, in my opinion, for the conditions abroad. On the other hand, they appeared strong enough in comparison with the strength of the frame. On occasion, we used to fit a third spring in front, and the inevitable result was that the front down tube or head lug fractured in a very short space of time. This little tip was rather useful when a Douglas was in such a condition that it was getting more than its fair ration of spares and attention, but not bad enough to send down to the base. Most of my springs on that make broke at the point where the taper commenced. In any case, I think the design was altogether too light for war conditions, thereby causing much too much work for the mobile workshops.

The author has practically ignored sidecar combinations. I had a certain amount of trouble with the 4 h.p. Douglas. Some connections broke, and sometimes the sidecar chassis itself. In fact, it seemed to be a question of chasing the weak spot in the chassis. If it were repaired in one spot it would break in another, and so on. It is my firm opinion that the average sidecar coupling is far too rigid, and as a result an unfair stress is placed on the

cycle frame. This was my deduction from the war experience I had, and it was fully borne out by last year's official report on the Six Days' Trial. To remedy this, I have recently introduced a spring sidecar coupling, which is so designed as to be virtually rigid under ordinary conditions of riding, but will yield under shock. Excessive yield is prevented by a damping device limiting the amount of "give." The effect on alinement is nil, and there is only a very small lateral movement of the sidecar wheel. The device is applicable to any make of machine and is not much larger than the ordinary coupling. The principle would also be applicable to springing in the ordinary way, and embodies the author's wish for a damping device, which would come into action after the first big deflection either way.

Captain T. E. B. CHALMERS: I agree that the lubrication of steering joints is extraordinarily weak. At the moment I am riding a very well known make of Birmingham machine, and find it suffers excessively from sticky joints. This is practically entirely accounted for by the employment of very tiny spring-covered oil holes. I am afraid I do not take the trouble to insert the necessary small oil can into the said tiny holes; hence the shackles stick and rust up and the spring forks become practically useless. Were the shackles lubricated by means of a screw-nozzled grease gun, the situation would be entirely different. One injection could be easily put in and would last a very long time. We used this system on the track bearings of the Tanks, and, although we had a very large number of bearings to grease, we found it quite the best method of lubrication. Capt. Teage mentioned the Truffault spring fork. I think it was quite the finest spring fork, without exception, that I have ever been on.

The matter of tyres is very important when considering springing. In France the $2\frac{1}{4}$ in. and $2\frac{3}{8}$ in. sections were universal. I re-built the wheels of my Triumph to take $2\frac{1}{2}$ in. tyres, and the difference in comfort was extraordinary. Further, the machine was much more easily controlled on greasy roads. The distribution of the weight of the rider also appears to have a lot to do with comfort in riding. I proved this when we substituted the straight T. T. bars for the standard touring bars.

I had the pleasure of testing Capt. Burt's Douglas machine in France, and spent a very pleasant and profitable afternoon with him at his workshop negotiating the various obstacles he has told us of. It was a truly remarkable machine, and, although I

(Captain T. E. B. Chalmers.)
came off several times during my initial attempts, I was thoroughly satisfied that, for comfort in riding, I had never been on a more luxurious motor cycle. It is quite impossible to imagine the easy floating motion over roads full of pot-holes without actually driving the machine.

Mr. Maurice Holroyd: With regard to wheel grip, I quite agree with Major Brook that on a smooth surface there is no apparent difference between a spring frame and a rigid frame, but I have my doubts as to whether a spring frame gives the best results on a loose or bumpy surface. Going up a steep incline, with a bumpy surface, I find we are troubled with wheel-spin. This wheel-spin, however, in my opinion, is rather an advantage as it allows the engine to pick up speed again. In the case of a loose surface I find that the wheel digs in and progress is impossible. With regard to ball bearings as a substitute for hinges or phosphor bronze bearings in conjunction with spring frames, I quite endorse Mr. Greenwood's remarks. If we use a hinge or a plain phosphor bronze bearing, the machine is subject to side roll which is most objectionable.

The question of sprocket centres is a most interesting point. I find there is a certain amount of chain snatch, even with a shock absorber on the countershaft or on the rear wheel. I have broken two driving chains, and it has been on occasions when I have ridden over an excessive bump, and I attribute this to the sprocket centres being thrown out of their original distance in relation to each other, thus causing the chain to get out of pitch with the teeth on the sprockets. My firm, Messrs. F. G. Baker, Ltd., is now experimenting with a driving sprocket having only ten driving teeth, the intermediate teeth acting as trackers so that as the distance between the centres becomes altered the chain will not ride over the driving teeth.

Mr. J. J. Richardson: Being a designer and patentee of spring forks and spring frames, I feel that I am responsible for a few of the faults attributed to motor cycle manufacturers, yet if we were to incorporate the whole of the improvements suggested in the paper and the discussion into motor cycles we should immediately get up against the question of cost, the whole of which has to be borne by the customer, if businesses are to be commercially successful. The difficulty is that people will not pay for these improvements.

It has been suggested by various speakers that when an

ordinary spring fork is fitted to a spring frame motor cycle it immediately shows up the shortcomings of the front system of springing. In the main, most of these forks, apart from those which are designed exclusively for the purpose of accommodating a spring frame, are made to give certain results in conjunction with a rigid frame motor cycle. If a very flexible spring fork be fitted to a rigid frame motor cycle, complaints are received of the movement being excessive, because the two things are not comparative. Therefore, it is not fair to the manufacturers of spring forks to condemn them when a fork which was originally designed for a rigid frame motor cycle is fitted to a spring frame motor cycle.

In order to provide comfort with a spring frame motor cycle it is necessary to give greater clearances than are usually provided. Some designers of motor cycles ignore the fact that a spring fork has got to be fitted, they do not give sufficient clearance, or arrange for the angle of the head lug to be sufficient to give the fork a proper trial. These and many other things have to be taken into consideration before a really well designed motor cycle can be arrived at.

The whole of the criticisms made as broad generalities can be supported, but it would be more useful if a particular type of spring fork and spring frame were taken and subjected to criticism. The broad generalities which might apply to one type would not apply in the case of others, whilst the whole of the generalities would not apply in every case. Personally, I have had some little experience with the difficulties indicated, being responsible for the design and production of many hundreds of spring forks and a fair number of spring frames every week.

Respecting the spring forks used on the Motor Cycle Machine Gun Batteries, which were supplied by the firm I am connected with, I would like to say that the manufacturers were not consulted as to the suitability of the model used. A certain standard type of fork which was available at the time was fitted, and it is not surprising that shackle pins broke fairly frequently when the weight of the outfit (12 cwt.) and the services required were taken into consideration. The authorities instructed us that the springs fitted were not strong enough to carry the weight, and springs were fitted to carry a 50 per cent increase of load; the result was that the shackle pins sheared. We tried to persuade them to take an improved arrangement of the shackle pins and links, but owing

(Mr. J. J. Richardson.)
to the spare part difficulty in the various depôts we were not permitted to change the design.

The other design of fork submitted for the job was used in connection with the M.T. Depôt, and we have never received a report that a single shackle pin has broken since this arrangement has been used. The shackle pin is of uniform diameter throughout its length, and provides for adjustment without the addition of any extra parts beyond those normally required in the non-adjustable type that is fitted with location shoulders for the links to be screwed up against.

These and other little difficulties we had to contend with during the war, and while forks have failed under abnormal conditions for which they were not designed, when we have offered better forks they have been refused, and when manufacturers are offered a better motor cycle spring frame, which anticipates all the suggestions made in the paper and the discussion, probably manufacturers will again refuse, because it seems too good to be true.

Mr. G. H. Savage, in replying on the discussion, said: I cannot agree with Captain Teage when he says that "the grease gun is probably the best possible form of lubrication." The operation of lubricating a machine should be as easy as possible, whereas the filling of a grease gun is most unpleasant, and unless the grease is of the right consistency, it is a strenuous operation. In my opinion, the quickest, easiest, and what could be quite an efficient method, is the large oiler with a flap lid and an oil can. It may be argued, of course, that the lids of these oilers break off, to which the obvious reply is that they should be better made, and this would seem to present no special difficulty.

I agree with Mr. Greenwood when he says that ball bearings are probably better for the rear main spindle bearing, for the reasons stated.

I purposely did not refer in the paper to the sidecar machine which Mr. Greenwood mentioned, through lack of time. In the paper I endeavoured to show some of the advantages of springing, and rather than sacrifice these benefits for a sidecar machine, an anti-rolling device (of which there are several on the market) could probably be cheapened in cost and embodied in the springing.

Mr. Greenwood goes on to say that "this rolling was very disconcerting *at first*," and I am inclined to think that the danger is more imaginary than real. A spring frame machine, with its

greater freedom of action, would naturally feel different from the rigid frame machine. With reference to his criticism of the Matchless design to deal with this rolling action, it would seem that this "tremendous stress" would be no worse than that which at present occurs in the same way on the rigid frame machine.

A last point raised by Mr. Greenwood was that of fitting extra springs in the saddle. In my opinion, this is quite satisfactory, but I would like to emphasise the danger which lies in this direction. The tendency now is to spring the rider well by means of these extra springs, which act between the rider and the frame only. As I stated in the paper, by these somewhat makeshift means, the engine, gear-box, and the other units of which the machine itself consists, remain unsprung. Thus, the greatest advantage of the complete springing, namely, that these units can be re-designed and made considerably lighter, is neglected.

With reference to Mr. Pearsall's criticism on the variation of weight, I would like to point out that my remark was confined to the solo machine—that I have not discussed sidecar springing at all. I would also recommend to the somewhat abnormal man of 17 stone who is thinking of taking up motor cycle riding that he should have other parts altered specially for him besides the springing.

I cannot understand where Mr. Pearsall obtained the figure of six miles per hour for the average speed in France. The only speeds quoted in the paper were those attained on the special 3rd Heavy Repair Shop spring frame Triumph, and these averaged over thirty miles per hour. It is, of course, impossible to say what was an average speed for the motor cycles in France, but it was certainly much nearer twenty than six miles per hour.

Several speakers raise the question of the amount of wheel grip on a spring frame machine compared with that of a rigid frame machine. On a spring frame machine the wheel grip is better, as the wheels tend to follow the inequalities of the road, and are consequently in better contact with the road surface. One advantage of a well designed spring frame machine is that the tyres have a longer life.

Why so many people doubt the merit of a spring frame machine, and also raise this question of wheel grip, is to a considerable extent because they hear of professional riders binding their standard springs up when riding in competitions. The reason for this is not so much to reduce the action of the springs, as to

(Mr. G. H. Savage.)

get greater friction between the leaves. The springs must, of course, if they are to be comfortable, be designed for more or less the average loads which they will be called upon to withstand at the average speed. In competition riding the springs are suddenly called upon to withstand very much greater loads than they were originally designed for on account of the greater speeds and worse road surfaces, and the only thing to be done under these circumstances is either to fit stiffer springs or to increase the friction by binding up the existing springs.

As was pointed out in the paper when discussing the "merits or demerits of friction in the springing," the springs must take the road shocks, but at the same time it is desirable that the oscillations be damped out as soon as possible, and that there shall be no risk of any cumulative effect arising from regular undulations; for this reason, in abnormal circumstances, extra friction is given to the springs by binding up the leaves.

I cannot agree with Mr. Maurice Holroyd in his remarks as to chain snatch. In the longest position the chain should always be a little slack. Then with the maximum movement of the rear wheel of 3 in. to 4 in. the shortening of the centres of the sprocket wheel and road wheel will on most machines be $\frac{1}{8}$ in. or under, and this should not be detrimental to the chain.

I think that Mr. Higham is rather extreme when he says that "it was suicidal to ride the Triumph machine without strapping the front forks up." This saved the springs from being overstressed, but it also had the serious effect (as mentioned later by Col. Davidson) of helping to break the top tubes and head lugs. Naturally, with a great part of the spring action cut out, the shocks transmitted to the frame were much heavier.

COMMUNICATIONS.

Mr. R. T. Morgan wrote: I would like to amplify my somewhat impromptu remarks in the discussion. The spring fork to which I alluded was the very height of simplicity, though its great advantages were tempered by several disadvantages. In appearance it resembled an ordinary bicycle fork, except that the two fork members were laminated springs. I believe that road springs should not absorb the energy of shocks, but should dissipate it. The chief trouble is the tendency for a quick rebound, as the leaves of laminated springs are usually made to support the main plate in one direction only. Consequently, a wheel moves rapidly into a pothole, and the upward movement immediately following is resisted by all the leaves. Special leaves should be provided to prevent the wheel from reaching the bottom of the pothole. Accordingly, in my spring fork, the main leaf spring was clamped between two thinner leaves in order that the friction should damp out or resist a deflection in any direction. The use of perforated sheet brass strips charged with tallow and inserted between each pair of leaves was excellent in practice, as the lubrication was permanent and the springs developed no harshness. A special spring clamp was fitted to each fork member to damp out independent motion likely to cause wobble in steering, and this was found to be fairly efficient in practice. At high speeds there was a tendency to sluggishness in the steering due to gyroscopic action of the front wheel. Also, on a rutty road, the wheel had a slight tendency to follow the ruts rather than the handlebars, which could be much reduced by more careful design. The comfort was really remarkable, particularly at higher speeds. The majority of the shocks were in the form of end thrusts up the leaves, but the fore and aft components were sufficient to swing the wheel through a range of about 7 in. or 8 in. I think that the fork would have been much improved by the addition of another leaf to each fork member.

The chief objection to the fork was the extreme simplicity, the absence of any form of "girder" making it appear weak by comparison with existing types. This "girder" construction is, no doubt, excellent when properly designed, but I have seen

(Mr. R. T. Morgan.)
several instances of "girder" forks in which the combination of strength and weakness was truly comical. My fork was designed so that the greatest stresses should occur at points where the danger from fracture was least, and on one occasion I rode on over 100 miles after discovering a broken leaf without the breakage affecting the running in any way. I should say that the fork would be very suitable for light weight machines without sidecars.

Lt.-Col. A. E. Davidson wrote: In France during the war those responsible for the type of machine to be demanded from England felt that great advantages would arise if a thoroughly efficient spring frame motor cycle could be introduced. These advantages are referred to by the author on page 43. It is only desired to add that the strain of supplying fresh machines to replace casualties was very heavy, and undoubtedly could have been reduced if there had been available a really well sprung frame which would have suffered less damage from shock and bad roads. The qualities which were considered essential in a spring frame before it could be recommended for adoption were as follows:—

(a) Absolute reliability of all the additional parts introduced.

(b) No adverse effect on steering or steadiness even after running a considerable mileage.

(c) No increase in wearing parts liable to require renewal.

Since none of the machines sent out from England fulfilled these requirements, it was arranged that one of our repair shops should set to work to endeavour to tackle the problem. Capt. Savage was in charge of the work, and his experiences in testing models sent out from England and making up others form the basis of this paper.

The Douglas spring frame shown in Fig. 2 failed owing to the top tube being too weak to take the additional stress brought on it by the spring anchorage. After the tube had been replaced by one of considerably stouter gauge, it stood up well and was, in my opinion, much more comfortable to ride than the later model with coiled springs. This was probably due to the fact that the range of movement was greater.

On the question of the range of movement and its effect in relieving the machine of stresses and road shocks, some facts regarding the Triumph and Douglas front fork springing may be

of interest. The various Triumph springs tested in *quantities* were as follows:—

(a) Standard spring giving *full range.*
(b) Long soft rubber buffers (about 5 in. uncompressed)—*medium range.*
(c) Short rubber buffer (about 3 in. uncompressed)—*very short range.*
(d) T.T. pattern felt buffer—*very short range.*

Frame breakages occurred in the earlier years of the war with (a), but were greatly reduced when a thicker gauge top tube was introduced. After that, top tube breakages were probably due in a large measure to the practice referred to by Mr. Higham of strapping up the forks so tightly that movement was restricted to the equivalent of that given by (c) or (d).

(d) was the device used by T.T. riders in pre-war days. It gave a very small range of movement—a restriction which riders consider desirable with the Triumph type of fork for high speed work.

(b) The long buffers of soft rubber could only be obtained in small numbers, and

(c) was the locally made substitute.

Frame breakages occurred with (b) after a reasonable mileage, but with (c) and (d) types after a small mileage. The point to note is that the frequency of frame breakage was more or less proportional to the range of movement allowed by the spring or buffer.

In addition, one trial machine was sent out with a specially long spring with wider range of movement, and although the spring and frame stood up well, the range was too great for safe or comfortable steering. Another trial machine was sent out with double springs, which in practice amounted to a very stiff spring with a small movement for normal road shocks. The frame broke with this device after a short mileage.

In the case of the Douglas, the spring fork eventually fitted to the $2\frac{3}{4}$ h.p. model, *vide* Fig. 1, was first introduced for the 4 h.p. model. The range of movement of the two types was as follows:—

$2\frac{3}{4}$ h.p. models before 1914 with straight links...1 in.
$2\frac{3}{4}$ h.p. 1914 model with curved links .....1-3/16 in.
4 h.p. model .....$2\frac{1}{4}$ in.

(Lt.-Col. A. E. Davidson.)

The 4 h.p. model fork was generally pronounced by riders to be one of the best spring forks ever used by them, and this tends to show that a large range is necessary for real comfort in riding and for saving the machine from shocks.

Mr. F. Strickland wrote: I feel sure that the future motor cycle ought to be sprung both back and front, and therefore that all information on the subject of springing is of great value.

Taking the paper in order, a good deal is said in the early part about the various breakdowns. As mentioned by the author, part of these were due to bad roads, but I agree with him that this was not by any means the whole reason, and that machines should have stood up better than they did. It seems to me that a great many of the breakdowns were due to the fact that the machines were in constant use every day, and that before the war the ordinary motor cycle had not been made so that it would stand such continuous use. Both in motor cycles and pleasure cars it has often been considered that if the machine would stand some years' use in the hands of the average owner it was a very durable machine, and it has been rather forgotten that the ordinary man does not use his machine every day or all day in the manner other types of machinery are used. It may not be necessary that a motor cycle should stand for twenty years doing eight hours' work a day (say 900 miles a week), which would be the equivalent of what many machines do, but they should be made more durable than they are.

In this connection it appears time that the diameter of the tubing composing the frame received real consideration. Motor cycle designers appear to have adopted a tube diameter about the same as that of the ordinary pedal bicycle and simply thickened it to make it stronger. This cannot be right, and if tube about $1\frac{1}{8}$ in. diameter is right for a pedal cycle, as appears to be the case, it cannot be anywhere near right for a motor cycle. Further, it cannot possibly be right to make the tubes the same diameter for the real light-weight and for the heavy-weight machine.

Coming to the question of springing, I absolutely agree with the author as to the desirability of this, both from the point of view of the machine and the rider. If anyone will consider what other vehicles would be like without springs, the advantage of them for motor cycles will be obvious.

I was rather disappointed on reading the paper to find no reference to the springing of the Indian motor cycle. This make

has been sprung for a great many years, and I believe that the system of springing has not been very greatly varied. There has, therefore, been enough experience to ascertain the advantages and disadvantages both of the principle and of the various details, and remarks on this point would be interesting.

In general principle there appear to be two points worthy of discussion, first, the direction of movement of the wheel, and, second, the question of leaf versus spiral springs. With regard to the first, it appears to me that the direction of motion of the wheel should be in the direction in which the shock is received. This is generally a little inclined backwards from the vertical. In cars, lorries, etc. the direction of the motion of the front wheels is usually almost vertical, though it may be slightly inclined back if the springs have any material camber. The back wheels often have a motion distinctly inclined back, as when controlled by a radius rod with a very distinct slope to it. In the front fork springing of motor cycles, however, there are patterns where the direction of motion inclines very much forwards, and the motion is therefore not in the direction of the shock but at some considerable angle to it. It might in some cases be almost at a right angle to certain shocks, and this, of course, would entirely neutralise the spring effect.

With regard to the question of leaf versus spiral springs, it is worthy of note that spiral and volute springs have been tried for all sorts of other vehicles, but have never come into general use on account of the fact that they have no damping effect. If a spring were perfectly elastic, a vehicle fitted with it would continue to oscillate for ever once it had been started doing so by going over a bump. Leaf springs have a considerable amount of friction between the leaves, and this friction damps out the oscillations. The very fact of the friction, however, means that the spring does not move at all till a certain minimum pressure has been put on it. A leaf spring with very few leaves may have very little damping effect, and I have driven a car in which the front springs oscillated violently for some time after going over a bump, and the result was very unpleasant indeed. A spiral spring would have had still less damping effect and presumably been worse. Hence spiral springs are not used in other types of vehicle unless there are in addition enough leaf springs to give the requisite damping. Thus, it is very common in locomotive practice to find *one* axle on spiral springs, as they are far the most convenient

(Mr. F. Strickland.)
constructively, but they are never used for all the axles. It does not follow that spiral springs will be a failure on motor cycles, for it sometimes happens that a method of construction which is unsuitable for general use serves some special purpose, but it seems probable that if motor cycle springs are given the same range of movement as other vehicle springs, leaf springs, either with or without additional volute springs, should be the best.

I cannot agree with the author that carrying a passenger on the carrier varies the load on the springs of a motor cycle to any materially greater extent than on other vehicles. The ordinary weight of the motor cycle with driver is about 400 lb., often more. The extra passenger is usually a lady, quite often young, and therefore with a weight of some 120 to 140 lb. This is approximately 33 per cent of the weight of the machine and driver. A motor bus with driver weighs about 70 cwt. and carries over forty passengers weighing some 60 cwt., or 85 per cent of the weight with driver only. In a lorry, the weight of the load is a still larger proportion of the weight of vehicle and driver. I do not, therefore, see any greater reason for making the extra passenger an unsprung weight than there is for making the load of a bus or lorry unsprung. No doubt it is easier to arrange correct springing if the load is constant, but this applies to cars and lorries as well as motor cycles. Most nominal 5-seated cars will at a pinch carry a good many more, and often do so, and yet are fairly well sprung with the driver, and I would suggest that motor cycle designers should be able to provide for equally varying weights.

The question of the wear of the shackle pins and various parts is very important. Here it is to be hoped that the shackle consisting of flat plates will soon be obsolete, as it now is in cars, owing to its lack of lateral stability and liability to work loose on the pin.

Lubrication is important, but it must be remembered that lubrication takes time, and time is equivalent to money. It is quite possible to spend more time preventing wear than it is worth. Every effort should, therefore, be made to economise the time required for lubrication. Oil cups usually take less time to fill than greasers, and the tops of the latter often shake off. Hence many firms at the Commercial Motor Vehicle Show exhibited vehicles with oil cups to the spring shackles, etc. in place of

grease cups. The ideal would, of course, be oilless bearings, and it appears that it would be quite worth while making a serious effort to fit them.

In conclusion, I would suggest that it would be of great interest if the author could give any information as to the actual stresses under which the springs work. In several places it is mentioned that springs failed from being overstressed, but there is no information as to what the stresses are. This matter of stresses in springs is one on which there is very little information, but from calculations I made on lorry springs when in France it appeared that most of the failures were due to the steel being stressed to a higher load than any steel will stand, and that they could have been very largely avoided by calculating out the stresses beforehand and limiting them to a reasonable amount. What is wanted is the stress on the metal when the spring is compressed to the maximum possible in practical work.

In reply to the written communications, Mr. SAVAGE wrote: In reply to one point raised by Mr. Strickland, I did not intend to say that carrying a passenger on the carrier varied the load to a greater extent than on other vehicles. I think Mr. Strickland will find that the variation of load on the motorbus, lorry or car is the chief difficulty of the spring suspension designer on all these vehicles. If the designer knew what the approximate load on his vehicle would be, he could design a very much better suspension than he does at present. My point, which was dealt with fully in the paper, was that a very good suspension could be designed for the motor cycle if it was not subjected to the unfair loading of an extra passenger on the carrier—a load which the solo machine was never intended to carry.

Mr. Strickland asks if the stresses on the springs can be given. When writing this paper I endeavoured to find out by inquiring from the makers, and by test to a certain extent, if there were any clear ideas as to the maximum stresses these springs would be called upon to withstand. My inquiries were not very successful, however, and it looked as if either the makers were somewhat reticent, or that it was, in many cases, being left to trial and error to decide the question. There is no difficulty in checking, if desired, as to whether any particular helical spring is being overstressed, either by practical experiment or by making use of ordinary well-known formulæ.

(Mr. G. H. Savage.)

On some of the machines in France the amount of the extension of springs was observed, and by ordinary substitution it was found that they were being much overstressed, which fact was, of course, borne out in practice. It is, however, only misleading at present to compare the different springs on the various makes of motor cycles. Nearly all the springs differ as to diameter of coil, diameter of wire, leverages, loads, etc., and each case must be judged on its own merits.

In reply to a further question by Mr. Strickland, in a parallel wound compression spring with open coils, the safe value of the stress per sq. in. is generally reckoned at 80,000 lb. A factor of safety of 3 to 4 should be allowed, however, as the spring is frequently compressed.

With extension springs, the safe value of the stress per sq. in. is only about two-thirds of that for open coiled compression springs of the same diameter.

INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

NOVEMBER, 1920.

The Third Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James' Park, London, S.W.1, on Wednesday, November 10th, 1920, at 8 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following list of Members who had completed their election since the previous Meeting was posted up:—

MEMBERS.

ARCHIBALD MONTGOMERY LOW (London).
HENRY SNOWDEN ROWELL (London).
ALBERT JOHN STEVENS (Wolverhampton).
REGINALD GRAHAM TOLLEY (France).
GEORGE WARWICK (Burton-on-Trent).

ASSOCIATE MEMBERS.

JOHN ADSLEY ORIAL ADDIS (London).
ANTHONY AINSWORTH (Hove).
FREDERICK WILLIAM BOND (Huddersfield).
FRANK GEORGE CUNDY (London).
LEONARD ST. JOHN CLARE (Thames Ditton).
GEORGE WILLIAM EASTWOOD (Dumfries).
GEORGE GILBERT (Lincoln).
JOHN HARRISON (London).
HAROLD GEORGE ROBINSON LAMBERT (Colchester).
JAMES ALASTAIR MARTIN (Glasgow).
HERBERT PARR (Southport).
DOUGLAS HOWARD GRAY RICHARDSON (London).
FRANK EDWARD SALTER (High Wycombe).
JOHN WILDSMITH SNARRY (Birmingham).

CHARLES LESLIE SHERRATT (Birmingham).
FRED SUMNER (Manchester).
HAROLD WHITAKER (Burnley).

ASSOCIATES.

URBAN ASPEY, JR. (Leeds).

GRADUATES.

CHARLES CHILD CORNFORD (Birmingham).
LESLIE FRANCIS ELSBY (Bebington).
FREDERICK ALVEY JONES (Coventry).
GORDON STUART LISSAMAN (Luton).
LIONEL PELLOW (Birmingham).
THOMAS DARWIN WELBY (Birmingham).
ALAN SNOWDEN WOODALL (London).

PROBATIONARY GRADUATES.

DOUGLAS WALTER KING (Wolverhampton).
JOHN EDWIN PEARSON (Birmingham).
EDWARD NORMAN SALMON (Birmingham).
WILLIAM KNOWLES SHUTT (Birmingham).

TRANSFERS.

ROBERT MORTON ATKINSON (Sheffield), Associate Member to Member.

LEOPOLD HAROLD BASKERVILLE COSWAY (London), Associate Member to Member.

ROLAND WALTER EDMONDS (London), Associate Member to Member

DAVID KENNEDY (London), Associate Member to Member.

GEORGE JOHN STEINHEIL (London), Associate Member to Member.

PERCY JAMES LESLIE MANNING (Coventry), Graduate to Associate Member.

ROBERT VICTOR WILLIE NEWTON (Coventry), Graduate to Associate Member.

THOMAS REGINALD SPECK (Birmingham), Graduate to Associate Member.

The following paper was then read and discussed:—

"Some Notes on the Report of the Steel Research Committee," by J. H. S. DICKENSON.

There were present 105 Members and visitors.

SOME NOTES ON THE REPORT OF THE STEEL RESEARCH COMMITTEE.

By J. H. S. DICKENSON
(Member of Council).

Introduction.

The recently published report of the Steel Research Committee, founded in 1916 jointly by this Institution and the Society of Motor Manufacturers and Traders, to establish the mechanical properties of the ten standard automobile steels scheduled in the B.E.S.A. Report No. 75, will, it is hoped, be of very considerable practical value to the users of special steels, and particularly to the members of this Institution.

It should be put on record, as an historical fact, that the first steps towards the completion of the present work were taken at a meeting of the Institution of Automobile Engineers held in Birmingham on December 11th, 1914, to discuss a paper by Mr. L. H. Pomeroy on "Automobile Manufacturing and the War." The somewhat provocative nature of the paper brought to the meeting a number of representatives of steel firms and drop forgers, and an exceedingly interesting discussion took place, with the result, unfortunately somewhat uncommon in such cases, that definite action was immediately taken by the Institution to deal with some of the troubles described by Mr. Pomeroy. It was suggested at this meeting by Mr. David Flather that "A Committee of the Institution should be formed with the object of trying to evolve a concrete system of steels and treatments, and that this Committee should prepare different specifications of steels . . . which will be a guide to the manufacturers in every way";*

* Proc. I.A.E., Vol. IX., p. 420.

a proposal which was supported by Mr. A. Stubbs, who spoke from the drop forgers' point of view, and was actually adopted at the close of the meeting.

A Committee was appointed by the Institution which, although not at first representative in the sense that the three industries concerned appointed their own nominees, comprised five members of steel-making firms, five automobile engineers, and four steel stampers.* This Committee proceeded to work immediately, the Sheffield members first drawing up a short list of specifications for standard steels which, after much discussion, and some modification by the Committee, was subsequently passed on to the British Engineering Standards Association, who, following their usual procedure, called joint meetings of various Committees concerned, submitted the proposals to a number of special steel manufacturers, and finally issued, in May, 1916, their Report No. 75, setting out ten standard specifications for wrought steels for automobiles.

This Report, which may now, for several reasons, be considered out of date, served a very useful purpose in drawing attention to the great advantage to steel users and steel makers of a short list of standard special steels, but, more important still, served as a schedule of typical automobile steels upon which useful research work might be concentrated.

During the summer of 1915 Conferences took place between the Technical, Standardisation, and Research Committees of the Society of Motor Manufacturers and Traders, and the Research and Steels Committees of the Institution of Automobile Engineers, and this resulted in the formation, in October, 1915, of a Joint Steel Research Committee.

It had previously been decided that, of the various matters investigation of which would assist the automobile industry, immediate needs would best be served by an investigation conducted to establish the physical properties of the B.E.S.A. Standard Automobile Steels. The Joint Committee was, therefore, eventually enlarged to include representatives of various interested bodies and proceeded to action immediately the B.E.S.A. Report No. 75 was issued.

The main Committee first appointed a small sub-committee to elaborate in rather more detail an outlined scheme of investigation

* For names, see Proc. I.A.E., Vol. IX., p. 430.

originally submitted by the Sheffield members of the original Steels Committee of the Institution, and finally handed over the entire conduct of the research, including the preparation of the report, to an Executive Sub-Committee composed of the following members:—

Brigadier-General R. K. Bagnall-Wild, C.B., C.M.G.
Mr. Hy. Brearley.
Mr. J. H. S. Dickenson.
Mr. A. A. Remington, O.B.E.
Mr. Wortley Fawcett.
Major H. P. Philpot. } Co-opted after the formation of the
Capt. E. W. Birch. } Sub-Committee.

Mr. A. A. Remington has been Chairman of the Main Joint Committee throughout the progress of the work, and also acted as Chairman of the Executive Sub-Committee up to the completion of the actual testing, after which, and during the tabulation of the results and the preparation of the Report, the author has had that honour.

The final publication of the Report in October, 1920, completes a task which all the members of the Sub-Committee have found to be one of considerable magnitude. The weight of steel presented to the Committee, much of which has been used, was over 14 tons. The number of tensile test pieces actually broken is well over 800, while about 2,500 Izod fractures, and nearly as many Charpy tests have been made, to say nothing of the Brinell tests. The preparation of the test bars, their allocation to the various investigators, and the collection of the results, has required very careful organisation, and for the flawless performance of this part of the work the Committee has been much indebted to Captain E. W. Birch.

Finally, the compilation of the results, and more particularly the preparation of the diagrams, has been greatly complicated by the not infrequent, and sometimes very remarkable, contradictions and discrepancies in the test results returned by the investigators. It would have been a simple matter merely to publish, without comment, the results as received, but the Committee conceived it to be their duty to assist those who in design offices, works test houses, and heat-treatment departments, will wish to make use of the Report, by the addition of representative test results, and, more important still, of smoothed curve diagrams illustrating the

variations of mechanical properties in the many steels and series investigated.

Notes on the Report.

Coming now to some discussion of the Report the author would wish to make it perfectly clear that the Report itself is, in every sense, the joint production of all the members with whom the author had the happiness to serve on the Executive Sub-Committee, but that he necessarily takes full responsibility for the present comments.

General Scheme of the Investigation.

The ten standard steels of Report No. 75 may be divided into two main groups, the first including two carbon steels, differing in carbon content, a 2 per cent nickel steel, and a 5 per cent nickel steel, all of which are primarily intended to be used for case-hardened parts, and the second comprising two carbon steels, one plain nickel steel, and three different types of nickel chromium steel (one of them of the air hardening type), which are ordinarily used either as normalised, or after some form of hardening with, or without, subsequent tempering. In order to establish the mechanical properties obtainable by varied heat-treatment from each of the ten standard steels, of which the limits of composition were laid down by Report No. 75, it was necessary to secure two sufficient supplies of material, one approximating to the lower, and the other to the higher, limits of composition, so that actually twenty different compositions of steel had to be obtained.

Most of the principal manufacturers of special steels were approached, and a very large number of offers of ingots were made to the Committee, affording a wide range of choice. If this did not, in all cases, provide samples precisely to the specified composition, very fair practical approximation was attained, Table I. giving the analyses of the twenty steels finally accepted. Three separate analyses were made on each steel, one by the steelmaker, and the others, of ingot and billet drillings respectively, by two independent analysts. Their results, on which the figures given in Table I. are based, are set out in the Report, and furnish an interesting comparison.

To eliminate, as far as possible, the influence of axial unsoundness on the test results, the firms supplying steel* were asked to

* The steel was supplied, sometimes in considerable quantities, quite free of charge to the Committee.

TABLE I.

CLASS OF STEEL.		Mark.	ANALYSIS.						
			C.	Si.	Mn.	S.	P.	Ni.	Cr.
10 Carbon Case-hardening Steel	High..	1H	0·17	0·24	0·72	0·041	0·047	..	..
	Low ..	1L	0·06	0·01	0·25	0·010	0·050	..	..
15 Carbon Case-hardening Steel	High..	2H	0·20	0·11	0·99	0·066	0·049	..	..
	Low ..	2L	0·12	0·05	0·71	0·080	0·048	..	..
2 per cent Nickel Case-hardening Steel....	High..	3H	0·15	0·05	0·61	0·017	0 012	2·60	..
	Low ..	3L	0·12	0·07	0·32	0·025	0·014	2·40	0·10
5 per cent Nickel Case-hardening Steel	High..	4H	0·17	0·39	0·34	0·014	0·014	6 00	..
	Low ..	4L	0·07	0·06	0·18	0·020	0·008	5·10	..
20 Carbon Steel	High..	5H	0·26	0·30	0·75	0·044	0·050	0·15	0·04
	Low ..	5L	0·13	0·06	0·41	0·025	0·036	0·20	0·08
35 Carbon Steel	High..	6H	0·45	0·32	0·78	0 020	0·025	..	..
	Low .	6L	0·26	0·20	0·50	0·060	0·065	0·03	..
3 per cent Nickel Steel	High..	7H	0·37	0·17	0·65	0 020	0·030	3 65	..
	Low ..	7L	0·17	0·05	0·34	0 033	0·023	2·90	0·20
1½ per cent Nickel Chromium Steel.....	High..	8H	0·38	0·33	0·60	0·045	0 039	1·72	1·65
	Low ..	8L	0·28	0·08	0·40	0·020	0·018	1·52	0·80
3 per cent Nickel Chromium Steel	High..	9H	0·31	0·14	0·70	0 027	0·030	3·27	0·82
	Low ..	9L	0·17	0·15	0·37	0·022	0·012	2·95	0·45
Air Hardening Nickel Chromium Steel	High..	10L	0·32	0·12	0·46	0·017	0·014	3·70	1·42
	Low ..	10LL	0·27	0·16	0·49	0·040	0·040	3·45	0·95

send forward for cogging and rolling only the lower halves of ingots from the selected casts, and, of course, the ingot drillings were taken from this part. This rather expensive expedient has been very successful, and very few test figures are in any way unreliable as a consequence of unsoundness, a matter of great importance in such an investigation as the present.

The programme of heat-treatment and testing for each steel naturally varied both in extent and character according to the characteristics of the material and the purposes for which it is ordinarily used.

In all cases most of the work has been carried out on bars $1\frac{1}{8}$ in. dia. at the time of heat-treatment, this size of test bar having been selected as a standard because of its general convenience for testing, and because such a mass forms a fair representation of a large proportion of automobile parts.

The tests in the normalised condition* of each steel and the numerous series to establish the effect of hardening in water, oil, or air, as well as that of tempering by reheating to various temperatures after such hardening, have been carried out on bars of this standard diameter.

Since, however, the mass of a forging at the time of heat-treatment has a very large influence upon the test results, most especially after hardening which naturally intensifies any hardness variation in different specimens, but also to a less extent after tempering, the programme for a number of the more important steels includes, in addition, selected heat-treatment and testing of bars greater and less in diameter than the standard, the sizes selected being $\frac{11}{16}$ in., $\frac{7}{8}$ in., $1\frac{1}{8}$ in., $2\frac{1}{8}$ in., and 3 in.

In the case of the case-hardening steels, which are generally used in the hardened condition, the effect of mass variation has been observed after simple hardening in water or oil, but in some of the steels, ordinarily used in a hardened and tempered state, series of bars of the various diameters have been hardened either in oil or water and then tempered at certain selected temperatures appropriate to the material.

It is not proposed, in the present paper, to set out the programme in full, or to discuss the results at all completely, there are far too many distinct series, Tables, and diagrams for this

* The heat-treatment terms used throughout the Report, and in this paper, are in accordance with the definitions laid down in B.E.S.A. Report No. 75.

to be possible, but rather to deal with a few selected and typical series of tests on certain steels. It is hoped in this manner to assist those to whom the Report as a whole might not be altogether intelligible, or who might not completely grasp the practical use to which the Tables of figures, and the diagrams, may be put.

The mechanical tests performed on the bars after heat-treatment included tensile tests, Izod impact tests, and in some cases Charpy tests, while Brinell tests have been made both upon the heat-treated bars and the test pieces machined from them. The Committee found it necessary to prepare a list of standard test pieces, details of which are published in the Report, and the author would most strongly urge all members of the Institution to use these test piece models, which have been found to give very satisfactory results. It should be added that in all cases the test pieces, whether for tensile or impact tests, have been machined axially along the heat-treated test bar, whatever the diameter.

The actual work of heat-treatment and mechanical testing has been performed by a considerable number of well-known investigators,* the procedure adopted by the Committee being to prepare three (sometimes four) complete sets of test bars for each of the series laid down in the programme, and to send these sets to different investigators for heat-treatment. It was usually arranged, with a few exceptions, that the heat-treatment and testing of any one series should not be performed by the same investigator, although in a number of cases the manufacturer of the steel also carried out a complete series of heat-treatment and tests to the Committee's programme, while the National Physical Laboratory stipulated that they should carry through completely any series sent to them.

For the greater part of the work, however, the principle of transfer was adopted. Thus, for example, a complete series of bars for heat-treatment on some given steel would be sent to The Sheffield Testing Works, afterwards being sent to Kirkaldys' for testing, while another similar series of the same steel might be heat-treated by Mr. S. N. Brayshaw or Dr. J. E. Stead, and then tested by The Sheffield Testing Works, and so on.

It was hoped that by carrying out a number of series of tests on each steel, and by mixing up the work of the different investi-

* The names are given in the Report.

gators in this way, the results obtained would represent as a whole those fairly to be expected in practice. Very marked discrepancies have, in fact, been obtained, which show quite clearly that, even in the hands of well-known investigators, the heat-treatment of a small bar of steel is not always accurately and precisely carried out,* but for evidence on this point the Report itself must be referred to.

It should be added that in the Report the identification of an investigator with a particular result is not published, but the confidential records in the possession of the Institution give precise details as to the maker of the steel and those responsible for the treatment and testing, etc., in the case of every test result.

It is next proposed to discuss, in some little detail, the results obtained in a few selected and typical cases.

The Programme of Investigation of, and some Results obtained from, B.E.S.A. 15 Carbon Case-hardening Steel.

Table II. sets out the composition limits specified for this steel in Report No. 75, and the analyses† given by the "high" and "low" steels obtained by the Committee for the purpose of ascertaining the mechanical properties of such material.

TABLE II.

	C.	Si.	Mn.	S.	P.
B.E.S.A. ..... High Limit..	0·20	0·20	1·00	0·07	0·07
B.E.S.A. ..... Low Limit ..	0·12	0·05	0·65	0·07	0·07
Steel 2H	0·20	0·11	0·99	0·066	0·049
Steel 2L	0·12	0·05	0·71	0·080	0·048

* It is much easier to measure precisely, to a degree or so, the temperature at the hot end of a thermo-couple than it is to heat a test bar evenly and uniformly to some desired temperature, at all events under conditions which in any degree represent those necessarily occurring in practice.

† These results are those considered by the Committee to be representative after consideration of the analyses returned by the maker of the steel, and by the two investigators who reported on the ingot and billet drillings.

These samples are in excellent agreement with the composition desired, better in fact than in the case of some of the other steels.

The programmes for the two steels 2H and 2L varied a little, but both provided that tensile, Brinell, Izod and Charpy tests should be carried out on $1\frac{1}{8}$ in. machined bars* subjected to the following heat-treatments:—

1. Normalised at 900° C.
2. Water quenched from 900° C.
3. Water quenched from 900° C. and tempered at 300° C.
4. ,, ,, ,, 400° C.
5. ,, ,, ,, 500° C.
6. ,, ,, ,, 600° C.
7. Oil quenched from 900° C.
8. ,, from 900° C. and tempered at 600° C.

In addition a series of bars $2\frac{1}{8}$ in., $1\frac{1}{2}$ in., $1\frac{1}{8}$ in., $\frac{7}{8}$ in., and $\frac{11}{16}$ in. dia. from each steel were to be water quenched from 900° C., then reheated to 760° C. and again quenched in water, after which tensile, Brinell, and Izod tests were taken from the core. In the case of the higher steel 2H it was also provided that two similar series of bars, ranging between $2\frac{1}{8}$ in. and $\frac{11}{16}$ in. in diameter, should be similarly doubly quenched in water from 900° C. and 760° C. and tested in the core, after cementation in the case of one series, and in the other after an initial heating for four hours at 900° C., but without cementation. This subsidiary experiment to ascertain whether the mechanical properties of the core after doubly water quenching are affected beneficially or harmfully by an initial soaking in either a cementing or a non-cementing atmosphere gave negative results, and will not be referred to again, but the evidence shows quite definitely that the mechanical properties of the core of case-hardening steels may be accurately ascertained by simple application of the regular hardening treatment to an uncemented bar without the necessity for the troublesome and expensive cementation, and subsequent grinding away of the hard skin, which was, at one time, prescribed in certain specifications.

Passing over the results obtained from the normalised, and oil hardened and tempered bars (the latter because such mild material

* These bars were rolled to $1\frac{1}{4}$ in. dia. and turned to $1\frac{1}{8}$ in. dia. before heat-treatment.

is seldom oil hardened), we proceed next to consider **a typical series to determine the effect of mass**, reproducing for the purpose the following Table from the Report:—

TABLE III (Table V., p. 5 of the Report).

Steel 2H.—Bars of varying mass, cemented, refined at 900° C., quenched in water, re-heated to 760° C., and quenched in water.*

Series.	Diameter of Bar in inches.	Tensile Tests.					Izod Tests.		
		Brinell on Test Piece.	Yield Point, tons per sq. in.	Maximum Stress, tons per sq. in.	Elongation per cent.	Reduction of Area per cent.	Energy Absorbed, ft.-lb.		
							1	2	3
A.	$\frac{11}{16}$	..	..	34·4	3·0	Nil.	51	44	40
B.		212	27·7	49·7	33·3	59·6	57	51	63
C.		..	27·3	55·4	25·0	44·5	90	79	56
X.				**52·0**	**28·0**	**52·0**		**59**	
A.	$\frac{7}{8}$	217	..	47·0	29·8	57·0	49	67	66
B.		207	31·6	45·1	32·0	61·6	57	61	63
C.		..	27·3	49.4	29·2	49·7	95	95	71
X.				**48·0**	**31·0**	**57·0**		**70**	
A.	$1\frac{1}{8}$	187	..	42·9	30·0	63·7	63	86	75
B.		207	25·6	43·6	35·5	63·6	75	83	92
C.		..	24·8	43·5	35·0	61·5	96	105	109
X.				**43·0**	**33·0**	**62·0**		**85**	
A.	$1\frac{1}{2}$	170		40·9	32·0	59·3	91	93	66
B.		192	25·7	39·9	35·5	68·0	91	79	71
C.		..	24·5	41·0	36·0	66·2	103	94	110
X.				**40·0**	**35·0**	**64·0**		**89**	
A.	$2\frac{1}{8}$	170	24·8	38·4	35·5	65·8	55	65	56
B.		179	23·6	37·5	32·5	59·2	84	83	73
C.		..	23·6	39·1	35·0	67·3	83	84	91
X.				**38·0**	**35·0**	**64·0**		**75**	

The A, B, and C series are the actual results obtained by different investigators, while the heavier (the "X") figures are those which, after careful consideration of the results, are considered by the Committee to represent the true mechanical properties of the material.

* The Brinell results on the Izod test pieces are given in the Report.

These "X," or representative, figures have not been prepared simply by averaging the results, but after the preparation of diagrams on which all the investigators' figures have been plotted, so that, by noting the general tendency of the plotted curves, obvious errors could be removed, and smaller discrepancies allowed for, by the production of smooth curves. Thus, for example, Fig. 1 shows all the "reduction of area" results in Table III., plotted against "diameter of bar," the smoothed curve actually adopted by the Committee being also shown. In this way, not only were the final "X" figures prepared, but curves such as Fig. 2* were produced, the latter corresponding to Table III. Examination of the Table will show that in this case the principal difficulty has been to deduce definite "X" figures for the Izod

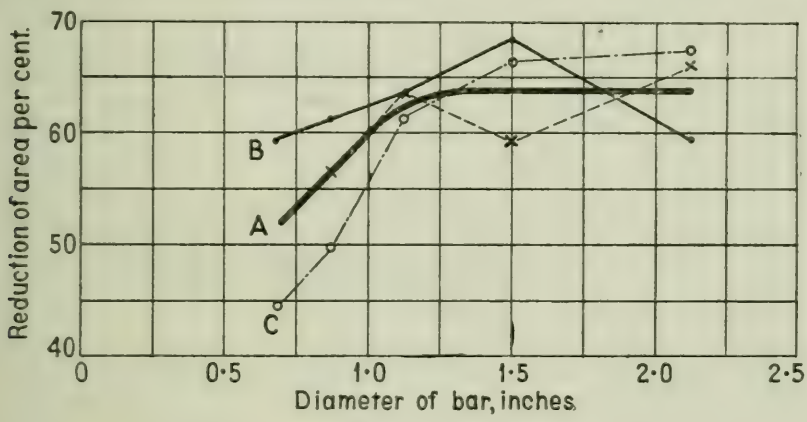


FIG 1.

test from the investigators' somewhat conflicting results. It will be found, however, that whether the nine Izod results for each size of bar are averaged, or whether all the results are plotted as on Fig. 1, the direction is first upwards to a maximum, and then downwards, as the mass increases, this being shown by the Izod curve of Fig. 2. On the other hand, the maximum stress falls, while the reduction of area and elongation rise, at a diminishing rate with increasing mass, the flattening of the curves indicating that an increase of mass beyond the limits actually tested would have little further influence on these test figures.

It will also be seen that as the mass diminishes below that of

* Reduced from the Report, where distinctive colours are used for the different curves.

$\frac{11}{16}$ in. round bars, the ductility, as represented by the reduction of area and the elongation per cent, and the toughness, as represented by the Izod test, might be expected to fall increasingly rapidly while the maximum stress would rise.*

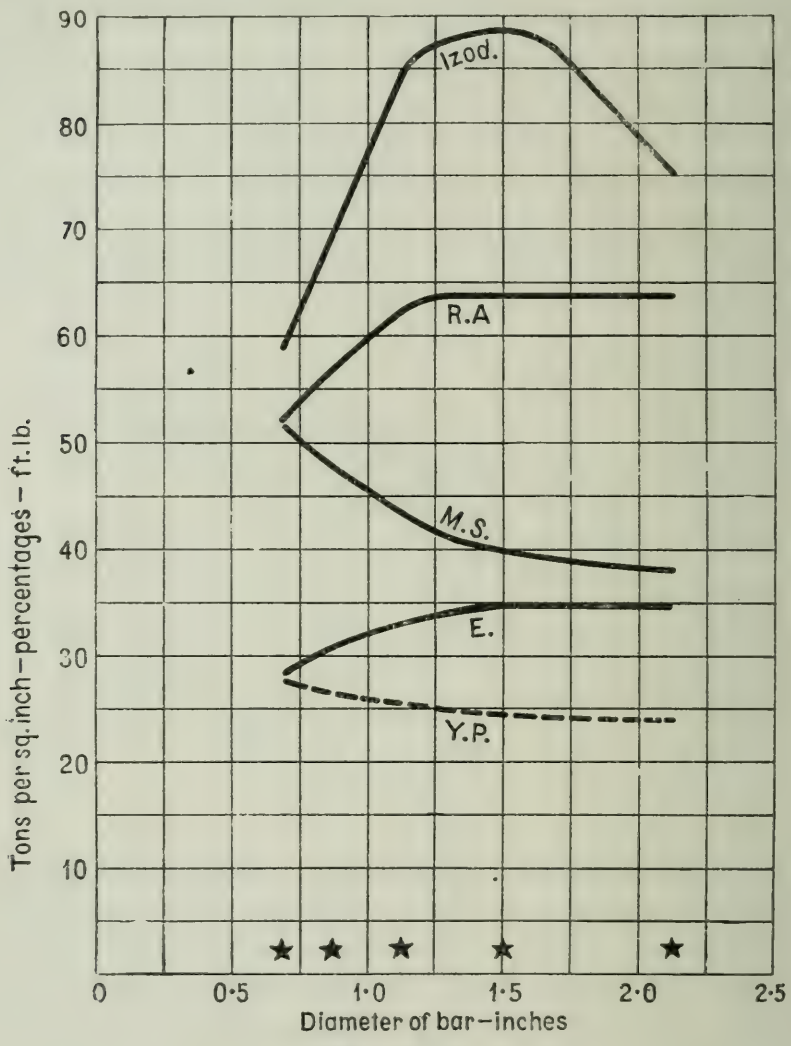


FIG. 2.

The author would also suggest, although it is perhaps outside the province of these notes to deal with such points, that the

* There are, of course, limits to what is permissible in such deductions, but it is indicated by these curves that if the cooling produced by quenching were instantaneous, as would be the case if the mass were infinitely small, the resulting hardness would be very considerable.

maximum in the Izod curve marks a mass above which water quenching is no longer able to produce a sufficient cooling rate to prevent separation of free unentangled ferrite from the martensite, while concurrently the grain size tends to increase as the cooling velocity diminishes. Both these causes tend to reduce

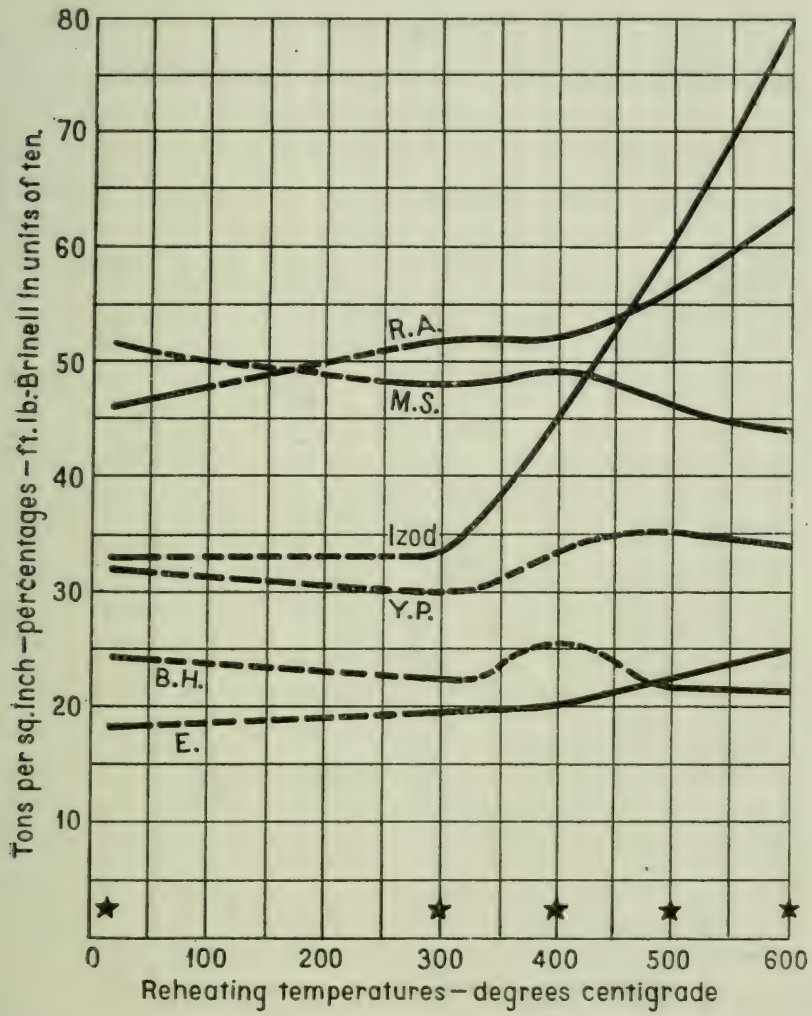


FIG. 3.

the toughness without the hardness as shown by the maximum stress being necessarily affected.

A precisely similar series of tests* carried out on the low limit steel 2L produces a very similar set of curves, the Izod results giving

* Except that the bars of the low limit steel were not initially carbonised.

the same shape of curve with a maximum at about $1\frac{1}{8}$ in. diameter. The immense practical value of comparing such diagrams from different steels in order to ascertain the mechanical properties which may be obtained after water quenching case-hardened parts of varying size, and composition, should need no emphasis. As

TABLE IV.

2H.—Bars $1\frac{1}{8}$ in. dia. Water quenched from 900° C., and tempered as stated.

Series.	Re-heating Temperature.	Tensile Tests.					Izod Tests.		
		Brinell on Test Piece.	Yield Point, tons per sq. in.	Maximum Stress, tons per sq. in.	Elongation per cent.	Reduction of Area per cent.	Energy Absorbed, ft.-lb.		
							1	2	3
A.	Not Tempered.	255	30·3	53·6	15·0	36·6	32	31	36
B.		228	37·2	50·8	18·5	47·2	35	34	36
C.		..	30·8	51·0	20·7	46·3	27	33	32
X.		**241**		**52·0**	**18·0**	**46·0**		**33**	
A.	300° C.	223	24·9	47·6	18·5	52·0	30	31	20
B.		217	33·1	47·2	19·0	52·4	42	42	46
C.		..	31·4	48·6	21·5	50·9	31	27	25
X.		**223**		**48·0**	**20·0**	**52·0**		**33**	
A.	400° C.	248	24·0	51·2	21·5	45·8	45	37	42
B.		269	40·6	52·0	19·0	49·6	48	55	79
C.		..	33·8	47·6	21·0	54·0	32	35	32
X.				**49·0**	**20·0**	**52·0**		**45**	
A.	500° C.	217	37·3	47·9	22·0	54·4	68	62	67
B.		217	32·4	43·6	23·5	61·6	62	67	72
C.		..	35·3	46·3	24·0	55·8	51	41	62
X.		**217**	**35·0**	**46·0**	**23·0**	**57·0**		**61**	
A.	600° C.	202	34·5	45·2	26·5	61·5	82	94	85
B.		228	34·4	44·9	23·0	63·6	78	61	88
C.		..	33·1	43·9	27·0	63·6	77	79	74
X.		**212**	**34·0**	**44·0**	**25·0**	**63·0**		**79**	

an example, however, it may be pointed out that the 2H and 2L curves, when compared with the similar diagrams of the nickel case-hardening steels, illustrate very forcibly the advantages of using alloy steels in case-hardened parts for many purposes, as well as the very high results which may be obtained from carbon

case-hardening steels when the mass is relatively small. Thus it is indicated that the high limit 6 per cent nickel case-hardening steel of $2\frac{1}{2}$ in. diameter, and the high limit 15 carbon case-hardening steel of about 0·5 in. diameter, give approximately the same maximum stress and Izod test result when quenched in cold water.

We may now pass on to consider another type of series, selecting the **water hardened and tempered series of the high limit carbon case-hardening steel 2H,** the actual results being reproduced from page 4 of the Report in Table IV.

The author has particularly selected this Table of test results, and the corresponding diagram (Fig. 3), not because they illustrate at all well some especially useful features of a tempering diagram, better examples of which will be given later, but because they reveal a very peculiar abnormality which has previously attracted the attention of several members of the Executive Sub-Committee, but which appears to be not very generally known, and has certainly not been explained. By this is meant the "hump" in the maximum stress and Brinell hardness curves at 400° C. which, so far as the author knows, has never been found in tempering series of the harder steels, these always showing a continuous fall in hardness as the re-heating temperature rises. It will be noticed that the results of two investigators indicate quite distinctly a rise in maximum stress and in Brinell hardness at 400°, although those of a third investigator do not. A similarly elusive rise in hardness on tempering quenched mild steels at 400° C. has been previously found by several Sheffield metallurgists, and it is clear that this abnormality deserves more investigation than the little which its practical unimportance has, so far, secured for it. It may be observed in passing that Heyn showed, some years ago, that hardened and tempered steels show a maximum solubility in acids at about this temperature.

For the rest this water hardened and tempered series requires little further attention, beyond noting the irregularity of the yield stress curve at the lower tempering ranges, a matter which will be discussed in more detail when dealing with the corresponding diagrams of harder steels.

Some Discussion of the Results obtained from the B.E.S.A. 35 Carbon Steel.

The plain carbon steels, ranging between about 0·25 per cent to 0·45 per cent carbon, are of very general utility, and the

programme of experimental work laid down for this material was in consequence of considerable extent.

The limits of composition for "35 Carbon Steel" specified in B.E.S.A. Report No. 75, and the analyses of the two corresponding high and low limit steels, 6H and 6L, obtained by the Committee, are given in Table V. It will be noted that 6H is distinctly higher, and 6L distinctly lower in carbon than was

TABLE V.

	C.	Si.	Mn.	S.	P.	Ni.
B.E.S.A. ..... High Limit..	0·40	0 30	0·85	0·06	0·06	..
B.E.S.A. ..... Low Limit ..	0·30	0·05	0·50	0·06	0·06	..
Steel 6H ..... (High)..	0·45	0·32	0·78	0·02	0·025	Nil.
Steel 6L ..... (Low) ..	0·26	0·20	0·50	0·06	0·065	0·03

desired, so that the carbon range covered is rather unduly wide, but it was impossible, at the time, to get more suitable samples, and if the Research Committee does any further work, it might very conveniently proceed to determine the mechanical properties of an intermediate steel containing, say, 0·35 per cent carbon.

The programmes for the high and low limit steels 6H and 6L were alike in prescribing that tests should be carried out on standard $1\frac{1}{8}$ in. diameter bars subjected to the following heat-treatments:—

1. Normalise at 870° C.
*2. Water quench from 870° C.
3. Water quench from 870° C. and temper at 300° C.
4. ,, ,, ,, 400° C.
*5. ,, ,, ,, 500° C.
*6. ,, ,, ,, 600° C.
7. ,, ,, ,, 700° C.
*8. Oil quench from 870° C.
9. Oil quench from 870° C. and temper at 300° C.
10. ,, ,, ,, 400° C.
*11. ,, ,, ,, 500° C.
*12. ,, ,, ,, 600° C.
13. ,, ,, ,, 700° C.

It was also provided that, on the high limit steel only, additional tests to determine the effect of mass should be made upon bars 3 in., 2½ in., and 1⅛ in. dia. respectively subjected to each of the heat-treatments marked with an asterisk in the above Table. The tabulated test results obtained from steels 6H and 6L occupy eight foolscap pages of the Report, and divide into quite a number of distinct series, of which only a selected few can receive attention here. In dealing with these and most subsequent series, only the "X" figures will be given in any Tables of test results adapted from the Report, to which reference must be made for the actual test results from which these values are derived.

TABLE VI.

6H.—Bars 1⅛ in. dia. Water quenched from 870° C., and tempered as stated.*

Re-heating Temperature.	Tensile Tests.					Izod Tests, ft.-lb.	Charpy Tests, kg.-m.
	Brinell on Test Piece.	Yield Point, tons per sq. in.	Maximum Stress, tons per sq. in.	Elongation per cent.	Reduction of Area per cent.		
Not Tempered ..	321	..	67	12	28	14	2·0
300° C.	311	..	65	15	38	16	2·5
400° C.	302	46	64	17	47	19	3·0
500° C.	277	42	59	21	55	34	3·6
600° C.	235	36	52	25	62	48	5·2
700° C.	207	31	45	28	67	59	5·9

An interesting comparison is that of the results obtained from the **water and oil treated series of the same steel 6H**, shown in Tables VI. and VII.

It is shown by the Tables, and better still by diagrams prepared from them, that, in both series, the effect of increasing the re-heating temperature is to reduce the yield and maximum stress, and raise the elongation and reduction of area, slowly at first and then more rapidly on reaching a certain temperature, which is lower in the initially harder, and less stable, water quenched

* Adapted from Table III., p. 27 of the Report

TABLE VII.

6H.—Bars $1\frac{1}{8}$ in. dia. Oil quenched from 870° C., and tempered as stated.*

Re-heating Temperature.	Tensile Tests.					Izod Tests, ft.-lb.	Charpy Tests, kg.-m.
	Brinell on Test Piece.	Yield Point, tons per sq. in.	Maximum Stress, tons per sq. in.	Elongation per cent.	Reduction of Area per cent.		
Not Tempered ..	..	..	56	20	49	27	3·3
300° C.	262	..	56	21	51	23	2·9
400° C.	255	37	55	21	52	27	..
500° C.	248	36	55	21	54	32	3·6
600° C.	223	32	50	24	59	39	4·4
700° C.	207	28	45	27	65	44	5·0

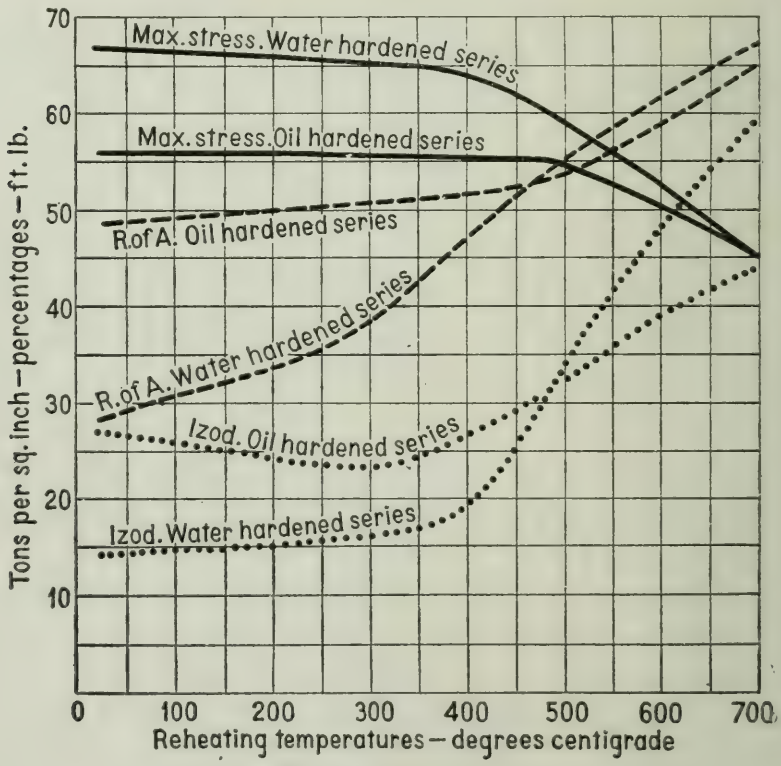


FIG. 4.

* Adapted from Table IV., p. 27 of the Report.

steel. It will moreover be found that the above mentioned test values, which are all widely apart in the untempered oil quenched and water quenched specimens, tend to converge as the re-heating temperature rises, so that at 700° C., when the critical range is practically reached, the hardness and ductility variations produced by the different cooling velocities of water and oil quenching are extinguished, as indeed would be expected. This is illustrated by Fig. 4, which compares the maximum stress and reduction of area curves of the two corresponding diagrams in the Report.

The toughness is, however, somewhat differently affected since the two Izod curves, which are also reproduced, do not converge to meet at 700° C., but actually cross at 500° C., so that water quenched bars tempered above this temperature give decidedly higher impact test figures than the corresponding oil quenched bars, along with very similar, indeed slightly greater, maximum stress. This is a fact of considerable practical importance, of which considerable use is actually made by those who have previously worked out such comparisons for themselves. While the convergence of the yield, maximum stress and ductility curves may be simply explained by assuming the regular and progressive breakdown and softening of the hard martensite, which is less stable and therefore more easily decomposed in the water quenched bars, besides there being more of it, the difference in the Izod curves would appear to be due to some more deep-seated and obscure alteration in the structure, or constitution, of the resulting sorbite. Such theoretical questions arising, at many points, out of the Report will, it is hoped, suggest much future research work, but perhaps enough has been said to show the practical usefulness to the steel user of ascertaining by means of such diagrams as these the possible **variations in the mechanical properties of any given material which may be produced by hardening in different quenching media and then tempering.**

We pass on next to compare two diagrams showing similarly the range of mechanical properties obtainable from **two different grades of the same type of steel when hardened in the same quenching medium and tempered,** selecting for this purpose the water quenched and tempered series of the lower limit steel 6L, the deduced test results of which are shown in Table VIII.,*

* Adapted from Table III. on p. 33 of the Report.

for comparison with the corresponding water quenched series of the higher limit steel, 6H, already dealt with (Table VI.).

Fig. 5 has been produced by extracting the maximum stress, elongation per cent, and Izod curves from the two diagrams (in the Report) corresponding to Tables VI. and VIII.

In this case it is seen that although the maximum stress and elongation per cent curves for the two steels are of much the same shape and tend to approach more closely as the re-heating temperature rises, they do not actually meet at 700° C., as happens when bars of the *same steel* have been quenched *in different media*. As this is practically within the critical range where tempering,

TABLE VIII.

6L.—Bars 1⅛ in. dia. Water quenched from 870° C., and tempered as stated.

Re-heating Temperature.	Tensile Tests.					Izod Tests, ft.-lb.	Charpy Tests, kg.-m.
	Brinell on Test Piece.	Yield Point, tons per sq. in.	Maximum Stress, tons per sq. in.	Elongation per cent.	Reduction of Area per cent.		
Not Tempered ..	196	..	44	24	56	16	2·5
300° C.	192	..	43	24	57	14	2·5
400° C.	187	..	44	25	58	16	2·6
500° C.	183	32	42	27	60	19	2·9
600° C.	179	29	40	29	64	22	3·2
700° C.	167	26	37	33	68	26	3·7

properly so called, comes to an end, the difference in results obtained at 700° C. is a natural or intrinsic variation in the mechanical properties of the two steels, the divergence towards the left being the intensification of this difference produced by water hardening. The yield point and Brinell hardness curves are very similar in character, but it will be noticed on reference to Tables VI. and VIII. (and the diagrams in the Report) that the reduction of area curves do converge to meet at 700° C., while the Izod curve for the harder steel is actually much higher in the upper tempering ranges than that of the milder material. This superior toughness of the harder of such a pair of carbon steels is,

in fact, often met with in practice, but in the present case the difference is so very marked as to suggest that the better values given by the 0·45 per cent carbon steel are due to some deeper underlying cause, such as the original structural characteristics of the ingot, or to the conditions under which it has been wrought, rather than to the simple effect of heat-treatment in relation to chemical composition. These latter remarks may, perhaps, serve to indicate, without over-emphasising, the fact that the correct

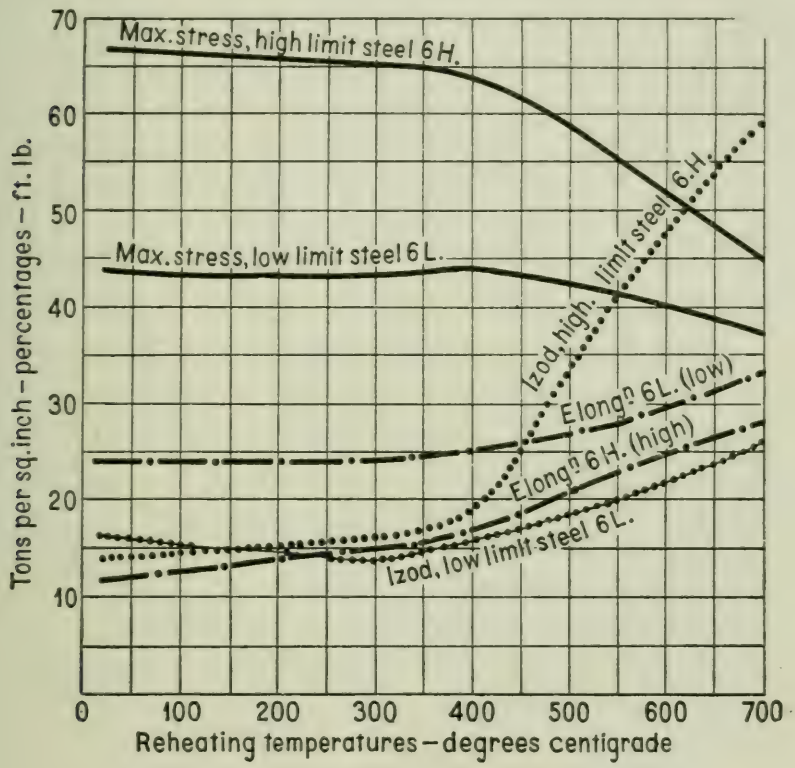


Fig. 5.

interpretation of these results and diagrams is not invariably quite so simple as it looks. In this connection it is interesting to compare the corresponding tempering diagram of 2H, which gives very much better Izod results, although in composition, and in the other test results, there is no very great difference from the steel 6L.

It will be observed that the maximum stress curve in Fig. 5 shows a slight hump at 400° C., such as was much more definitely shown in the water quenched and tempered series of steel 2H.

In the present case, however, the rise is within the limits of error of testing.

TABLE IX.

6H.—Bars of varying mass. Water quenched from 870° C., and tempered at 600° C.

Diameter of Bar, inches.	Tensile Test.					Izod Test, ft.-lb.
	Brinell on Test Piece.	Yield Point, tons per sq. in.	Maximum Stress, tons per sq. in.	Elongation per cent.	Reduction of Area per cent.	
$\frac{11}{16}$	255	44	55	23	60	76
$1\frac{1}{8}$	235	36	52	25	62	48
$2\frac{1}{8}$	217	31	49	26	60	44
3	212	29	48	26	59	44

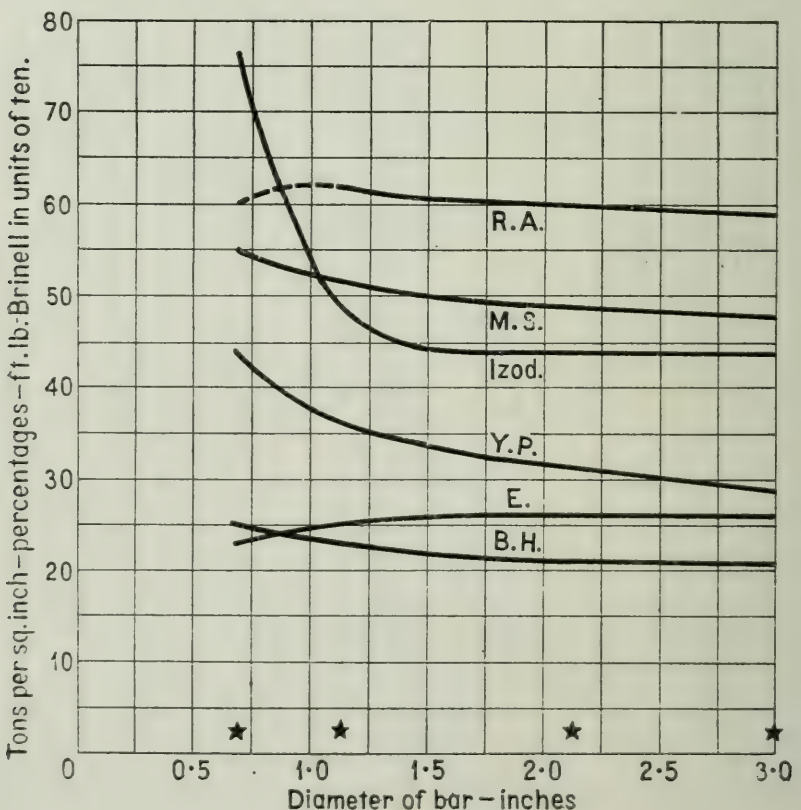


Fig. 6.

We come now to consider a further type of diagram, of which there are a number in the Report, that **illustrating the effect of mass when a series of bars of varying diameter are subjected to a definite hardening and tempering treatment.**

Table IX.* gives the "X" results obtained from a series of bars of the high limit steel 6H after water quenching from 870° C. and tempering at 600° C. in each case.

The corresponding diagram, Fig. 6, having "test results" and "diameter of bar" as co-ordinates, is reproduced on a smaller scale, and without the advantage of coloured curves, from the Report.

Apart from a troublesome little anomaly in the reduction of area curve, probably due to secondary causes, the curves are quite simple in form. They show, indeed, much the same general tendency as the water quenched mass series of the carbon case-hardening steel previously dealt with (Table III.), the yield and maximum stress falling, and the ductility values rising, at a diminishing rate as the mass increases. It will be noticed, however, that the Izod curve takes a totally different direction and indicates very positively that with this material a rapid quenching rate when hardening, followed, of course, by a sufficiently high tempering, is the way to get toughness, the micrographical object being to get the closest possible association of iron carbide and ferrite in the tempered steel. It is possible that this Izod curve would reach a maximum, and then fall, further towards the left, but in this connection the corresponding Izod curves of the other water quenched mass series of steel 6H, untempered, and tempered at 500° C., should be studied, as also the oil quenched and tempered mass curves on the same steel. The value of this set of Tables when considering the probable variation in the physical properties produced in, say, different parts of a drop forging of irregular shape, by different forms of heat-treatment should be obvious.

Some Discussion of the Results obtained from the B.E.S.A. 3 per cent Nickel Chromium Steel 9H.

The limited compass of the present paper permits the examination of only one more typical series, that **to establish the effect of progressively tempering oil hardened bars of a**

* These "X" figures are taken from Table VI., p. 29 of the Report.

typical nickel chromium steel, this being the high limit "3 per cent nickel chromium steel" 9H having the analysis:—

Carbon	0·31 per cent
Silicon	0·14 per cent.
Manganese	0·70 „
Sulphur	0·027 per cent.
Phosphorus	0·030 „
Nickel	3·27 „
Chromium	0·82 „

The author thinks that the great importance of this steel, and series, and some extremely interesting features in the results, well brought out by the corresponding diagram, Fig. 7, justifies, in this case, the reproduction from the Report of all the investigators' figures upon which the curves are based.

The generally close agreement in the three investigators' results made the preparation of the tempering diagram, Fig. 7, and the establishing of "X" figures, a much less perplexing problem than in a number of similar series where the results were much more erratic.

This evenness of result is, in fact, a characteristic and very valuable feature of those alloy steels, and particularly of nickel chromium steels which harden more or less intensively, or which, differently expressed, have a sufficiently low critical cooling velocity to ensure that on quenching parts of irregular shape, or considerable sectional area, the hardening is fairly uniform throughout.

The relative behaviour, in this respect, of the various steels investigated may be gauged by the "mass series,"* when these have been carried out, and also to some extent by the difference between the Brinell hardness test taken on the surface of the test bar, and that afterwards taken further in on the machined test piece.

Turning now to the diagram, Fig. 7, we find that it resembles in several respects the tempering diagrams of the 0·45 carbon steel previously discussed. Thus the maximum stress and Brinell

* For example, the $\frac{11}{16}$ in. bar of 35 carbon steel 6H after oil hardening and tempering at 600° C. gives a maximum stress 6 tons per sq. in. higher than that of the similarly treated 3 in. bar of the same series. The two corresponding bars of the 3 per cent nickel chromium steel 9H show a difference of only 2 tons per sq. in.

Table X.

9H.—Bars 1⅛ in. dia. Oil hardened at 820° C., and tempered as stated.*

Series.	Reheating Temperature.	Tensile Tests.					Izod Tests.		
		Brinell on Test Piece.	Yield Point, tons per sq. in.	Maximum Stress, tons per sq. in.	Elongation per cent.	Reduction of Area per cent.	Energy Absorbed, ft.-lb.		
							1	2	3
A.	Not Tempered.	477	93·2	113·7	13·5	39·7	11	13	11
B.		514	65·2	119·1	14·5	36·8	9	14	4
C.		..	..	115·8	12·5	34·9	11	14	..
X.		**495**		**116·0**	**14·0**	**37·0**		**11**	
A.	200° C.	460	84·6	106·1	14·0	52·8	14	15	15
B.		477	74·6	110·1	15·5	47·3	17	17	18
C.		..	..	110·6	16·0	53·4	25	25	30
X.		**469**		**107·0**	**14·5**	**50·0**		**20**	
A.	300° C.	418	83·3	95·2	14·0	50·0	4	4	4
B.		444	78·3	98·1	15·0	53·4	3	3	2
C.		..	78·4	96·8	15·0	55·2	5	5	5
X.		**436**		**97·0**	**14·5**	**53·0**		**4**	
A.	400° C.	364	77·0	82·5	15·5	52·8	8	8	8
B.		415	76·4	91·2	16·0	54·8	2	4	3
C.		375	75·0	85·4	15·5	55·8	8	11	14
X.		**388**	**75·0**	**85·0**	**16·0**	**54·0**		**8**	
A.	500° C.	321	66·7	70·8	18·0	55·0	35	30	33
B.		352	68·0	73·3	17·5	57·1	22	24	24
C.		321	62·0	68·5	20·0	56·9	31	26	27
X.		**331**	**65·0**	**71·0**	**19·0**	**56·0**		**28**	
A.	600° C.	286	59·0	62·1	22·0	63·7	59	58	60
B.		293	54·0	60·7	22·5	60·4	59	58	53
C.		269	52·0	59·3	22·5	62·6	60	60	55
X.		**285**	**54·0**	**60·0**	**22·5**	**62·0**		**59**	
A.	650° C.	269	50·4	57·9	24·0	63·9	68	68	68
B.		269	49·7	57·2	23·5	65·7	83	86	87
C.		269	45·6	55·1	25·0	66·3	84	82	80
X.		**269**	**48·0**	**57·0**	**24·0**	**65·0**		**74**	
A.	700° C.	286	49·4	68·8	12·0	25·0	32	30	30
B.		255	44·3	54·9	25·0	65·9	82	83	79
C.		241	34·0	58·9	23·0	47·8	51	51	51
X.									
A.	725° C.	460	86·0	112·8	5·5	11·6	18	10	13
B.		269	33·0	60·7	20·0	48·3	47	46	47
C.		388	..	84·2	10·0	15·0	12	12	12
X.									

* Adapted from Table III., p. 47 of the Report.

hardness number fall, while the elongation increases, in graceful and unbroken curves as the re-heating temperature rises until

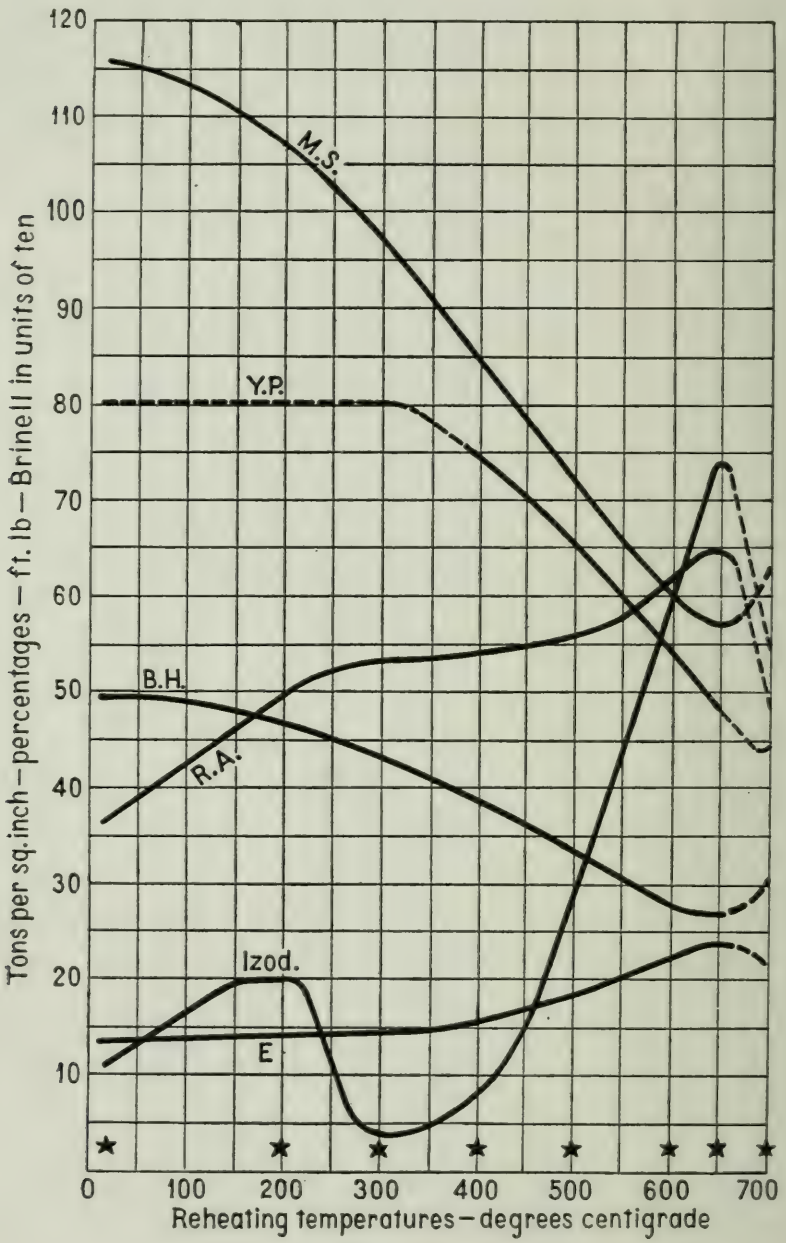


FIG. 7.

the critical range is entered, in this case at about 650° C., when changes occur which we may consider later. The rate of fall (or rise) is greater than is shown by the corresponding curves of the

milder steels simply because the initial hardness is so much greater, but there is no real difference in character. The Izod impact test, yield stress, and reduction of area curves are not so simple, and their irregularities need careful attention because of their great practical importance, if for nothing else.

The form of the Izod curve has been repeatedly confirmed by several of the author's friends, as well as by himself. He believes that several investigators discovered quite independently the existence, in similar series, of the rise and fall between 200° C. and 300° C., but it is now agreed that it does not occur in all samples of nickel chromium steel to anything like an equal degree. Apparently conditions apart from the ordinary chemical composition, such as the method of steel-making, of casting, and so on, play a part, but generally it is sound practice to avoid tempering anywhere between 250° and 450° C.

The reasons why about 600° C. should be generally and properly prescribed as the tempering heat for structural parts requiring machining may be left for a moment, but the practical advantage of tempering oil hardened (or, when the composition is suitable, of air hardened) nickel chromium steel at 200° C., when very considerable hardness is desired along with the greatest possible toughness, is made obvious by this diagram. In the present case the impact test figure is actually doubled by this treatment at the price of less than 10 tons of the maximum stress, which still remains, with the relatively excellent Izod result of 20 ft.-lb., at 107 tons per sq. in.

The shape of the yield stress curve is also a matter of considerable importance. This curve is roughly parallel to that of the maximum stress in the upper tempering ranges, but below about 400° C. indicates a continuously diminishing yield ratio as the re-heating temperature is lowered. In quite a number of similar tempering series carried out by the author the anomaly in the yield stress curve is still more striking, since it actually *rises* to a maximum at 200° or 300° C., then gradually falling and becoming, at 400° or 500° C., approximately parallel to the maximum stress curve. The true explanation of the *apparently* low yield ratios given at the lower tempering ranges not only by nickel chromium steels, but by other steels which are considerably hardened by quenching in water, oil, or air, is very important when considering the ability of such materials to withstand alternating stresses. It is obvious that a steel having the high tensile

strength of, say, 100 tons per sq. in., and, for that hardness, the considerable toughness represented by an Izod impact test of 15-20 ft.-lb. would be of great practical value, given the same yield ratios as would be observed in a tensile test of the same material, reduced in hardness by tempering at, say, 600° C. The disconcertingly low yield points observed after low re-heating temperatures are, however, shown by another method of determining the true elastic limit (the stressed rotating beam method) to be due possibly to initial internal strain, possibly to the existence of some austenite in the hardened specimen, and possibly also to both acting together.

In the second case tempering would increase the yield ratio by the formation of martensite. As regards the other explanation, the author ventures to quote Mr. Harry Brearley, who says:—

"In the absence of any demonstrated explanation, it may be suggested that the yield stress observations are probably falsified by hardening stresses, released in the softer specimens at the higher re-heating temperatures, but not released in the harder specimens until, in the act of being pulled in the tensile machine, they are overcome, and leave the hitherto-compressed material free to elongate. Such elongation, consequent on the inversion or balancing of hardening stress, will influence the local extension diagram as though it were an actual plastic movement, but it would bear no relationship whatever to the structural deformation which produced the well-defined yield."

"The clearly-marked jog at the yield point in a local extension diagram of ordinary soft steel occurs in the softer specimens referred to in the previous paragraph, but it is absent from the diagram taken from the harder steels. It is also absent from the diagrams of those steels which have been re-heated within the critical range. For these reasons, the yield point or the proportionate yield stress quoted by different investigators have been found to vary widely."

It is a fact, supporting Mr. Brearley's view, that the yield stress results returned by different investigators for most of the hardened, or only slightly tempered, specimens, have revealed the most extraordinary discrepancies, the corresponding maximum stress figures being usually closely in agreement. As a consequence the Executive Sub-Committee abandoned any attempt to deduce yield stress "X" figures for any specimen tempered lower than 400° C., and although an attempt has been made to give

approximate yield stress curves in the diagrams, these are shown as broken lines below that temperature.

Since the yield stress observations are quite useless, and because in really high elastic limit steels the limit of proportionality may be passed (in the tensile test) long before the true elastic limit is reached, there is no direct commercial method for ascertaining the ability of such steels to resist fatigue producing stresses. It does appear, however, that, in any given steel brought by heat-treatment into a definite structural condition, there will be a fixed relationship between the maximum stress in tension and the safe range of stress, so that, having once determined this ratio under fixed conditions, it should be possible to deduce the fatigue range of other samples by noting the maximum stress and observing that a similar microstructure, as regards the manner of association of ferrite and cementite, has been produced by heat-treatment.

The reduction of area curve shows a depression between about 250° C. and 500° C. which roughly corresponds with that of the Izod test, and may result from the same cause, whatever that may be.

Finally, some reference should be made to the sharp change in direction of all the curves above 650° C., that is to say, when the critical range is reached. In this series, as throughout the whole programme, the tempered bars were cooled off in water, the object being to avoid complications arising from the "temper brittleness" which may under certain conditions be produced in nickel chromium steels by slowly cooling after tempering. The subject of "temper brittleness" has excited very much attention, especially during the past few years, on account of its very great practical importance, as well as its theoretical interest, but is much too large a subject to be dealt with here, especially as the existence of the phenomenon is not brought out in the Report. It should be carefully studied by all interested in the heat-treatment of alloy steels.

A similar procedure was adopted with the carbon and other steels, since quenching out after tempering could, at least, do no harm and has some practical advantages. In this case water cooling after tempering has accentuated the effect of reaching the critical range since hardening, with loss of ductility and toughness, commences the moment carbide begins to pass into solution, but depreciation of the mechanical properties would have occurred

with any method of cooling. The relationship of the yield stress and maximum stress curves in this part of the diagram is especially interesting, and the author has attempted to explain the characteristic divergence of these curves by imagining that, as the critical range is entered, free ferrite is still being disentangled in distinct areas from the sorbite, thus reducing the elastic limit and yield stress, while at the same time carbide is dissolving ferrite within the separated eutectoid, which increases the hardness on quenching. However, he has taken no steps to prove, or disprove, this.

Turning to practical considerations, the diagram as well as the suddenly erratic test results obtained at 700° C. and 725° C., prove sufficiently clearly what is, indeed, fairly well recognised, that tempering at, or immediately below, the critical range is a very undesirable proceeding when uniform mechanical properties, and especially a high yield ratio, are of importance.

The soundness of the general opinion that nickel chromium and other alloyed structural steels should be tempered at about 600° C. after hardening is upheld by this diagram. In such cases it is necessary to reduce the hardness sufficiently to make machining tolerably easy, securing at the same time the best possible combination of high resistance to fatigue and ductility and toughness. This diagram shows that at 600° C. the Izod test, the reduction of area and the elongation are approaching the maximum, while the maximum stress is high and the yield ratio does not show the fall which becomes very apparent 50° higher when the critical range is touched. This re-heating temperature is not invariably the best since, for example, high chromium steels having a raised critical range need to be tempered somewhat higher, while carbon steels may very conveniently be tempered somewhat lower. Nickel steels occupy a midway position, but reference to the Report will show that the 3 per cent nickel steel 7H needs to be tempered at not much less than, say, 550° C. in order to get the best possible combination of the different mechanical properties.

Conclusion.

In this very inadequate discussion of a few typical series of tests from the Report of the Steel Research Committee, the author has necessarily left unmentioned, not only most of the steels and series, but also many additional uses which may be made of the data. Among these omissions are such interesting comparisons as those of the maximum stress and Brinell hardness curves, which

may be made to give very reliable "Brinell factors," and of the Izod and Charpy test results which give, on the whole, striking similarity in their indications of relative brittleness.

This, however, was inevitable since an exhaustive discussion of the Report would produce a volume many times its size, and these somewhat casual and discursive notes are intended to be helpful rather by suggestion than by complete explanation.

It was desired to introduce a section discussing the relationship between mechanical testing and the actual conditions of service, but this subject, which the author has dealt with in some detail elsewhere,* does not admit of brief treatment. It must, however, be studied by anyone wishing to make the fullest use of the Steel Research Report. Still it is hoped that these notes may, to some small extent, assist users of "automobile" and special steels to determine and compare the mechanical properties of their materials, to judge their possibilities and limitations under varied conditions of treatment and mass, and to learn that in the interplay of different, and sometimes opposed, physical properties (such as, for example, hardness and toughness, both greatly prized), the increased possession of one must usually be paid for more or less by loss of another. However, such data as those in the Report, and especially the use of the diagrams, will enable the engineer, who knows what he wants, to obtain by selection of material and judicious heat-treatment the most advantageous combination of the properties he desires.

It is not for the author, in concluding these notes, to mention the many willing helpers who have contributed to the work recently placed at the service of the Automobile Engineers of this country. That has been dealt with by the Chairman of the Joint Committee, Mr. A. A. Remington. It is, however, possible and proper to record that this Institution and the industry, as well as the Research Committee, owe very much to Mr. Remington's vision, energy, and mastery of detail.

The author, who has taken the greatest pleasure in the work from the first meeting in 1915, feels that a research carried through as it has been with complete harmony and goodwill on the part of all associated with it, cannot fail to be of value to engineers, drop forgers, and steel makers alike.

* Dickenson, "The Mechanical Testing of Steel," Proceedings, Sheffield Soc. Engs. & Metallurgists, 1919, II.

THE DISCUSSION.

Sir Robert Hadfield, F.R.S., in opening the discussion, said: I think the work set forth in this Report is representative of the very wonderful advance made during the past twenty-five or thirty years in the manipulation of iron alloyed with other elements. I can remember the time when we knew very little about this subject, and it was at the Paris Exhibition of 1878 that I saw the first exhibit of alloy steel in the world of any great importance, by the French Terre Noire Company. We all owe a great debt of gratitude to our Ally, France, for the magnificent work she did in the early part of the history of the alloying of iron with other elements. On my return from that Exhibition, I was encouraged to go on with my own researches, which eventually culminated in the discovery of manganese steel, by the very able work there presented, and presented freely to the world.

In this present Report we have an exhibition of English courage and wisdom in not wishing to hide anything, even from our competitors and enemies. We have here the mechanical qualities and the history of production of these very important series of steels set forth. This, I claim, shows that we British steelmakers do not fear foreign competition.

Before discussing the paper, I should like to refer to the excellent work in connection with automobile steel production which was done during the war by a very capable Sheffield man, Mr. Bernard Firth, who was Chairman of the Alloys Steel Committee, and by our President, Sir Henry Fowler. He was very often down in Sheffield and other works fulfilling his special duties of the Production Branch of the Ministry of Munitions, and gave us much encouragement and assistance in the war when we were all feeling a little down-hearted. There is no one who did more to encourage the engineer and the steelmaker than our President.

I think it is rather a pity that Frémont shock tests were not included in the Report. There is one special advantage in this test, that is, we should have been able to compare our results more closely with those of our friends in France and elsewhere, because

there they largely use the Frémont test, which enables a higher speed of impact to be obtained with the falling weight or tup. For this reason I am confident a more accurate estimate of the quality of the steel that is being examined is obtained. Although the Izod test is, in a sense, a falling weight, it has not the same speed of impact. The Frémont has a speed of impact of 30 to 35 foot-seconds, whereas the Izod has only a speed of about 10 or 12. We are at present dealing with this matter in an important Committee inaugurated by the British Engineering Standards Association, and before long we expect to have presented several important papers on the subject. If the Research Committee care to have the Frémont tests done, I shall be very pleased indeed to promise to carry them out in the Frémont machine owned by my firm. The test pieces are only about $1\frac{1}{2}$ in. long and $\frac{3}{8}$ in. square, and could be made from the ends of the tensile bars used in the Research. I wish to congratulate Mr. Dickenson very heartily upon the Report and on the excellent curves given therein, which represent some of the best comparison curves I have ever seen. They are easily understood, and I wish all authors of papers would adopt this principle of thick coloured lines, which are so much easier to read than thin uncoloured ones. It would be a good thing, too, if they would adopt similar sets of colours to those chosen by the Research Sub-Committee in the future for the same type of observation, i.e., maximum stress, yield point, etc. With reference to the heating curves shown at the end of the Report, I would like to make the following suggestion, namely, that if any further copies of the Report are to be published, the analysis should be added alongside the curves.

There is one other point—which I am afraid it is too late to remedy—that on all the tests only yield points are given, but a good deal may happen to steel below its yield point. It would be a good thing, therefore, if we could also know the elastic limit of the different specimens. I am aware that there is a good deal of controversy as to what is meant by that term. Once a steel commences to elongate the structure of that steel is altered, and I think it will be by watching these minute differences that we shall in the future be able to understand the behaviour of steel and find why it has failed and why in other cases it has succeeded better. It may have rather a high yield point, but its real elastic limit may be very much lower than expected. There is no doubt that most steel which has been quenched and tempered does contain

(Sir Robert Hadfield.)
a certain amount of internal stress, and that is to some extent detected if the elastic limit is taken. In this connection I might quote the tests on a steel with which I have been working, which is similar in composition to the specimen 9H referred to by the author, so far as the carbon, nickel, chromium, etc. are concerned. We obtained in one specimen 88 tons yield point, 117 tons maximum stress, 12 per cent elongation, 40 per cent reduction of area, and 28 ft.-lb. Izod, which I think the author will admit is a very good result. The steel was quenched in oil and not even tempered, but another sample, re-heated to 250°, showed a slightly higher Izod result—30 ft.-lb. On the Frémont test, the first sample gave a breaking load of 6 kg.m. × 10 degree bend, and the second 8·1 kg.m. × 10 degree bend. To convert to ft.-lb., multiply by 7·7, so that the comparison between the Frémont and the Izod shock test is a very simple matter.

Mr. A. A. Remington: I should like first of all to point out that the Report of the Steel Committee is not a Report on a research. Research was carefully and purposely avoided, and it is a Report of tests carried out with the object of establishing the physical properties of certain steels, and furthermore it was carried out with the aid of a definite amount of finance, and in certain respects the Committee had to cut its coat according to its cloth. These two reasons explain why we did not include elastic limit or the Frémont or many other tests that might have given interesting and valuable results. One of the early difficulties in preparing this work was to find out just what to do, how to do it and where to get it done. The work was carried out during the war, when everyone was immensely busy, and that added not a little to the difficulties, although in some respects the war assisted, because it made the importance of the work realised, and that brought assistance even under great difficulties.

The voluminous nature of the work in the Report has been commented upon, and on this point I think we all agree, but it appears to me that it only just touches the fringe of the subject. The Report covers a series of tests carried out on one cast only of each of twenty steels, which, after all, is a very limited matter compared to what must be done in this direction before the work can be called at all complete. It is really unsafe to base too much upon, and attach too much importance to, a series of tests carried out on only one cast of a steel of a particular type. Fortunately, the Committee was composed very largely of specialists

who thoroughly understood the subject under review, and therefore tests that were incorrect or doubtful could be suitably dealt with, and the curves plotted tempered with the knowledge of the members of the Committee, otherwise the work might not have been nearly as useful as it is likely to be.

With regard to the suggestion that has been made by Sir Robert Hadfield, and probably will be made by others, that further tests are required, the Committee have a certain stock left of nearly all the twenty steels, and doubtless could arrange that anyone sufficiently interested could carry out further tests on these steels so that the identical steels of which the characteristics are given in the Report could be tested for further physical properties that it was not considered advisable to investigate at the time.

I should like to emphasise what the author said with regard to test pieces. When this work was started, it was found that there were no standard test pieces upon which the tests could be carried out, and the Committee had first of all to standardise these test pieces. They had before them the British Standard "C" test piece, but that was quite indefinite, except as to diameter and length. There were no standard ends in existence to suit the various testing machines, and the Committee had to design the standards which they adopted and which are published in the Report. For the Izod test, the height of the hammer from the anvil and also the form of the notch had to be standardised. There were various heights and notches in existence at the time, and I believe the work done by the Committee in this respect has already been of considerable utility.

The author referred to the difficulty of finding out what the engineer wants. Mostly the engineer has to use what he can get from the steelmaker, and not necessarily what he would like. The basis of all engineering work is necessarily the material that the metallurgist can provide.

With regard to the speed of impact, mentioned by Sir Robert Hadfield, this matter was discussed a number of times in Committee, and I almost think it was the discussions at the various Steel Research Committee meetings that led Major H. P. Philpot to carry out the research referred to in his paper before the Institution* in connection with speed of impact, where he showed that the energy absorbed in the breaking of a notched bar was the

* See Proc. I.A.E., Vol. XII., p. 235.

(Mr. A. A. Remington.)
same whatever the speed of impact within the range of his tests.* He found that by breaking a notched bar by slow deflection in the Brinell machine, the energy diagram when measured showed exactly the same absorption of energy as a similar fracture in an Izod machine.

Mr. H. Brearley: I propose only to refer to one or two points in the paper where I think the author has offered an opinion which is, to my way of thinking, either not a correct opinion, or is not expressed in the simplest possible terms. There are two, notably in connection with the comparison he makes in the diagrams relating to the influence of mass. When talking about the mass effect of the 2H steel, he showed us a curve bowed like an inverted U, and he expressed the opinion that one of the reasons why that curve comes down after going up is due to the increase in grain size caused by the relatively slower cooling of the larger mass. That is a very good example of the kind of point of difference where I always find pleasure in telling the author that he is mistaken, and in this case I take the opportunity of telling him that the rate of cooling has nothing to do with grain size, at least, not such variations in rate of cooling as existed between the smallest test bar that was used and the largest test bar. I know the author may tell me that he is right and that I am labouring under a momentary delusion, but I also know that he will go back to Sheffield and make a few experiments, and in a few weeks' time will probably modify his statement. On page 117 in connection with that very interesting comparison shown in the curves on page 116 between the steel hardened in water and that hardened in oil, the author makes this very amazing statement: "While the convergence of the yield, maximum stress and ductility curves may be simply explained by assuming the regular and progressive breakdown and softening of the hard martensite, which is less stable, and therefore more easily decomposed in the water quenched bars . . ." I should like the author to tell us how he knows that the martensite due to water quenching is less stable than the martensite due to oil quenching. Then he goes on to say: "Besides there being more of it, the difference in the Izod curves would appear to be due to some more deep-seated and obscure alteration in the structure or constitution of the resulting sorbite." I rather regret that in this respect the author has given a metallographic explanation. I do

* See Proc. I.A.E., Vol. XII., p. 295.

not say the difference is not explained: it may be explained to people who understand metallography and are able to make a distinction between the different terms that do not always mean different things, but for this drop in the Izod curve there is, to my mind, an explanation which can be expressed in purely mechanical terms. The water hardened and tempered specimen gives a better Izod result because it is harder to begin with. The user of steel, whether he is building automobiles or anything else, hardens materials in most cases, not because he wants hardness; he hardens it and then he softens it, and the reason is that with hardness he can buy toughness. Thinking along this line, it can be seen that in many cases the main reason why people put nickel or chromium into soft steel, or sometimes why they put nickel or chromium together into steel, is that they may, in the first instance, get a larger amount of hardness in order that they may spend it in buying toughness.

The author referred to the hump shown in some of the mild carbon steel curves at 400° as being discovered by a man named Deakin. The discovery was made many years ago: it was at the time that Professor Heyn, of Berlin, was publishing results showing the abnormal corrosion which takes place in steels that have been hardened and tempered at 400°, and I thought it was so remarkable a corroboration of some of Professor Heyn's results that I mentioned the matter to him, but he said it was all rubbish, and that there was nothing in it. The particular piece of work which led Deakin to observe the hump in the curve at 400° was carried out as the first effort of a workman who was enthused with the idea of doing a complete piece of work on his own. He was given some bars of mild steel and asked to harden and temper them, to prepare his own test pieces and pull them, and this abnormal behaviour at 400° was one of the results he came up against, which he thought was a gross irregularity arising because he was not very competent to do that kind of work.

I should like to say a word about the impact test results given in the Report. Anyone who carefully looks through the figures will be struck by the fact that the Izod figures on the whole are more consistent than the Charpy figures. Impact testing in England was almost entirely neglected at a time when impact testing on the Continent was making considerable headway. On the Continent the standard impact testing machine is the Charpy machine, and I think we should congratulate ourselves upon the

(Mr. H. Brearley.)
fact that the machine known as the Izod testing machine, which was really developed—I do not say first introduced—at a much later date, has been shown by the enormous use made of it in these investigations to give results which are more uniform than those given by the Charpy machine. In my opinion the Izod machine is the best of the impact testing machines for works purposes, that is to say, for internal works information purposes, yet designed, and I do not in that statement except the machine designed by M. Frémont.

Dr. Walter Rosenhain: As regards the general plan of the investigation, Mr. Remington has carefully, and I think, wisely, disclaimed the word "research." The Report is a valuable, an important and a useful document, and one which contains a large amount of information that was very urgently needed, and is now given in a very presentable and accessible form, which will be extremely useful provided it is rightly applied, but that does not constitute a satisfactory basis for a knowledge of automobile steels. That I do say, and say quite deliberately, because the work has been carried out on merely conventional lines. The tests which have been selected are in many cases either meaningless or nearly so, and the work has not been carried to the point where real knowledge of the fundamental facts has been acquired by it. It has established what it was aimed to establish. I am criticising the aim and not the execution. It was aimed at establishing ordinary test results which could be obtained from certain definite types of steel when treated in certain definite ways. It has done that, but the further step to know that which was really wanted, what the value of a particular steel was when treated in a particular manner, for any given particular purpose, it has not ascertained, or at any rate, only in a very imperfect manner. Leaving that aspect of the matter, let us look at the facts of the Report itself. There were, I believe, variations in the results obtained by different investigators. That was to be expected, but unfortunately the only thing that it was considered possible to do in such a mass of results was to smooth out these "errors," using the best judgment and the highly expert knowledge of the Executive Sub-Committee, in which expert knowledge and experience I, personally, have every confidence. When the results of an investigation, even if it is not called a research, are examined and discrepancies are found, the most useful thing to do is to find out the cause of these discrepancies. They may have been in the

tester, they may have been in the method itself, they may have been in the steel, or they may have been in the treatment which the steel received somewhere or other in its manufacture, but by simply smoothing out these odd observations which did not fit in with the general knowledge of the Executive Sub-Committee, or did not fit in with the results of other testers, I think most important material has been neglected. If we could have discovered as the result of these things the reason why wrong results occur in testing, or why steel occasionally goes wrong and gives an erratic figure, and perhaps occasionally an erratic result in service also, then we should have done something very important indeed. Now I know I am asking for more, and this work was done under war conditions of extreme difficulty, but it does seem a pity to say that we should be limited by conditions of finance or circumstances of time to neglect carrying on this work to a point where we can really get a good deal more forward in our knowledge of the whole business. These smooth curves are extraordinarily good and clear, and very excellent if the object of the Report were the teaching of a class, but I think that, for the practical man to use them is possibly just a little dangerous in one or two places, and I think the author, although he is very familiar with these curves and knows exactly what they mean, has gone a little bit astray in interpreting them. For instance, in Fig. 7, relating to the tempering of nickel chromium steel, and still more, in Fig. 4, which relates to the tempering of a series of oil hardened and water hardened steels, he argues at considerable length about the manner in which these curves approach each other and intersect. If he had realised what was the margin of inaccuracy or uncertainty about any one of these curves he would have seen that there was no explanation needed of such apparent differences as the fact, for instance, that the reduction of area curves appear to intersect at 450°, whereas the maximum stress curves only meet at 700°. Surely, if these curves are broadened out so as to cover the range of variability of the test results, there is nothing that requires explanation.

Mr. Dickenson: I did not say reduction of area.

Dr. Rosenhain: I beg your pardon. At any rate, with the Izod figures there is obviously a crossing over which is probably larger than the experimental error. For instance, the maximum stress curves only meet at 700° as drawn, but I think it would have been perfectly possible to have drawn them somewhat

(Dr. Rosenhain.)
differently without any stretch of the imagination. The thing is not vital in this case, but we must not be misled by seeing smooth curves in pretty colours to attach too great a degree of accuracy to them. We must carefully read the figures on which they are based, and realise that they are smooth curves representing the judgment of very competent people as to what is the average figure for these things, and we must not attach greater value than that to them, and I am sure the Committee themselves will emphasise that point of view.

The other point that I want to deal with is the question of the yield points. Although the yield point is a conventional thing to measure, I am inclined to go so far as to say that it is an utterly useless thing to measure, because I do not think in many cases it has any real meaning. These steels are to be used for moving parts in automobile construction. At any rate, in the majority of cases they are exposed to repetition stresses and so-called fatigue stresses, so that what we want to know is neither maximum stress nor yield point but the resistance to fatigue, the power which that steel has of standing up to the particular kind of treatment it is going to receive in the engine. I am not prepared to say that there is any individual test which will give that information except that of putting the steel in the engine. I think ultimately we shall get it, but whatever else will tell it, the measurement of yield point in the ordinary way certainly will not. Whether even to measure the elastic limit and the limit of proportionality or the limit for elastic recovery—the stress under which a specimen will go back to its original length—will tell us more accurately than the yield point what we want to know, I am not prepared to say, but a definite series of Wöhler tests would have told something very important, and if the Report contained nothing else it ought to have contained these tests, because they are of such very fundamental importance, and as the result of our experience, particularly in aero engines, we are coming to the conclusion that these other figures have comparatively little meaning, and that it is the fatigue resistance of the material that matters, and upon which design will have to be based. Therefore, it is a very great pity that something of that kind has not been included, because sooner or later we shall come to the realisation that it is the fundamental knowledge which we must have. With regard to extraordinary yield points, I want to refer to the theory of internal stress as producing an apparently low yield point.

I want to suggest to those who put that view forward what it really means. Supposing we have a bar of steel (AB in Fig. 8), which we will regard in longitudinal section, and that it is in a condition of internal stress most favourable to misleading the man who tests it, a certain portion, C, being in compression and the other part in tension, and that these two forces are sufficiently intense to stress the outer portion to its elastic limit, i.e., it carries all the stress it can without undergoing permanent deformation, and correspondingly there is a heavy internal compression in C. These two things are in opposition to each other. Now, if we stretch that bar we will increase the stress on the part already in tension, and decrease the stress on the part previously in compression; that is obvious, but what will happen is that the elastic limit, i.e., the point at which the permanent stretch occurs in

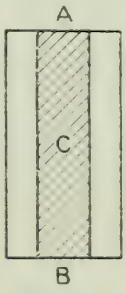


FIG. 8.

the outer portion, will be reached sooner than it would have been otherwise. But no more stretch will result from the release of that kind of stress than the elastic compression of which the portion C is capable, and if it is worked out it will be found that, supposing an internal stress of something like 50 tons per sq. in. is assumed, the total stretch which can be accounted for in this way is of the order of one-quarter or one-half of 1 per cent. There cannot be internal strains, internal elastic displacement, greater than the amount of the maximum internal stress which the thing can bear, i.e., its elastic limit stress, multiplied by the modulus of elasticity, and that is a very small amount indeed, so small that most methods of measuring yield point would fail to detect it even if it happened suddenly. But it does not happen suddenly. In such a bar there is a complicated system of balanced stresses which are released gradually, and my view is that the effect of internal stresses will be merely to alter the slope of the *elastic* curve in the stress-strain diagram. I do not think that

(Dr. Rosenhain.)
the position of the "yield" point, or of the elastic limit itself, is likely to alter very much. It may be altered a little, but what is going to be altered is mainly the amount of apparently elastic stretch which is produced for any given increment of load, in other words, the slope of the elastic line will be altered, but I do not think that the position of the apparent yield point is likely to be altered. A gradual unloading of these internal stresses will occur as external load is applied, but not a sudden unloading. That is a difficulty which will have to be explained before we can accept these views of internal stress effects. The somewhat critical spirit in which I have looked at this Report will, I hope, be taken in the way that it is meant, that is to say that I hope the investigation will yet be completed into a research, and that the organisation which has been set up for carrying out this work will be continued and will bear still further fruit, though I do not undervalue the fruit that it has borne already.

Mr. R. T. Rolfe: The Izod brittleness which is obtained on tempering nickel chromium steels at from 250° to 450° C. is shown by some of the other steels dealt with in the investigation, for example, in the oil quenched series of steels 6H, the 0·35 carbon steel, 7H, and the 3 per cent nickel steel, but not in the water quenched series. I think this point is important, because it affords additional confirmation of the view that the author has put forward elsewhere in the paper. On page 111, in another connection, he says, with reference to the results of tempering bars of different sizes, "The maximum in the Izod curve marks a mass above which water quenching is no longer able to produce a sufficient cooling rate to prevent separation of free unentangled ferrite from the martensite, while concurrently the grain size tends to increase as the cooling velocity diminishes." The fact that that Izod brittleness is shown in the case of both these steels with oil quenching but not with water quenching is, I think, strong confirmation of the correctness of that particular view. The only other steels which show this brittleness on tempering at about 400° C. are those oil hardening and air hardening nickel chromium steels, 8H, 9H, 10L and 10LL, in which again there is a relatively slower cooling velocity. I think that is a point worth mentioning. The cause of this brittleness is apparently quite obscure, but it is interesting to note that with grey cast-iron precisely the same thing happens. The resistance to shock is lowered if the steel is tempered at about 400° C., as has been shown

by J. E. Hurst* and by myself.† In cast-iron, the brittleness is probably due to a change in the pearlite, while in tempering hardened steels the maximum Izod brittleness is apparently coincident with the maximum amount of troostite, which is obtained on tempering martensitic steels at 400° C., when also, as the author states on page 113, a maximum solubility in dilute sulphuric acid was found by Heyn. There is again the interesting fact that the hump in the Brinell and maximum stress curves shown on tempering at 400° C. in the case of the two carbon steels, 2H and 6L, when water hardened, is not associated with this Izod fragility, while in the other carbon steel, 6H, there is a falling-off in the Izod figure not associated with a hump in the Brinell and maximum stress figures. There is again the question of the brittleness in mild steel when actually worked at a temperature of 300°, the so-called blue brittleness, by reference to the tempering colour which is produced at that temperature. The author has emphasised a point of practical importance, which is the avoidance of tempering over this undesirable range, and I think it is already fairly well known that this range should be avoided, but there is this practical point, that tempering in service may actually take place at these temperatures, as, for example, with certain aero engine exhaust valves. This points to the need for further work being done to show to what extent the brittleness which is found in testing cold, but when tempering has been carried out at these temperatures, is related to the actual brittleness when the tests are carried out at the temperatures in question, 300° to 400°, and the maximum stress and Brinell hardness figures which are co-related therewith. It appears that work might be done in that direction which would throw further light on the question of brittleness in steel.

Mr. A. Stubbs: I well remember the paper read by Mr. Pomeroy in Birmingham in 1914, out of which has arisen this most excellent piece of work. The Report provides information of real practical use to the automobile engineer. For years papers have been read before various Institutions dealing with particular steels and particular heat-treatments, but this is the first occasion on which a work of this character has been attempted by any Institution.

* "Engineering," July 4th, 1919, p. 1.

† Discussion on W. H. Hatfield's paper on "Steels used in Aero Work," read before the Aeronautical Society, April 18th, 1917.

(Mr. A. Stubbs.)
and I think this Institution is to be congratulated on taking steps firstly, to deal with the standardisation of automobile steels, and, secondly, to publish the Report of the investigators, and in such an able manner. When the magnitude of the work is considered, and the conditions operating during the war—when the work was being carried on—I think it speaks well for the way in which every person, manufacturer and investigator, connected with it gave their services whole-heartedly to the work.

It is to be regretted that Mr. Pomeroy is not here to-night, because I think his views respecting the English steelmaker, drop forger, and those connected with the supply of raw and semi-raw material to the automobile industry will have changed somewhat.

If the whole of the French, German, and American technical literature is studied no single report or paper will be found to give such a vast amount of information as is contained here, and I should like personally to thank the author for the able manner in which he has edited the work and also for his paper, which suggests not only how some of the results should be studied, but also the manner in which the information can be profitably interpreted.

Lt.-Colonel Hutchinson: First of all, I wish to endorse all that the other speakers have said as regards the excellent work that is contained in this Report, which I am sure will remain as a standard, and I hope it will be the beginning of still further work. I am not a metallurgist, but I have been greatly interested in the work of the metallurgist, because we have had to deal with a number of failures of parts, and I cannot help thinking, from the fact that certain parts on some particular machines stood up to their work very well whereas other parts failed, that there was a great deal of variation either in the material or in the design or in the treatment or manner in which the material had been applied. The fact that one maker of a chassis produced a vehicle of which the road springs, for instance, did not fail, whereas the springs on other makes of chassis employed under the same conditions, doing the same class of work, failed badly in use, was inquired into, and we heard several different stories. In one case it was the makers of the steel of whom it was said that they would not supply the kind of material asked for, in other cases the material had not been treated properly, while in further cases we were told that the material had not been properly applied, or, in other words, the design was defective. These variations in the results showed

that there is a good deal to be learned by producers of the machines and possibly also by the makers of the steel or the other materials employed. Although the author to-night naturally refers only to steel, there are other materials, such as white metal and bronze for running bearings in contact with steel where we sometimes get cases of excessive wear. Something further has to be learnt about these details. My main point is that we wish to obtain more information as to the practical results obtained from these materials when they are put into service, so that we can compare and find out what is really the most suitable material for certain particular sets of conditions. I suggest that information can best be obtained by a critical analysis of the conditions under which the materials are used, and the results of their behaviour under these conditions. From many aspects this can only be obtained by co-operation between the users and the repair shops and the designers and the metallurgists; I think we want a fuller co-operation than we have had in the past.

Mr. James (United States Bureau of Standards): I am not a metallurgist, but I have been engaged on internal combustion engine work at the Bureau, and there is one point I would like to make which I do not believe has been emphasised by some speakers, and that is the excellent lay-out of the work of this Committee. As I understand the reason for the work, it was to enable the men who draw up specifications to be certain they receive the material they want. I think the value of the broad-minded way in which the work has been distributed amongst various observers in getting test results and in preparing specimens has been overlooked, because the test results show the range of variation which is to be expected in ordinary production and testing; from that point of view the work is very valuable, and I believe some of the criticisms which have been offered from the viewpoint of pure research are not altogether fair. The work was carried out during the war, and as I gather, the main purpose was to obtain assistance in interpreting specifications, and not to carry out pure research. I think it takes a great deal of courage on the part of men carrying out research work to stick to a programme. I know we have great difficulty at the Bureau of Standards, and they often criticise us for not sticking to a programme. Therefore, I think the Executive Sub-Committee is to be very heartily congratulated by the Institution for standing by their original programme, which is a most excellent one.

(Mr. L. H. Hounsfield.)

Mr. L. H. Hounsfield: I want to ask the author a few questions about points which are not clear to me. First, Fig. 6, page 120, shows a marked improvement in qualities for small diameter bars. The author has explained how this is due to difference in the structure of the material, brought about by heat-treatment. I would like him to say what effect, if any, would be produced if the smaller sections had been machined from larger diameters instead of, as I presume they were, being rolled more or less to the diameters given, because it does frequently happen in structures that the part has to be machined down from a larger diameter before heat-treatment, and I would like to know if these greatly reduced diameters get the benefit of the small diameter as indicated in Fig. 6. I gather from Fig. 4, page 116, that water quenching is decidedly preferable to oil quenching if tempering is to follow. Will the author tell us whether, in handling parts of widely differing sections or complicated shapes, there is not more danger of cracking these parts if they are water quenched to start with instead of oil quenched? In explaining Table III., page 108, the author referred to the first line of figures and said that the elongation, which was 3·0 per cent instead of about 33·0, was possibly due to the bar having broken at the end. I would like some further explanation of this, as I do not see how any figure at all could be obtained if the specimen broke outside the portion under observation, and even if a value of 3·0 per cent were obtained, would it be due to the unreliability of the observer or the unreliability of the steel? Then again, in the next column, reduction of area, is given "Nil," whereas the curve opposite, Fig. 1, apparently gives it as 52.

Mr. S. Whyte: I think the object of the investigation carried out by the Committee has been attained; it is, as far as I understand, really to give automobile engineers some specification on which they can order steel on a rejection basis within limits, and let them know exactly what they are ordering and what their limits are expressed in physical terms. From the point of view of the engineer himself, however, we want to know much more than what we get in the Report. As Dr. Rosenhain said, we want to know why steel breaks down in use. For that reason, at the Associated Equipment Co. we have our own laboratory with suitable equipment, where we determine curves on similar lines, but we pay more attention to our Wöhler tests, repeated impact tests, and repeated bending tests. We represent these in

similar form to the curves shown in the Report, and also include the usual tensile tests, because these provide a certain amount of knowledge as a matter of comparison with other steels. We find that the maximum resistance to fatigue under repeated impacts has only an indirect bearing on the yield point; it usually comes, for, say, a chromium nickel steel somewhat similar to the one mentioned in the Report, at about 400° C. With chromium vanadium steel, which we prefer for parts which have to stand high fatigue resistance, it comes at about 500° C. Chromium vanadium is much superior to nickel or nickel chromium steel in this respect, and up to the present the result of many investigations plotted against nickel steel and nickel chromium steel shows that we get a much higher resistance to fatigue with a chromium vanadium steel of 0·4 carbon and 1·4 per cent chromium than with any other nickel or nickel chromium steel. I hope the Committee will include chromium vanadium steel in any future investigation. With regard to nickel chromium air hardening steel mentioned in the Report, I think that the results, taken as they are from two casts of steel, one to the higher limit and one to the lower limit, are extremely misleading, because every cast gives such very variable results. Sir Robert Hadfield has mentioned a steel giving 117 tons maximum stress and 28 ft.-lb. as the Izod figure. One of my tests gives 105 tons maximum stress, the yield point and maximum stress both rise at 100° C. and then fall uniformly. The Izod impact figure is at a minimum between 100° and 200° C. and is there 28 ft.-lb. After air hardening it is 33 ft.-lb. and after tempering at 600° C. it is 80 ft.-lb. I think that since such extraordinary discrepancies are obtained with very minute differences in chemical composition in nickel chromium steels, a rider should be added to the Report to the effect that the results are not representative for air hardening steels. Certainly it appears that the steels reported on are very near the minimum for impact, and in that way it is rather an advantage.

The PRESIDENT: In closing the discussion, like nearly all other speakers, I want some more information, and the information I want is what all users want, and that is some test which will give us the results in practice. It is much easier in automobile work than in many other directions, but with all due respect to Dr. Rosenhain, I do not look upon the Wöhler test as any more reliable than any of the multitude of others. With automobilism, as Col.

(The President.)

Hutchinson has said, the thing is to get close co-ordination between the users and the manufacturers. It can be done, because, after all, the results can be obtained within, comparatively speaking, a short time. In the work I am chiefly connected with, in some cases we have parts which break, but only after about twenty years', and sometimes forty or fifty years' life. Dr. Hatfield the other day asked me whether I thought that the Wöhler type of test running to about ten million reversals was enough. I find some of our steel stands about 350 million reversals before we get a failure, and we do want a test which will give in a reasonable time a real relation to its use in practice. That is a very large thing to ask for, but it is a thing which, by co-ordination on the part of the steelmaker and user, we shall get a good deal nearer to in a few years than we have done in the many years' struggle in the past, simply owing to that co-ordination which Col. Hutchinson spoke of, and towards which we must all work with all the frankness which lies within our nature.

The only other point I would like to deal with is, that I should like to see microstructures of the steels which gave those discordant Izod results. I feel it may be we should like to have greater variation in these with varying temperatures than those in which the Izod tests agree with the others.

Mr. Dickenson, in replying on the discussion, said: In reply to Sir Robert Hadfield as to why the Frémont shock test was not included in the programme of tests, I would say that the limited number of testing methods which were finally selected after very full discussion were chosen, not because they were necessarily the best for giving the most valuable scientific information, but because they were sound and reliable methods and were generally used in engineering practice, and in the works test houses in this country. The object of this work from the commencement was simply to make it possible for engineers, accustomed to the use of well known and well tried testing methods, to know definitely and on good authority, what test results they might fairly expect to get from certain typical steels after different forms of heat-treatment. This is all that the Committee was asked to do—it will be understood that I am now speaking quite unofficially on their behalf—and I do not know what other tests than those selected have any wider or more useful practical application. I must agree that it would have been better if we had been able to include certain other testing

methods, but these additions would necessarily have lengthened the time required to produce the Report. I also quite agree with Sir Robert that, in the case of some steels at any rate, it is necessary, in order to get full information, to carry out notched bar tests with higher striking velocities than that of the 120 ft.-lb. Izod machine. The Charpy machine which has been used by the Committee to some extent for alternative notched bar tests has a striking velocity of 5·3 m. per sec. as against 3·5 m. per sec. in the Izod machine, so that within this range the effect of varied striking velocity may be noted in a large number of cases. The Frémont impact testing machine is capable of giving higher striking velocities still. When the programme now completed was drawn up four years ago, this matter of striking velocity had not received the attention which has since been given to it, and the Izod was selected because it was, in this country, by far the most commonly used machine for breaking notched test pieces. Whether under other circumstances, and despite some of its practical disadvantages, the Committee would have considered the use of the Frémont test as an alternative method, is a matter on which I will put forward no opinion. At the same time, I would express my personal view that the increased striking velocity of this machine assumes importance in only a few special cases, and here I agree with Mr. Brearley, that as a practical means of determining the relative brittleness (or toughness) of almost all steels in common use, there is no other existing machine which combines the advantages of regularity and reliability of result with ease and speed of manipulation, so well as the Izod. Speaking generally, and apart from a few very special cases, there is no doubt that the Izod test does give very reliable indications of comparative brittleness.

I agree that the usefulness of the critical curves published in the Report might have been increased had the analyses been shown in each case, just as in the "graphs" of the mechanical properties, but this omission can be put right if, and when, a further edition is required. In the meantime, it is only necessary to refer to another page for the analyses, which may, if desired, be written alongside the heating and cooling curves.

I deal more fully in my written reply with the question of the absence from the Report of true elastic limit determinations referred to by several speakers, but my own view, briefly expressed, is that if we had attempted to include, in every case, an exact elastic limit determination in addition to the yield point, and if

(Mr. Dickenson.)
the measurements were to be taken with sufficiently delicate extensometers, the Report might not have been out for years to come. It is not only a very troublesome matter to make these measurements, but I am quite unable to see any reason for supposing that such determinations of the limit of proportionality of the test piece (this alone being actually measured, and not the real elastic limit of the steel) are of any value whatever to the engineer.

Mr. Brearley deals with the function of initial hardening as a means of securing a good Izod test result, and I gather that he disagrees with my metallographic explanation. He says that the user of steel hardens his material so that he can then soften it, and thus buy toughness. I venture to think that this is not quite the right way of explaining the process, and that the real object of oil, water, or air hardening is not so much to produce an excess of hardness which may be exchanged for toughness by a further treatment, as to obtain by the quenching a structural condition of which homogeneity is the first essential, and which only involves hardness as a secondary and unavoidable, but not useless, consequence. The close association of the carbides and ferrite which is absolutely necessary before it is possible to obtain the best combination of resistance to fatigue—this implying a high elastic ratio—and toughness, demands first the production of hard martensite which is brittle, or lacks toughness, because of its hardness. When this is softened, by decomposition to sorbite, on tempering upwards to the critical range, the toughness rises as the hardness falls, certain anomalies apart, but the high elastic ratio is maintained because the carbide particles which are thrown out by the decomposed solid solution remain in exceedingly close association with the ferrite. I think, in short, that it is better, and more precise, to say that the object is to produce hard martensite because this is the only way to obtain the desired type of sorbite with its advantageous combination of mechanical properties, rather than to speak of giving away hardness for toughness.

As regards Dr. Rosenhain's remarks, I am well aware that from one point of view, no mechanical test is of any real value unless it measures definite physical properties which can be correlated with the behaviour of material in service, but, as Dr. Rosenhain knows perfectly well, the programme we have carried out did not call for any such correlation of physical properties as determined by mechanical testing and actual working conditions. The investigation has not been carried out to ascertain why steel parts

sometimes fail in service, or to learn how the deficiency of this, or that, physical property may lead to fracture under certain circumstances, and generally to answer questions of that type, but simply to establish certain mechanical properties of a selection of typical materials after subjection to a series of definite heat-treatments.

Another criticism of Dr. Rosenhain is the omission of the Committee to discover and explain the causes of the various discrepancies in the investigators' results which appear here and there throughout the Report. Some reference to these differences is made in the Preface to the Report and in my "Notes," but when Dr. Rosenhain complains that the Committee has smoothed out, without explaining, these discrepancies, in the "X" figures and the coloured graphs, which I still venture to think will be found of very great value to the engineer, he should not overlook the fact that the actual test results returned by the investigators are given in every case.

Any inquirer may study these variations and read into them, if he can, some meaning, some evidence of value, which has been overlooked by the Committee when deciding on the "X" figures. However, I would re-state my considered opinion, concurred in by all the other members of the Executive Sub-Committee, that most of these discrepancies are not to be traced to any obscure peculiarity of the material, but simply to the fact that test bars have not always been heat-treated at the temperature laid down in the programme. In several cases bars have been returned to the Committee, after treatment by well known investigators, with one end of much greater Brinell hardness than the other.* Sometimes such bars have been returned for re-treatment, after which they have given uniform hardness. The difficulty of making reliable deductions from such data will be fairly obvious, but I am glad to know that Dr. Rosenhain has confidence in the judgment and expert knowledge of the Executive Sub-Committee who have only, to the best of their ability, attempted to rescue the ordinary user of the Report from quite unnecessary mental confusion.

I have to thank Mr. Stubbs and Colonel Hutchinson for their congratulatory remarks, and I agree with Col. Hutchinson that there is a great need for co-operation between the various quite distinct branches of industry concerned in the production of an

* Some of these curves would have caused a disciplinary enquiry in a well conducted steel works heat-treatment department.

automobile. I really think that this Report is a step in the right direction, as it is certainly one of the first pieces of work of its kind.

Mr. Dickenson, in further reply to the discussion, wrote: Referring further to Sir Robert Hadfield's remarks, I shall have much pleasure in reporting to my colleagues his offer to have additional tests carried out on his Frémont machine, using for this purpose the ends of the broken tensile test bars.

I am very glad that Sir Robert agrees with the suggestion that all authors of papers dealing with mechanical testing should adopt for their diagrams the same colour scheme as has been adopted in the Report, so that, for example, all would identify at a glance the green curve as indicating Izod test results. If this became a generally accepted convention, the labour of studying other investigators' published results would be greatly lessened.

Mr. Remington's remarks call for little comment, since they come from the Chairman of the Main Committee, and are mostly explanatory of the Report. I would, however, point out that when Prof. Philpot observed that the energy absorbed in the breaking of a notched bar was the same whatever the striking velocity, within the range of his tests, including the breaking of a notched bar by slow deflection in a Brinell machine, he was experimenting with a definite type of nickel chromium steel, and that certain carbon steels are, when suitably heat-treated, as has recently been shown by Sir Robert Hadfield, much more sensitive to variations in the velocity of impact.

Mr. Brearley made some remarks on the influence of cooling rate on the grain size in the 2H water quenched series. I have followed my usual course when Mr. Brearley disagrees with me upon such a point, and have very carefully examined the microsections prepared in my own works from a similar set of water quenched bars of varying mass, and find that the difference in the grain size between the water quenched 3 in. and 11/16 in. bars is quite inconsiderable. It is a very difficult matter to state with any confidence the mean grain size of such specimens, but although I think there is possibly a tendency for the larger bars, which must necessarily have cooled in the water at a lower rate than the smaller bars, to show a slightly greater grain size, yet this is far

less than I should have expected, and I have pleasure in informing Mr. Brearley that, on this point, he is quite right.

In further reply to another point raised by Mr. Brearley, I would remark that "the main reason why people put nickel or chromium . . . into steel" is not so much that they may, in the first instance, get a greater amount of hardness to be expended in buying toughness, as that they may more readily, and more uniformly, obtain a high degree of homogeneity, and close association of the hard carbides and softer ferrite—this implying a martensitic structure—which is essential if considerable toughness is to be combined with a high elastic ratio. The incidentally produced hardness, which involves more or less brittleness, must subsequently be reduced by tempering.

Dr. Rosenhain's remarks were partly appreciative, showing clearly that he understands the objects which animated the work of the Committee and admits the practical usefulness of the Report—and for these I thank him—but they include some criticisms to which exception must be taken. Take, for example, his surprising statement (page 136) that "the tests which have been selected are, in many cases, either meaningless or nearly so." In actual fact the complete list of tests selected were the Brinell ball indentation test, the tensile test, with its indications of yield point, maximum stress, elongation per cent, and reduction of area per cent, the Izod test, and the Charpy test, both the latter measuring the energy absorbed when a notched bar is broken by a falling weight.

Putting on one side the yield point in the tensile test, which is often misleading, as I have already stated, we will examine the other test results reported on. The Brinell test shows quite definitely the amount of deformation which can be produced in a sample at a given load, and forms a most useful workshop indication, to say nothing of its value in the laboratory, of uniformity of hardness and of heat-treatment. A testing method which informs us whether a batch of steel has been properly and uniformly oil hardened, or otherwise treated, and which enables the expert user to deduce, not merely the tensile strength, but by inference the ductility, and in many cases the approximate fatigue range of the steel, is surely not a "meaningless" test. Similarly with the maximum stress in the tensile test which, if it does not measure any definite physical property of importance, appears to be the best method we have for determining the relative hardness

(Mr. Dickenson.)
of structural steels, and probably gives, under definite and known conditions, much more precise information regarding the position of the true elastic limit than the elusive yield point. To make this clear, I would point out that the true elastic limit can only be determined exactly by a prolonged series of tests in some form of alternating stress machine such as was used originally by Wöhler. The so-called elastic limit as ordinarily taken in the tensile test is only the limit of proportionality, and is apt to be affected, even if it can be accurately measured at all, by internal stress in the test piece. Such "Wöhler" tests take too long to be of any use in a busy works test house, but it does appear that the true elastic limit, which is to say the fatigue range, bears a fixed ratio, in different samples of similar composition and microstructure, to the maximum stress. It follows that once this ratio has been determined for a certain grade of material, the maximum stress will indicate the fatigue range of that material, providing that the sample test is known to have been subjected to a definite heat-treatment, say, oil hardening at 820° C. and tempering at 600° C. to give a definite microstructure. This, then, is not a "meaningless" test result.

Next, as regards the elongation per cent and the reduction of area per cent, indications of the tensile test, I really do not understand how results stating clearly and definitely the ductility of the materials of construction can be described as "meaningless." It is quite true that in the past engineers have often valued ductility too highly, and sometimes for the sake of this property have sacrificed others, for example, resistance to fatigue, which would have been of greater value to them in certain applications, but none the less ductility is required to a greater or less degree in almost every stressed part of an engine.

Finally, we come to the indications given by the Izod and Charpy notched bar tests. If Dr. Rosenhain contends that these tests are "meaningless," then much of his published work, and certainly much of his teaching are equally so, but I cannot believe that even if indentation hardness and ductility are turned down as of no importance, Dr. Rosenhain would deal in this way with the property of toughness.

Yet although I am very much surprised at Dr. Rosenhain's sweeping condemnation of the testing methods which are used and valued not merely by engineers throughout the world, but actually by the National Physical Laboratory, I fully realise how

much the value of the Report would have been enhanced had it been possible to determine on every sample in the programme one property, which I agree is of fundamental importance, namely, the "safe fatigue range." The reasons for this omission were, however, pretty obvious, and I have no hesitation in saying that if the Committee had undertaken this work the Report would not have been forthcoming for many years. It is, indeed, probable that many of these additional results would have been, in every sense of the word, "meaningless," since, as I have found in my own experience, that such determinations, however carefully and laboriously made, are apt to be inconclusive and unsatisfactory. Still, there is every reason why this work should be put in hand the moment it can be shown that there are available sufficient investigators and machines to carry out such a large number of tests as would be required, and that the results, when obtained, would be exact and precise determinations of the fatigue range, and not merely values equally as doubtful as the yield point itself.

In the meantime, since such values cannot yet be as readily and confidently ascertained as Dr. Rosenhain's remarks would appear to suggest, we are driven back, at all events in the everyday routine of the works test house, to other methods of estimating in our materials a property of the most vital importance in any stressed portion of a moving machine. In criticising the explanation offered by Mr. Brearley for the abnormal and widely divergent yield points which observers sometimes record in hardened and not very highly tempered specimens, Dr. Rosenhain should remember that in these cases there is no sudden yield point such as occurs in annealed mild steel, but only an ill-defined point where the strain begins to increase appreciably more rapidly than the stress. Thus, it is unnecessary to expect a sudden unloading of the internal stress, and the gradual unloading, which Dr. Rosenhain admits, is quite sufficient explanation. In actual fact most men carrying out tensile tests on such steels practically guess at the yield point, feeling obliged to make such a record because there is a place for it on the printed test sheet, and the sooner this is recognised by inspectors, and more particularly by those issuing specifications, the better. However, as I have stated elsewhere,* I am rather inclined to think that, in some cases at all events, the abnormally low yield points of hardened steels, are partly to

* West of Scotland Iron and Steel Inst. "Nickel Chromium Steels."

(Mr. Dickenson.)
be explained by assuming the production of some austenite, although there seems to be very little doubt that internal stress in the test piece assists in producing movements under stress which are interpreted as a physical property of the material, but which are in actual fact nothing of the kind.

I have already referred to Dr. Rosenhain's remarks on the treatment by the Committee of the discrepancies which occur here and there in the Report. As I stated at the meeting, most of these discrepancies are simply and obviously to be explained by the imperfect manner in which the heat-treatment was occasionally carried out, but there are, here and there, discrepancies which require rather more probing. Thus, reference to page 2 of the Report will show that several anomalous results were obtained from the softer of the two dead-mild carbon case-hardening steels, and the Committee did some extra work, finally to find that the variations were due to segregation, which was very marked in some of the test pieces. Every test piece broken in the preparation of the Report has, however, been carefully stored away so that it can be examined if, and when, the need arises. I think, nevertheless, that Dr. Rosenhain will find that very few of these discrepancies have any serious effect on the practical usefulness of the Report, and they certainly give most valuable indications regarding the allowances for error which should be made in practice.

Dr. Rosenhain's remarks on the care which should be taken in interpreting the smooth curves are very much to the point, and I can assure him that the Committee have been anxious from the outset that these diagrams should be properly used, and not abused.

Mr. R. T. Rolfe in his very interesting contribution supports the view expressed on page 111 with reference to the effect of the cooling rate of quenching on the test results. As explained in my reply to Mr. Brearley, I find, on looking further into the evidence, that the grain size can play no very large part in this particular matter, but this leaves unaffected the view that the extent to which free unentangled ferrite may separate from the martensite during cooling is of very great importance. I agree with Mr. Rolfe regarding the importance of ascertaining the mechanical properties of various materials, not merely in the cold after heat-treatment, but at elevated temperatures, such as occur in certain conditions of service. Such work is actually in progress in several other laboratories as well as my own at the present time.

Mr. Hounsfield's first question has led me to put in hand some experiments, which are not yet completed, to ascertain the precise effect of increased work, as compared with that of the heat-treatment, on the mechanical results obtained from small diameter bars. There is no doubt, however, that the greater amount of working does improve the elongation, reduction of area, and impact test results taken from such bars when tested longitudinally, but an opposite tendency would be found in cross-wise tests if such tests could be conveniently taken. This, however, does not apply to the effect produced by heat-treatment, which is to improve the test results, considered as a combined whole and including the true elastic ratio, of small as compared with large diameter bars, and certainly small pieces cut from, not worked down from, larger diameter bars give very much better results on heat-treatment, which is a very much more important matter than securing a little more elongation or toughness in one direction with corresponding loss in another.

The answer to his next question is that with parts of widely differing sections, or complicated shapes, most of the harder steels are much more liable to be cracked by water quenching than by oil quenching, because the variation in the rate at which different parts cool is greater, with a corresponding increase of internal stress acting principally and most dangerously at the changes of section. There is simultaneously more variation in the hardening effect, so that for parts of very irregular shape which are required to be of uniform and high tensile strength, etc., an alloy steel capable of being hardened effectively at a lower cooling rate is indicated.

As regards the elongation and reduction of area results quoted by Mr. Hounsfield, the letters O.M.H. (outside middle half) attached to a tensile test result, as is well known to most people connected with routine mechanical testing, mean that, for some reason or other which is not necessarily connected with the reliability either of the steel or of the observer, fracture has occurred towards one end of the measured gauge length, so that much of the elongation of the specimen has taken place beyond the gauge mark and is lost, giving a lower result than the real ductility of the steel would justify, when the stretched gauge length is measured. Then, also, a hard test piece provided with inadequate radii, or not pulled quite straight, will sometimes break off sharply in the neck and not in the gauge length at all, so that no reduction

(Mr. Dickenson.)
of area can be recorded, although under better testing conditions a satisfactory result, indicating considerable ductility, would be obtained. In drawing the curves, and deducing the "X" figures, the Committee have made due allowance for these well-known testing mischances.

I am afraid I cannot deal adequately with Mr. Whyte's remarks on the relationship of the maximum resistance to fatigue under repeated impacts and the yield point, in the absence of precise knowledge of his testing method and conditions. The striking energy in a repeated impact test is a matter of very great importance, and must be known before the results can be considered. With a considerable striking energy, producing fracture in relatively few blows, the toughness of the steel becomes the predominating property in influencing the result, while the results obtained by applying very light blows, requiring many repetitions before fracture ensues, are more greatly influenced by the true elastic limit, or the fatigue range, of the steel. When such tests are carried through with some one striking energy of blow, which is neither very large nor very small, the result obtained is affected by several factors, notably the fatigue resistance, and the toughness of the steel, each to some unknown extent, so that the result obtained measures no one single property and is, therefore, of no particular value. These remarks may be amplified a little in replying to the President, who voices a very general desire when asking for some test which will "give us the results in practice." The real object of all mechanical testing is to enable the engineer to ensure in his material the qualities leading to long life, stability, and generally good behaviour in service. I cannot think, however, that the desired information will ever be obtained by the use of a single test, and certainly not by means of those tests which attempt to imitate actual working conditions, but which inevitably fail to do so because the very important time factor is not taken into account. Since each test must be completed within a reasonably short time, the conditions of stress which cause fracture are not the same as those which, occurring in service, may, or may not, lead to failure in, at the least, a very considerably longer time. In my view, the correct procedure is for the engineer to decide what physical properties are concerned in resisting the various types of failure, and then exactly and precisely to determine those physical properties, each separately, in his material. Milk is tested, not by trying it on the family cat, but by measuring the

amount of butter fat it contains, testing for vitamines, and ascertaining its freedom from injurious bacteriological contamination. Much the same thing should apply to the testing of steel, and the determination of the amount of ductility, of toughness, of hardness, of the true elastic limit (or fatigue resistance), and so on, after ascertaining how each of these is related to behaviour in service, is the troublesome but sure road to knowledge and certainty. There is much work to be done here which can only be brought to a successful issue by that co-ordination of the steel maker and user of which the President speaks, and I submit that in this Report we have made definite progress in this direction.

COMMUNICATIONS.

Dr. W. H. Hatfield wrote: I consider that the author's notes are a fair statement on the work of the Executive Sub-Committee, and the Institution is to be congratulated on a unique piece of work which will give to automobile engineers a perfectly reliable set of data concerning most of the different steels which they use in the industry.

There is little to criticise in the paper. I would, however, like to comment upon the remarks on page 119 in which the superior toughness of the harder of the pair of carbon steels is discussed. The author appears to find his explanation in some intangible occurrences in the course of manufacture, but if he compares the analyses, he will find that the tough steel has a sulphur content of 0·02 per cent and a phosphorus content of 0·025 per cent, whereas the other steel has a sulphur content of 0·06 per cent and a phosphorus content of 0·065 per cent. Add to this that the tougher steel contains 0·78 per cent of manganese as against only 0·50 per cent manganese in the other, and it will be seen that we have here certain tangible influences to which proper weight must be given.

Again, at the bottom of page 119, the author, speaking of steel 2H in comparison with steel 6L, states that there is no great difference in composition, whereas if reference is made to the analyses, it will be found that 2H has a manganese content of 0·99 per cent whereas 6L has a content of only 0·50 per cent. I think the author will be the first to admit the important influence of this great increase in manganese.

(Dr. W. H. Hatfield.)

In conclusion, I feel that the Executive Sub-Committee which carried out the work are to be congratulated on the unchallengeable data which have now been issued to the Industry.

Mr. George Murray wrote: I should be glad if the author would amplify some of his suggestive and helpful notes on the Report on one or two points which seem to me to be obscure. The ten E.S.C. steels in Report No. 75 do not give the high and low composition limits *as* given in the Steel Research Report, and the percentage analysis with resultant strength variation in each E.S.C. steel I assume was a tolerance necessary for the steel manufacturer: also, that a user in ordering one of these steels could only expect to get physical properties within the limits specified for the particular steel.

In the paper it is mentioned that the X results given are those that may be expected in practice. I see, however, nothing definite in the Report, nor suggested in the paper, as to the research steels becoming the subject of a further *Standard B.E.S.A. Report*, or to indicate that a user could order, and get, steel to the high and low composition limits as tested, and wonder why.

When the most favourable procedure adopted by the Committee to get their steel samples correct in analysis, and the differences in composition and physical tests returned by the various eminent investigators are considered, it is evident that widely different results must be looked for from manufacturers' steel as a commercial product, taken in conjunction with the average works' heat-treatment.

I was disappointed not to see any alternating stress tests on the various steels, as while I realise the limitations imposed on the Committee, there are many users who rely on published test results as a basis of design, and, as such, I should have been greatly helped by the inclusion of these in the Research Report. The Izod results in themselves unfortunately are not a reliable guide as to the alternating properties of a given steel. I think if such results were for instance embodied in Fig. 2, page 110, "which shows the most instructive effects of mass," that they would add greatly to the value of these results from a design point of view.

Lord Invernairn (Sir William Beardmore) wrote: The Tables given in the Report showing the properties which can be expected from a given composition of steel when subjected to varying heat-treatments will be of great assistance to those engaged in designing

motor cars, aeroplanes or similar light structures. The magnitude of the work undertaken can be judged from the weight of steel supplied and the number of tensile, Izod, and Charpy tests made. The heat-treatment of this weight of steel, where the dimensions of the test pieces are considered, also the tabulating of the results obtained, must have entailed an enormous amount of labour, in respect of which the Committee deserve very great credit.

On reviewing the Report and the extracts made therefrom by the author, I was struck by the varying results obtained both in the analysis and in the mechanical tests by the different investigators, to which the author specially draws attention. The worst example in the Report is, I think, the analysis of steel 1L, which is given as follows:—

	S.	P.
1. Manufacturers' results	0·043	0·010
2. Independent analyst (ingots)	0·008	0·071
3. „ „ (billets)	0·007	0·048

I do not think it is to the interests of those engaged in metallurgical work to have results such as these published. Surely it was possible for the Committee to have investigated those discrepancies and to have reported only correct results. These could easily have been obtained in any well managed laboratory. There are several other cases where the discrepancies are outside those due to errors of analysis. Considerable variation is also shown in the mechanical test results obtained by investigators on the same steels, for example, Steel 2H, the 1⅛ in. bar, water hardened at 900° C., tempered at 400° C.; Steel 3L, the 1⅛ in. bar water hardened at 880° C., tempered at 600° C., and Steel 10L, the 1⅛ in. bar oil quenched at 820° C., tempered at 650° C. It does not appear to me that the Committee have made the best use of their opportunities, as the results quoted are outside the limits of experimental error, and the plotting of the results obtained and the drawing of a curve do not ensure a satisfactory X result. Would it not have been better, and have made the results tabulated in the Report of real value, if the reasons for those discrepancies had been investigated and their causes and the errors to be avoided during the course of heat-treatment embodied in the Report?

The author will help considerably towards a solution of the problem if he will in his reply briefly outline the methods employed by the various investigators, and the means used for taking the

(Lord Invernairn.)
temperatures. It will be of further assistance if he will state whether the test pieces were treated in a muffle furnace or in a furnace where the flame came in contact with the test pieces; if in the latter case, were the temperatures recorded with the gas off or on?

Some of the X results quoted, I think, want some explanation:—

1. 1L C. 0·06 Mn. 0·25 normalised at 920° C. gives a yield of 19 tons.
2. 5L C. 0·13 Mn. 0·41 Ni., 0·2 normalised at 900° C. gives only a yield of 18 tons.
3. 2L C. 0·12 Mn. 0·71 Ni., nil normalised at 900° C. gives a yield of 21 tons.

It will make the Report of greater value if the author in his reply will give the complete history of the test bars, namely:—

(a) In what type of furnace were the steels made, and were those acid or basic lined?

(b) The dimensions of the ingots from which the bars were made.

(c) Were the ingots allowed to go cold, then reheated for rolling, or were they lifted hot and heated direct for rolling?

(d) Were the bars forged or rolled?

In reply to the written communications, Mr. Dickenson wrote: With regard to Dr. Hatfield's remarks on the influence of sulphur and phosphorus content, he will find that although the steel 6H is much lower in sulphur and phosphorus than the softer steel 6L. the other comparison given. that of steel 2H with steel 2L. shows no very marked difference in the analysis, and yet the former gives markedly better impact values. I do not wish, as I stated on page 119, to over-emphasise this point, but certainly there is something else inherent in the steel, besides the composition, which must be brought in to explain some of these differences in toughness, and I am quite sure that Dr. Hatfield will agree with this view.

In reply to Mr. George Murray's questions, I would inform him that the possible basis of the present Report as the foundation on which further work can be done is not a matter for myself alone, and that all that it was intended to do in the present work was to obtain samples as nearly as possible to those specified in Report No. 75, and to establish their mechanical properties. The use which may be made of the data thus collected, and the prepa-

ration of a further report upon any subsequent specifications, is entirely a matter for the B.E.S.A.

The communication from Lord Invernairn raises three matters of very great importance, namely, the treatment by the Committee of the discrepancies in the investigators' results, the heat-treatment methods used by the various investigators, and the conditions of manufacture of the various steels.

The matter of the discrepancies has been dealt with earlier in my reply, but I would just add that it was the decision not merely of the Executive Sub-Committee, but of the Main Committee, of which Lord Invernairn has been a member from the outset, that the figures returned by the different investigators should be given exactly as they were received, and I venture to think that if obviously incorrect results had been checked by the same investigator, and then suppressed, the Report would have lost very much of its value in indicating the possibilities of error in current analytical and testing practice. In any case, the variations quoted by Lord Invernairn are neither necessarily, nor probably, due to errors in testing alone, since the A, B and C results in each case were obtained from samples heat-treated by different men. Therefore it is not possible to state the exact limit of experimental error applicable, the figures themselves representing the "overall" variation, or experimental error, including both heat-treatment and testing. Viewed in this way, the results give exceedingly valuable information to those concerned in the commercial testing of steel. As the Report stands, the user is assisted by the indications of the Committee that certain figures are suspect, and he should have no difficulty in arriving at the truth.

The "X" figures throughout the Report were, except in the Charpy test, worked out to the nearest unit, so that the difference of 3 tons per sq. in. in the yield points of the normalised bars of 5L and 2L is no great matter, since the one might have been raised to 19 and the other reduced to 20. The difference in the maximum stress is only 1 ton per sq. in., a very satisfactory agreement considering that the two steels are much alike in composition if the nickel in 5L is considered equivalent to an increased manganese content. The high yield in 1L is, no doubt, to be explained by the segregation specifically referred to in the Report. In any case the results actually returned are there to justify the "X" figures.

I regret my personal inability to give the desired information

(Mr. Dickenson.)
on the methods used by the various investigators, or the details regarding the steelmaking processes, ingot sizes, and so on, without reference to the Main Committee who instructed the Executive Sub-Committee regarding the layout of the Report and the amount of information which should be given. If, at some future time, the matter is brought before the Main Committee, and this additional data collected and published, it would, no doubt, form a very interesting Appendix to the existing Report.

The following comments are extracted from a Review of the Steel Research Report, which appeared in "The Engineer," of Nov. 19, 1920:—

"These masses of figures are of interest as indications of discrepancies between different observers, but they are of more positive value as the material from which those responsible for the Report have deduced a series of curves beautifully executed in colours and put forward as typical of the steels to which they refer. . . .

"The volume is of great educational value, although the work has been to examine a series of steels in a systematic fashion and thus establish their properties—which were not previously unknown—rather than to explain the variable properties of any one, or all, the steels. The members of the responsible working committee would, no doubt, admit that the curves show anomalies which they are unable to explain; their work, however, brings such anomalies into greater prominence, and a careful study of them may inspire acute thinkers and careful experimentalists to search for the fundamental causes. This is by no means the least valuable feature of the Report, as previous efforts in the same direction compare unfavourably in point of completeness and are inferior in the point of accuracy. There is an impression of truthfulness about the curves which could not have been conveyed if the editors had attempted to force them into a nearer resemblance to straight lines.

"In one respect the Report is a striking indication of a change which appears to be taking place in the beliefs and commercial aspects of steel making. It does not state whether the steels were prepared by the acid or basic process, nor does it say whether they were made by the crucible, the electric furnace, the open hearth or the Bessemer converter method. Moreover, we are not informed whether rolled bars tested bore blue labels with gold

letters or white labels with common black letters: we do not even know which steel maker, amongst the long list of those who offered material, made a particular kind of steel. In placing the results before the public as typical properties of the separate steels to which they refer, is it to be understood that the responsible members of the committee, and particularly the steel making members of it, wish the reader to infer that, under certain guarantees for sound material, it does not greatly matter who makes the steel, nor by what process steel of a given composition is made and put into bar form? If that is so, we are a long way in point of progress from pre-war days, when the quality of a steel was defined in terms of an elusive element called 'body,' the visible warranty of which was a printed name and title on a more or less pretty label. The atmosphere of the Report certainly suggests that the properties of flawless steel are defined by the uniformity of its chemical composition and the known effect of certain thermal treatments; if the Committee stands behind that suggestion its 'research' has indeed ended in a discovery of very great value. If, on the other hand, the mountain of work recorded in this Report refers only to the actual twenty steels examined, and the results are not broadly typical of any other commercially sound material of like composition, then the Committee has been employed to small purpose during the last six years; and it has committed a serious error in omitting to label the steels in the conventional manner according to the process of manufacture, if not also with the maker's name. . . . .

"This vein of thought stretches wide, and if pursued will either fail or be hard up against some cherished preferences and prejudices. If comparative curves are to be trusted, the mechanical value arising from the use of a new steel making alloy can be determined. The salesman asking a tall price because his chromium steel has been physicked with vanadium, costing 50*s*. a pound, may properly be asked to adduce proof, in concrete curve form, that the multiple fifty shillings' worth of physic charged into the furnace has improved the steel to an extent worth paying for. Under these searching comparisons the real value, and not the advertised value, may come to light. To this aspect of the question the engineer may advantageously devote some attention, both to avoid 'spending money for that which is nought,' and to familiarise himself with a touchstone that may well be used in his own office to correct specifications and compare supplies.

(Steel Research Report.)

"The curves relating to carbon steels reveal a fact which is gradually making its way into the minds of engineers, viz., that the carbon steels used on account of cheapness may be enormously improved in the mechanical properties by well-selected process of heat-treatment until they rival the more expensive alloy steels, and are even superior to the latter steels in the untreated condition. This knowledge should be useful whether the steel is used for structural purposes or for tool making. Tool steels, it is true, are generally applied to cutting purposes, but how many of them break through failure at some point remote from the operating edge or surface? Rock drills, sates, boiler-makers' snaps, and similar tools break down as often through failures in the head or shank of the tool as through any other cause, and the remedy may be found in strengthening the tool, not by making it harder, but by enormously increasing the toughness by heat-treatment as these curves show is possible. There is also no reason why alloy steel prepared primarily for structural purposes should not also be used with economy and increased reliability for certain types of tools. . . .

"The alloy steels dealt with are either nickel steels or nickel chromium steels. There are six nickel steels and six nickel chromium steels. On the assumption that these steels are those most widely used by automobile builders, there may be some justification for the choice. From an educational point of view this limited choice is unfortunate. If chromium enhances the value of nickel steel, why should it not enhance the value of carbon steels, and if manganese is an important element in all the steels, why should a suitable amount of it not produce a rival to the mild steel containing 2 per cent of nickel? These questions are not raised grudgingly. The engineering public has good cause to be grateful for what the report contains; the most important feature of it, however, is its suggestiveness and the means it discloses of answering in comparative curve forms such questions as are here raised. . . ."

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

DECEMBER, 1920.

The Fourth Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1, on Wednesday, December 8th, 1920, at 8 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following list of Members who had completed their election since the previous Meeting was posted up:—

MEMBERS.

THOMAS BLAIR SOMMERS (Glasgow).
JAMES ARTHUR GALBRAITH (Manchester).

ASSOCIATE MEMBERS.

ROBERT BAMFORD (London).
CLEONE DE HAVENINGHAM BENEST (Miss), (Birmingham).
RICHARD PARKINSON BRADLEY (London).
GEORGE WILLIAM COTSWORTH (London).
FRED LEON CREAGER (Beloit, U.S.A.).
CYRIL CHARLES HENRY DURLACHER (Brighton).
GORDON GRAY (Glasgow).
THOMAS HUTCHINSON (Newcastle-on-Tyne).
FRANK GREENWOOD JAGGER (Manchester).
EDMUND LASHBY MAYALL-EMTAGE (London).
GEORGE JACKSON MEAD (New Brunswick).
PHILLIP SEYMOUR ROBINSON (Chipping Norton).
ROMESH CHAUDRA ROY (Manchester).

ASSOCIATES.

HARRY ASHLEY BENNETT (Nottingham).
FREDERICK GEORGE BRISTOW (London).
MAURICE CHAPMAN (Glasgow).
GEORGE FRANCIS O'RIORDAN (Hull).

GRADUATES.

REGINALD NOEL KENDAL AVELINE (Coventry).
DONALD DALRYMPLE BORLAND (Croydon).
CHARLES HENRY CLAUD BURROWS (Coventry).
GEORGE HERBERT DAY (Coventry).
RUPERT ANTHONDY WILSON JONES (Coventry).
DAVID WILLIAM MUNRO (Birmingham).
HENRY NIGEL ST. VALERY NORMAN (Cambridge).
EDWARD BRIAN PLATT-HIGGINS (Coventry).
ROLAND FREDERICK SEALE (Coventry).
RONALD MIDDLETON WHITEHEAD (Huddersfield).

PROBATIONARY GRADUATES.

PERCY DONALD COLE (Birmingham).
WALTER SUTTON DAVIES (Birmingham).
WILLIAM ARNOLD DEAKIN (Birmingham).
VICTOR ALBERT FORD (Birmingham).
HAROLD ERNEST GOULD (Birmingham).
LESLIE BONHAM HARRIS (London).
ARTHUR JAMES LIDSEY (Derby).
CHARLES DENNIS MALE (Birmingham).
ROY MOUNTFORD (Coventry).
HARRY FREDERICK RICHARDSON (Birmingham).
CHARLES HENRY RIDING (Birmingham).
GILBERT RUSSELL (Birmingham).
ARTHUR EDMUND SPARKES (Birmingham).
HERBERT LESLIE VINSEN (Luton).
THOMAS HOWARD WROUGHTON (Mansfield).

TRANSFERS.

WILLIAM HUGHES BUTTERFIELD (Birmingham), Associate Member to Member.
WILLIAM HERBERT SMITH (East Molesey), Associate Member to Member.
LEONARD GRIFFITHS (Coventry), Graduate to Associate Member.

The Crompton Medal for the Session 1919—20 was presented to Mr. A. P. Young and Mr. H. Warren for their paper on "The Process of Ignition."

The following papers were then read and discussed:—

"Roads and Vehicle Maintenance," by Col. R. E. CROMPTON, C.B.

"The Combustion of Naphthalene Solutions in Internal Combustion Engines," by L. S. PALMER, M.Sc.

There were present 96 Members and visitors.

ROADS AND VEHICLE MAINTENANCE.

By Col. R. E. B. CROMPTON
(Past President).

Although the author, Mr. Mallock, and several others have read papers before this* and other Institutions on the Road and the Vehicle, there are points which, in the interests of road traffic, still require to be discussed, and experimental work undoubtedly is urgently needed.

The theory and practice of the construction of road surfaces has made great strides since the bicycle, the motor car, and, finally, the commercial road vehicle, have successively so increased in numbers, that the automobile traffic is now the chief factor in the cost of road maintenance. Bad road surfaces are costly to maintain and repair, and are the cause of heavy maintenance costs of the vehicles that use them, so that improvement of the road surface is equivalent to a reduction in the cost of vehicle upkeep. We have gone far since the days when the softness and consequent heavy draught on our roads in winter reduced the winter traffic to a low figure; in fact, we have already reached a point when the amount of summer and winter traffic has almost reached equality.

We have seen the first improvements effected by tarring and sanding the originally waterbound surfaces partly superseded by the adoption of bituminous surfaces laid in one or more coats, and by the carpeting of our best roads with an impermeable and elastic bituminous mixture, we have obtained surfaces on country roads almost equal to the best asphalt that we have in our towns. Finally we have seen the great improvement which affects our second class roads, of supplementing the annual tarring process by "chipping" the entire surface, so that a protective coating is added to the surface annually or biennially.

* See Proc. I.A.E., Vol. II., p. 319, and Vol. VII., p. 403.

The altered value of money—the increase in the cost of materials and labour—has so enormously increased the cost of both road and vehicle maintenance, that we engineers ought to consider all possible means of reducing the damage done to our roads by wheel traffic.

In the present paper the author proposes to discuss the general question of wheels rolling over a road surface. In early days before road surfaces were rendered waterproof by tarring and by carpeting with materials bound by bituminous matter, road engineers used to talk about the "wear" of the road caused by the traffic. This wear took place by the actual removal of material caused by the grinding of the stones of which the road crust is composed. This grinding away of the angles of the stones took place chiefly during the winter or during wet seasons, and caused the spacing apart of the stones, so that under the traffic they moved in relation to one another as their angular points were ground away; thus the stones gradually approached a spherical form, whilst the material ground from them formed the mud which was scraped away in winter, or was blown away in clouds of dust, during the dry season. At that time, therefore, the road really *did* wear as material was removed in considerable quantities in some degree proportional to the traffic.

When the new forms of traffic, the bicycle, then the motor car, and, finally, the commercial vehicle carrying goods and passengers, appeared in turn on our roads, the dust nuisance of itself during the summer traffic forced upon us tarring and other dust preventatives. This tarring was commenced a few years previous to the formation of the Road Board, and our knowledge was sufficiently advanced in April, 1911, for the Road Board to issue standard specifications for surfacing roads, using bituminous binders, as well as for the more superficial treatment by surface tarring.

During the nine years that have elapsed since the issue of those specifications, and notably during the first four years up to 1915, great progress was made in our knowledge of the various methods of producing waterproofed roads. During the war years the progress was not so rapid; still, some progress was made, and it is the object of this paper to bring together a few of the important matters which we feel that our road engineers ought now to consider so as to deal with them in the most economical manner.

We must accept as axiomatic that the wear of the road material

grinding away under the action of the traffic in wet weather now no longer exists in well constructed bituminous road surfaces; the deterioration of these surfaces which must be met by surface renewal is of another order. It takes the form of deformation, that is to say, that the rolling action of the wheels over the surface, although the material worn away may be small, causes the surface to extend, just as sheet metal extends by being lengthened and widened under the rolls of the rolling mill, or as pastry extends under the rolling pin. The spreading is longitudinal in the direction that the wheel rolls, and it is to some extent lateral whenever the sides of the road are not properly supported by kerbs or other means to take the lateral thrust. The spreading or forward rolling which tends to move the whole mass of the road surface forward in the direction in which the wheel rolls is resisted by the mass of the road surface in front of it, so that the form that the original flat surface invariably takes is that of the waving or corrugations of which we all complain. It is the author's object to show that the capacity of the road surface to resist the deformation caused by this forward rolling action largely depends on the extent to which the road crust itself is confined at the sides, and that if sufficient support is given at the sides, the tendency of the forward rolling action to form waves is correspondingly reduced.

In the year 1911 the Road Board ordered a considerable number of trial lengths to be laid down near London to test the capacity to resist wear and deformation of various methods of road surfacing that it was then proposed to use. Attempts were then made to measure the wear and deformation by providing at a number of points on the test lengths means by which the surface of the road could be accurately measured from wires stretched across to serve as datum lines. It was soon seen that with these methods intended to measure the extent to which the surface of the road was lowered by wear, no such rate of lowering could be ascertained because the surface of the road became deformed, that is, it rose in some places above the original level and was depressed at other points. Straightedges of various forms, some of them provided with self recording apparatus, gave us the actual contour of the road taken longitudinally, as well as transversely, and were found to give the best indication of the extent to which various parts of the road were deformed from their original surface contour as first laid down.

It is seen that wherever support in the shape of continuous kerbing is given to the sides of the road the tendency to waving is reduced, and in this case the actual waving, although small, is fairly equal in extent across the whole width of the road, but wherever the surface is badly supported at the sides the centre of the road shows least waving, whereas the sides, especially the outer portions, called by the roadmen the "haunches," show the undulations in their most exaggerated form; in the case of wide roads, where the up and down traffic naturally divides itself, wave formation on all roads without kerbs is worst at the haunches, especially on a hill, on the downhill side.

For a long time it was not an easy matter to fix any point in the road surface so as to measure its movement outwards from the centre towards the haunches, but during recent years the author has been able to note and measure the positions of hexagon nuts, which had fallen from the machinery of passing vehicles and had become embedded in the bituminous surface, and thus for many months were convenient points by which to observe the direction and extent of the movement of the part of the surface.

All these observations are in accordance with what we should expect in view of the nature of the ordinary road surface, and it will now be convenient to remind members of the general features of the process of resurfacing as it is still carried on, on the majority of the roads of the kingdom, in fact on all roads other than a few main lines of thoroughfare where the original road crust has been replaced by various forms of two coat work.

In most cases the process of resurfacing a worn out road is as follows. The old surface is not patched or brought up to contour but an attempt is made to bring to the desired contour by varying the thickness of the coating of broken stone spread upon it, that is to say, that the coating is thickest over the hollows and thinner over the high portions of the original worn surface. After spreading roughly to contour the roller is passed once over it, and then for the purposes of consolidation fine material called "hogging" is spread over it. It is then heavily watered, and the whole is rolled until the desired contour and finish are obtained. A few weeks or months later, when the surface is fairly dry, it is swept and tarred, sanded, and in a few cases chipped and rolled again. During the period which intervenes between the first process of recoating using hogging and water, and the second process of waterproofing by tarring, part of the hogging is removed, the amount varying with

the weather; if wet weather intervenes the hogging that is washed out by rainfall, aided by the traffic, is increased. Thus if the period of wet weather which intervenes between the recoating and the waterproofing by tarring is considerable, more of the hogging will have been washed away, and the stones being no longer equally spaced by the intervening hogging are free to rearrange themselves, which they invariably do under the impact of the wheel traffic into the wave form, that is to say, the unsupported stones crowd themselves together at the crests of the waves and the spacing between them is increased in the depressions. Even under the most favourable circumstances, when the tarring follows almost immediately on the recoating, the stones not being originally rolled into close contact are to some extent free to rearrange themselves, and thus cause waving.

Now let us consider what happens to roads formed as above described when wheel traffic passes over them. We find that even at very slow speeds smooth rolling action never takes place, in all cases the wheel progresses over the road by a series of leaps or bounds, the distance between the points where the tyre leaves the road and strikes it again increasing with the speed. Every time the wheel strikes, a blow is given to the road surface and this impact effect is the agent which pushes the stones apart, brings them into new positions, *and is the real and sole cause of wave formation.*

From this it will be seen that every effort should be made by the road engineer to insure that when he recoats his road the stones forming the coating are not temporarily supported by hogging but are really forced into close mechanical contact by the roller, and this can only take place, as has been already said, by giving sufficient side support and thus preventing spreading at the haunches.

It will therefore be seen that in all methods of surfacing the cause of wave formation is the same, in all cases it is due to insufficient mechanical interlocking or close contact of the individual stones when the coating is first spread and rolled, and that this interlocking contact cannot be obtained unless adequate side support be given. Wherever such side support has been given, where footpaths with stone or concrete kerbs separating the footpath from the road have been carried down to a sufficient depth to resist the side thrust, excellent results have followed.

It was due to some of our enterprising road surveyors, of whom Mr. Alfred Dryland is a notable example, that this provision of kerbs to take the side thrust of the traffic has been extended to country roads, and the notable improvement in the Surrey roads during the time that he was County Surveyor is largely due to the extent to which he has provided kerbing, filled in side ditches, and taken other means to provide adequate side support to the county roads.

The author is of the opinion that if a careful survey were made of the whole of the roads of the United Kingdom it would show that a very large share of the total cost of annual upkeep is expended on those lengths of the roads which are lacking in side support, and that there is no way in which money could be better or more usefully spent than in providing kerbs on main roads and in providing side supports on minor roads by filling in side ditches and pipeing in the side drains.

Any motorist who keeps his eyes open when driving over our roads, and who naturally notices the difference in quality of the road surfaces he passes over, cannot fail to see for himself that practically and invariably, the bad lengths of road are those low-lying in the valleys where water accumulates and must be taken away by roadside ditches. Wherever such low-lying roads cross naturally wet land and the road is found to be in good condition, he will notice that the road authority has there wisely spent money on pipe drainage so that the abutment to the road surface has been given by the filling in of the ditches which previously existed. Where such ditches continue to exist he will see that the road is really carried on an embankment, the height of which is the depth measured from the crown of the road to the bottom of the side ditches.

This consideration of the necessity for side support has led some of us to study the whole question of developing kerbs or longitudinal abutments to road surfaces. Mr. Drummond, in Renfrewshire, who has had to deal with very heavy traffic (heavy in the sense that a large number of vehicles loaded with heavy weights mounted on wheels of small diameter are continually passing over his roads to serve the great Glasgow and Paisley workshops), has shown us that in addition to kerbs at the two sides, when the roads are wide and have to carry great weights, extra longitudinal supports which take the form of submerged

kerbs have given excellent results, and have reduced deformation to a surprising extent.

As a large proportion of our main roads are 18 feet wide between kerb and kerb, it is probable that such submerged kerbs are not likely to be introduced into these roads, although the author has been informed of several cases where excellent results have been obtained from submerged kerbs laid in roads less than 18 feet wide, especially where these roads have passed through marshy country and are on embankments. When, however, roads are wider than 18 feet he feels certain that it would be decidedly advantageous to use submerged kerbs with a space between them varying from 12 to 14 feet, that is, from 6 to 7 feet measured on either side from the centre line of the road.

Kerbing, whether of the ordinary form dividing the footpath from the road or the submerged kerbs just described, would in those parts of the country where stone is plentiful take the form of dressed setts, but for the submerged kerbs much lighter stone will suffice. In the South of England and any other places where stone is not readily available, satisfactory kerbs of both classes can be carried out in reinforced concrete, and Mr. Dryland has been highly successful in introducing kerbs of this kind at a moderate cost. Mr. Drummond's kerbs, however, have been uniformly made of dressed setts.

Another point in road construction which appears to be gradually establishing itself is that the old method of repairing a macadam road with a coating of irregular thickness, so that at one point the stones as originally spread may be three stones thick, in another two stones thick or less, is wrong both in theory and practice. Roadmen thought it less trouble in this way to fill in the holes of the old surface and bring up the new surface to the desired contour. The best practice is, however, undoubtedly to patch very carefully all the low places, take great pains with the patching so as to bring the old worn surface nearly to a true contour surface, and then to apply a coating of stone *only one stone thick* and wedge it so firmly by chips of suitable size forced down between the stones that the stones are forced into such close mechanical contact that they cannot move, and finally to cement them in this position by pouring in pitch, bitumen or similar binders at a very high temperature. A new coating is thus provided of equal thickness throughout resting on the trued surface

of the old road, which offers far greater resistance to the deformation or waving, which generally showed itself at a maximum at points where the new coating was of extra thickness.

These remarks have been advisedly confined to those road surfaces which form nine-tenths of the roads of the United Kingdom; for these roads it is practically certain that existing methods are not likely to be greatly changed or supplemented except by the introduction of machinery to lessen the cost of labour and material when carrying out existing methods.

It has, however, recently become evident to the author that the tarring and chipping above described can be carried out by machinery and extended so that the coating of tar and chips, which at present when rolled down by the traffic only adds a veneer $\frac{1}{4}$ in. thick or so to the road, can by using $\frac{3}{4}$ in. chips mixed with finer gauges be brought up to a coating up to $\frac{3}{4}$ in. thick, and could be applied to a road and finished right off by machinery requiring little hand labour and giving an excellent and durable surface; it follows that if this process can be perfected it will be an alternative means of maintaining the surfaces of roads which may turn out to be more economical both of material and labour than any of the existing methods in use. At any rate, the cost of labour on road work has now reached a point at which we engineers must use our best efforts to adapt machinery to maintaining our road surfaces by reducing the labour bill to the lowest possible point.

The Vehicle.

We now turn to the side of the question which concerns us automobile engineers, the cost of vehicle maintenance as influenced by the road surface. It is up to us to consider the best means that can be adopted to reduce the damage done to the vehicle by cushioning the impact of the wheel at present and future travelling speeds. We must take the whole range of vehicles from the heavy tractor or motor waggon moving at speeds of 4 to 12 miles per hour, the faster motor lorries, omnibuses and char-a-bancs travelling up to 25 miles per hour, and the still faster motor car running up to the maximum speeds that may be in the future considered safe and possible on open highways.

The author has endeavoured to show that the effect of wheel

traffic on road maintenance may be measured by the impact blow given to the road surface by the wheel, and it is evident that this is equally a measure of the damage done to the vehicle or to its load by the shocks arising from these blows. We now wish to know how these impact forces may be determined so that we may devise a formula which may be of assistance to designers which will give the effect of varying the wheel diameter, the nature and width of tyres, the extent of cushioning or shock absorbing effect obtained at various points taken in order, starting from the point of contact at the road surface up to the load itself, that is to say, the cushioning of wheels themselves by their tyres, the use of shock absorbers at the axle, and then comes the whole range of springing and the shock absorbers which are combined with springs.

It will be seen that this problem is a very complex one; the author has been considering it for fifty years, in fact, ever since the late R. W. Thomson, of Edinburgh, first introduced elastic tyres, as they were then called, and which were solid rubber tyres of great cross section. From that date, which we may fix at the year 1870 up to the present year, which is the jubilee year of rubber tyres, a controversy has raged on the cushioning effect of the various classes of elastic tyres and how far they supplement or take the place of refinements in springing the vehicle. For many years the road authorities would not admit that the use of soft tyres of any kind was of benefit to the roads; in fact, they urged on the contrary that the sucking effort of the rubber on a dusty or on a muddy road by causing the removal of material was actually worse for the road than the crushing effect of rigid wheels which, they said, to some extent acted as rollers. During the last few years, however, road authorities have come round to see how much it is to their interest to encourage not only rubber tyres but improvements in the springing of vehicles, and there can be little doubt that eventually differential charges will be levied on vehicles reduced in proportion to the efficiency of the cushioning arrangements.

The author has been at some trouble to read what has been written and discussed elsewhere than in the papers read before this Institution, but he finds that those who really have studied the springing of vehicles have kept their knowledge very much to themselves; he believes that this is really due to the innate complexity of the problem, and that the laws which govern spring-

ing and cushioning are known to very few in this country, and it is for this reason that no reasonably simple formula has been

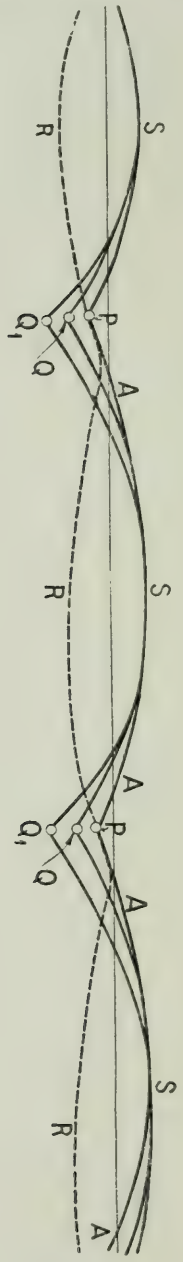


FIG. 1.

devised to determine the impact force of a vehicle when the varying factors of weight, speed, wheel diameter, and methods of cushioning have to be dealt with.

In illustration of the difficulties of arriving at such a formula the author has prepared Fig. 1, where RRR represents a waved road surface. If we take the case of the wheel of an unsprung waggon, not only the wheel itself being rigid, but the whole, i.e., the wheel and the vehicle having the rigidity of an ordinary commercial unsprung vehicle, the path of the wheel centre running over the road RRR will follow the line AAA, the cusps at the points PP will be sharp, the contact being instantaneous, so that if the wheel and vehicle were absolutely rigid the pressure would be infinite, and either the wheel or the road would be shattered. But although all roads are to some extent elastic with unsprung vehicles on rigid tyres the actual pressures at the points PP will be great even at low speeds and excessively damaging both to road and vehicle at high speeds. This elasticity of the road will modify the path AAA and the path of the centre of the axle will somewhat resemble RQRRQR. If, however, we go further and introduce cushioning, the same class of curve will be followed by the wheel centre, but the points QQ will be lowered to Q1Q1. In the former case the road only is temporarily deformed, in the latter case both the road and the vehicle are temporarily deformed by the cushioning. It is evident then that in order that we may calculate the strain on the road and on the vehicle, in each case we ought to be able to measure these three vertical distances and the vertical velocity V.

Our American friends have attempted with some success to use the cinematograph to determine this relative movement of those points of a sprung vehicle which move relatively to one another at the time when the wheel strikes an obstacle in the road. Mr. Alfred Masury, Chief Engineer of the Motor Engineering Co., America, wrote a paper giving useful data obtained by running motor waggons fitted with solid and pneumatic tyres over a prepared track in front of several sets of motion picture apparatus. He marked on his vehicles by spots of white paint the points which he wished recorded on the film and provided other means for obtaining synchronous measurements of the relative movements of the parts. He gives a formula for the force of impact F, which the author here gives for what it is worth:

$$F = \frac{W\left(\frac{22ST}{15}\right)^2}{64{\cdot}32D} + W$$

where F = Impact force in ft.-lb.
W = The weight of the vehicle in lb.
S = Speed in miles per hour.
T = Tangent of angle of incline of wave crest.
D = Distance the axle drops in feet.

Mr. Masury says that this formula can be applied to the unsprung weight of a motor truck by taking the impression of the tyres and the extent to which the road surface is deformed, which must be added to the sprung weight by measuring the spring and tyre impressions.

The author has used Mr. Masury's own words, as he undoubtedly is entitled to the credit of giving us an up to date means of calculating impact force, but he leaves out so much which appears to the author important, one factor being the diameter of the wheel, that his formula does not greatly help us. However, it is so evident that by using his method of observance valuable information would be obtained which would be of great value both to the designer of the vehicle and to the road engineer who wishes to know which class of vehicle it is which most affects his repair bills, that the author strongly urges that this cinematograph method of measuring impact forces should be given a trial here, and makes a formal proposition that this Institution should join with the Society of Motor Manufacturers and Traders and with the Ministry of Transport to jointly bear the expense, which need not be great, of carrying out a well-considered series of tests of all the representative types of vehicles which use our roads, by running them over a portion of a road already sufficiently deformed to represent a bad case of waving. There must be many such points on main lines of traffic near London which would be suitable for such a series of tests. It is reasonably certain that the data obtained will be of high value and that the cost of this combined road and vehicle research will be very small as compared with the value of the results obtained.

In this paper the author has endeavoured to put before the Institution in a convenient form for discussion the case of the road maintenance which applies to three-quarters of the mileage of the roads and streets of the United Kingdom, and to the very difficult question of determining the damage done to these roads by various classes of automobile traffic.

It has been purposely condensed by the author from lengthy memoranda prepared during the war years. He however believes that the Members will find that it gives ample scope for a full discussion of the two questions of the day: first, how to set about maintaining our roads at the least cost to the ratepayers, and the road users who are to be taxed to aid in their maintenance, and, secondly, to suggest means by which we can ascertain the extent to which the widely varying classes of self-propelled vehicles now using our roads contribute to damaging their surfaces.

THE DISCUSSION.

Dr. W. R. Ormandy, in opening the discussion, said: I want to add my plea to that of the author for the necessity for research, and the co-ordination of that research, on the part of the automobile engineer and those who are responsible for road making. A great deal of work has been done in America in connection both with the lateral support of roads and the provision of waterproof foundations, upon which waterproof topped roads shall be built. The problem that they are faced with there is very much more severe than any we have here since with their very great variations in temperature a waterlogged road is liable to almost complete disintegration. A great deal of work in connection with the use of cement foundations and ferro-concrete foundations and waterproof concrete is accordingly being carried out over there, which will be of great benefit to us if the information is properly made use of. The Americans have also investigated the effect of impact by running motor wagons of the same gross load, but with a difference of unsprung weight, over specially prepared surfaces and allowing them to drop off $\frac{1}{2}$ in., 1 in., 2 in., and 3 in. boxes on to a portion of the road which is contained in a manhole, with means for measuring the pressure. They have found that the impact is not a direct function of the speed, nor is it a function of the square of the speed, but something in between. The amount of the actual blow, for instance, from a $5\frac{1}{2}$-ton army lorry dropped 2 in., with an unsprung weight of 1,000 lb., gave an impact pressure of 23,000 lb., whereas with a lorry of the same gross weight but having an unsprung weight of 1,800 lb., the impact was of the order of 38,000 lb. The experiments were carried out by running the vehicles over various blocks, with an inclined piece placed at certain distances away, and the various vehicles ran up this and over the pedestal and into the road, and dropped on to a carefully softened copper block, the deformation of which was used to measure the impact.

It seems to me that as we have to pay very heavily for the roads we are, at any rate, entitled to a say in the matter of their construction; on the other hand, the road engineer may quite reason-

ably say at the present time that he is given no figures or anything to go on. In America, however, they have realised the necessity for co-operation between the road engineer and the automobile engineer, and the Government Departments which have to do with the two aspects of the question have members on each other's committees, and they have decided that for certain roads the maximum economy to the inhabitants at large will be arrived at by using vehicles not exceeding a certain weight and having not more than a certain unsprung weight. In other words, although by employing a very heavy vehicle the costs of traction may be reduced for the time being, the result is an enormous increase in the cost of upkeep of the roads. It does not seem to be practicable to use a vehicle which is too small in order to keep road expenses down, but by co-operation it may be possible to arrive at that relationship which will be of maximum benefit to both industries. The one point that I do wish to urge is the necessity for taking notice of this question of unsprung weight. It was found, for instance, by running a certain vehicle at varying speeds and allowing it to drop on to the impact measuring device, that the impact rose steadily with the speed up to a certain point when there was a sudden rapid increase in the impact, and at speeds still higher the impact increased again steadily with the speed, possible due to some synchronising of the vehicle springs.

Mr. T. Clarkson: The lateral displacement of the road to which the author has drawn so much attention is now becoming fairly well recognized by road engineers, and it stands to reason that if that road surface can be anchored, jammed or wedged more firmly in position, its tendency to flow will be checked. The tendency to lateral flow, for instance, might be checked if the foundation of the road were made in the form of a series of longtitudinal serrations giving the appearance of a ratchet in cross section, the faces of the ratchet on each side being turned towards the centre of the road. I see some difficulties in providing a steam roller to produce that sort of effect and yet be suitable for rolling the top surface afterwards, but that is only a detail. It must be clearly understood that economical transport depends on two things, a well designed vehicle and a suitable road for that vehicle to operate upon, and the best vehicle without a good road is obviously useless. The real point is that we must try to keep the periphery of a wheel in constant contact with the road surface. The question of preserving a uniform surface is one for the road engineer, but the

(Mr. T. Clarkson.)

vehicle engineer has got to devise means of improving suspension, and no doubt a great deal more can be done in that direction than has yet been done. In the cars of lighter and medium weight the use of the cantilever spring has been possible, and it may be that we shall find a greater development in the use of cantilever springs for the heavier class of traffic, but only by the co-operation of the road engineer with the designer of the vehicle shall we get that condition which undoubtedly means a reduction in the operating expenses and which will make it possible to carry a ton a mile for some fraction of a penny less than is possible to-day.

Mr. H. L. Heathcote: For some years it has been my lot to have to concern myself with wire wheels and ball bearings, which, coming as they do between the vehicle and the road, suffer from the ills which both vehicle and road are heir to. I would like to say something about the genesis of the waves to which the author refers and a word or two about the formula attributed to Mr. A. Masury. With regard to the latter we are not told whether it is derived from, or embodies, the experimental results of the tests recorded with the help of the cinematograph, or whether it is based upon assumptions as to the movements, the forces concerned and their rate of variation. At first sight I thought it might be derived from experiments, but on a closer look the fraction 22 15 became 88 60, the denominator 2gD, and rising hopes were dispelled. The formula is approximately what is arrived at if it is assumed that the wheel axle, etc., weight W, travelling at S miles per hour, is raised D feet as the result of the tyre meeting an obstacle, the path followed being assumed to be a parabola with its vertex at the point where the axle *begins* to rise.* Under these assumptions the force of the impact is dependent on the radius of the wheel, for, as its radius decreases, the distance of the obstacle from the centre of the contact surface between the ground and the tyre also decreases, and the time required to mount is less. The upward acceleration, therefore, is greater for the smaller wheel. This is

* The expression for the force (F) during impact is

$$F = 0{\cdot}1336 \frac{W S^2}{2R - D} + L;$$

in which R is the radius of the wheel in feet, or more precisely the distance from its centre to the obstacle it is mounting, D the height of the obstacle in feet, S the speed in miles per hour, W the weight of the mass lifted by the obstacle, and L the load sustained by the wheel. F is measured in lb. when W and L are in lb.

implicit in Mr. Masury's formula if the tangent T drawn to the wave at its crest coincides with the tangent to the wheel.

We will next consider the author's account of wave formation. On page 171 we are told, in italics, that it is the impact of wheels that causes wave formation, and that in all cases the wheel progresses by a series of leaps or bounds, the tyre actually leaving the road. As it stands, the account against the motor vehicle looks very black, but the author has not given us any of the data upon which these opinions are based, doubtless with some good reason. We are, therefore, debarred from discussing the matter in the ordinary way, for instance, considering whether the data justify such lugubrious conclusions, and we are restricted more or less to agreeing or disagreeing with the theory which the author puts before us. I cannot agree that on a newly made road surface the wheel leaves the ground or that it progresses by leaps or bounds. Nor do I believe that the action of the wheel on the road is such as to produce permanent deformation just as sheet metal extends under the roll of the rolling mill or pastry under the rolling pin. The only time I have observed anything akin to such effects on a roadway was when watching a steam roller "making" a road. How, then, does the road surface become so uneven? To answer this we will consider first what will happen to the wheels of a car when they are driven with uniform velocity along a newly made or comparatively new road free from obvious surface defects. On the parts of the road surface, if any, which are level or have a uniform gradient the wheels will proceed with linear and angular velocity which are very nearly uniform. When the car comes to a short part of the surface which has a sightly greater gradient than before, its body will tend to travel forward in a straight line, and still with the same velocity. The wheels, however, will climb and then descend the surface, and in so doing will traverse a longer path than the body. The wheels, therefore, must possess a greater angular and linear velocity over the part of the road in question than on the part they have just left. The acceleration of these velocities requires the application of force, and the only place where this force can be applied is at the tyre and by the ground. If we consider the matter a little more closely we can see that the force of the tyre on the ground—which, needless to say, acts in the direction of the linear acceleration of the wheel—may be very considerable. If the change of the gradient be very small but sudden, unless slipping or bending

(Mr. H. L. Heathcote.)
of the rubber occur or elastic twisting of the rim of the wheel relatively to the hub or yielding of the ground, the acceleration of the angular velocity will be infinitely great. The wheel of a car travelling at 24 miles per hour mounting a short incline of the road surface, 12 in. base and 2 in. high, will possess a linear velocity of 24·33 miles per hour. If this were imparted in the time time required to climb the incline to a Rudge-Whitworth wire wheel fitted with a Dunlop tyre having a moment of inertia of 58·43 sq. ft.-lb., the force at the ground required to produce the acceleration of angular velocity would be 14·1 lb. If the wheel were a rear one, to the moment of inertia of the wheel we should have to add that of the brake drum, axle and other rotating parts affected by the acceleration of the wheel. It is, however, far more likely that the acceleration takes place in a much shorter time than that required to reach the top of the incline, and I think we may expect forces of 200 lb. and more, in fact, sufficient to cause slight skidding of the wheel at high velocities. When the top is reached, if flat, the angular velocity of the wheel will be reduced just as suddenly to its original figure. On descending the other side of the slope it will be accelerated again and decelerated once more at the bottom. If I have made my view clear it will be apparent that as a direct and inevitable result of even slight variations of the gradient of the road surface the action of wheels over it results in considerable forces tending to shear and rub the surface layers of the road sometimes forwards, sometimes backwards. These forces come into existence whenever the gradient changes, or, in other words, whenever the wheels rise or fall relatively to the body, and they will be greatest at the bottom of an upward slope. In my opinion it is the action of these forces that leads to the exaggeration and multiplication of the slight defects in the road surface as delivered to the cyclist, motorist and commercial car driver for use. If this view of the matter is sound, it is not only a credible sort of theory to account for road surface depreciation, but a working hypothesis which may be of great service in helping those whose concern it is to give us good road surfaces. For instance, the author has told us of the excellent results given by roads which are supported longitudinally by kerbs at the side. The hypothesis now put forward suggests that any device which will minimise the undulations left by the road makers will have the effect of lengthening the life of the surface. Providing side support will, I think, certainly minimise the

undulations produced and left by the roller. It may be that the proper way to finish a road is to grind it true, like we do the races of our ball bearings.

Mr. F. W. Lanchester: In connection with the author's description of the proper way to make a secondary road I must admit that it would never have occurred to me to do other than peck up the old surface and then roll in a new layer of metal. I understand now from the author how stupid a proceeding that would be. The idea of repairing the road first and then applying a layer of new metal without pecking or breaking up the old surface, would not, I think, have entered my mind; but after hearing the author's lucid argument I feel quite sure that he is right.

The author talked of the need for research, and spoke of what is being done in America. Candidly, I am no believer in Committee research, though there may be a certain type of routine research work that can be effectively done by qualified men directed by a competent Committee, but in my opinion there are very sharp limitations as to what can be accomplished; moreover the method at the best is scarcely economical. It is nevertheless pleasant to think that something is being done, or at least attempted, and very important that it is recognized that the road cannot be considered without at the same time considering the car and vice versa.

I disagree with Mr. Heathcote who referred to the secondary variations in the motion of the wheel on the road (due to the varying inclinations of the surface) as being the cause of the road undulations. I rather lend the weight of my opinion, such as it is, to the view that the greater erosion or wear in the trough of the undulations is due to the fact that the centrifugal force of the unsprung weight is added to the gravitational load, whereas on the cusps or ridges of the undulations the centrifugal force is subtracted from the gravitational load. It is only necessary to assume that the wear of the road surface is related to the effective load at every instant to realise that this must have a great tendency to exaggerate any waves in the surface that may originally exist.

Stated broadly, the condition of a road surface from this standpoint takes its place naturally amongst many other dynamic phonemena. The perfect road surface is a surface which, in view of its usage, is in unstable equilibrium just as in discontinuous fluid motion a surface of discontinuity is in unstable equilibrium; any wave motion or irregularity once instituted tends to become

(Mr. F. W. Lanchester.)
more and more exaggerated. Other examples that may be cited are the surface of a sea in the presence of a wind, which is a particular case of a surface of discontinuity separating two fluids, and in which the discontinuity is one of density as well as motion. Again the phugoid oscillation in a gliding model or flying machine which is unstable by reason of its too great amount of inertia is another case in point. The fact that the wave on the road surface is cusped is not inconsistent with there being an underlying wave of sine curve form, since the cusped wave in a Fourier analysis would give the sine curve as one of its main constituents; but, further, if we fix our attention on the track of the wheel centre when passing over a cusped wave form it is far more near to being a sine curve; it may be that the movement of the wheel centre or axle is the motion of real dynamic importance when the surface waves are in course of formation.

Mr. W. Worby Beaumont: With regard to this question of the waving of roads, I agree with nearly all the author has said, much of which leads to the conclusion that it is the paviour we want to make roads and not the work of the tip wagon and the steam roller run over materials, which are often extremely difficult to press or to get into anything like a uniform mass such as is required for a roadway. Road material, when only broken to the size of a man's hand, or half that size, is to a great extent wasted material when it is put down on to a road and rolled in with the idea of getting it into anything like a compact mass that shall remain a compact mass. Even when broken into much smaller sizes, the pieces which apparently do settle down into a happy position amongst each other are, many of them, only touching at the corners, and the result is that the road materials as far down as 9 in. below the surface have been found, when taken up after a comparatively few years, to have had their corners rounded off. This movement of the material below the surface rapidly grows when the road is made of materials that will not, to put it shortly, compact, and the author is, therefore, quite correct when he says that a great deal of the movement that occurs in the surface of the road and leads to the waving afterwards under traffic, is due to the materials put down being pressed by the steam roller in a way which only compacts it so far as the surface appearance is concerned. The author comes to the conclusion that every time the wheel leaves and returns to the road surface a blow is given, and that this impact effect is the agent which pushes the stones apart and

brings them into new positions, and that that is really the sole cause of wave formations. That is generally true with regard to roads made of materials of that kind, but it is not the sole reason. In some of the London streets which have really good, solid concrete beds, and a well laid surface of bituminous sandy material, this wave formation makes its appearance in spite of the fact that there are no movements of small pieces in the road which are free to move because they have rubbed against each other and become of less dimensions. In this case the corrugations are due to the rolling action of the loaded wheels that run over the surface, and not to shifting of the materials. It is not quite the same thing, but it is analogous to the corrugation that occurs on railway rails, which depends on several factors, including hardness and toughness of the particular materials.

With regard to the formula mentioned by the author, there is little difficulty in stating precisely what would be the impact stresses brought about by any conceivable or actual conditions of the working of the vehicle on the roads so far as this relates to speed and weight, but the modifying effect brought about by the relief under spring compression followed by intensification of the secondary impact by the recoil of the spring at, say, 1,300 ft. per second calls for much less simple treatment.

Colonel Crompton, in replying on the discussion, said: I am sorry that the road makers who might have had something to say in defence of the present methods of maintaining our secondary roads have not cared to defend their position.

In reply to Dr. Ormandy, I know that American roadmakers are working hard at the road problem, and I am in general touch with them. Their conditions are, however, widely different from ours. For instance, it may be a very suitable method of forming new roads for America to make them of cement concrete, but it is difficult to apply here, simply because, as a rule, we cannot close any existing country roads long enough to give time for the concrete to set properly, so as to obtain its proper strength before the traffic goes on to it. For this reason there is not one road in fifty where cement concrete would have a chance.

We have already good foundations in our existing road crusts: what I point out is that it is a pity to disturb these. It is better to utilise them as foundations and put on the top of them a modern elastic surface, and this applies pre-eminently to our secondary

(Colonel Crompton.)
roads, which are the roads I have been talking about, and for which the ratepayers cannot afford to pay more than £200 per mile for annual repair. For such roads I have given chapter and verse, and I challenge any road engineer to controvert what I have said.

Mr. Heathcote complains that I do not give sufficient data in support of what he calls "my lugubrious conclusions" regarding wave formation. I thought that it was generally admitted by road engineers at any rate that waving is *always* eventually set up in every kind of road surface, whether it be granite pavement, wood pavement, asphalt or our most improved country road surfaces. Wherever fast automobile traffic comes on to it wave formation invariably appears. The question I now bring forward is, how we engineers are to set about minimising it, both by more scientific road construction and by scientific springing of our vehicles or by modifying our tyres.

I must repeat emphatically that however perfect the surface on which the wheel rolls may be, in practice smooth rolling action never exists, for even on the extremely perfect rail track of the Naval Tank at the National Physical Laboratory, although the rail surfaces and those of the wheels themselves are as perfect as possible, the wheels never roll smoothly enough to insure perfectly continuous electrical contact between them; the smallest particle of sand or dust will cause a bounding or jumping action to commence, but of course in this case the amplitude and height of the bounds is small; still it always exists, and increases as the wheel and the surface on which it rolls fall away from perfection. Again, on the road machine at Teddington we were able closely to watch and study the action of wave formation; from time to time we took casts of the surfaces we were testing, and these showed how trifling cross corrugations of small amplitude and height gradually extended themselves into waves of considerable length and height. Again, when omnibus services have been diverted from main roads under repair over suburban macadam roads in good order I have found that on the average within twenty days of the service commencing waves would reach a length of 16 in. from crest to crest and a height of over $1\frac{1}{2}$ in. These waves always take the cusped form and not the sine wave form which mathematicians would like them to take.

Mr. Clarkson's suggestion that the lateral spreading of the upper crust of the roads might be checked if the upper surface of the

foundations were prepared with longitudinal serrations has not escaped the notice of some of us: in fact Mr. Drummond's submerged kerbs is one way of giving effect to this idea, and it has been successful.

I have to thank Mr. Lanchester for dealing with Mr. Heathcote's theories, and for putting into language far better than my own the principles that underlie the re-arrangement of the materials which compose the road surfaces into the waved condition, and I am glad to hear that the cusped form of wave that we actually observe might be expected from the mathematician's point of view. I have to point out that his opinion coincides with that of several eminent mathematicians whom I consulted as to the possibility of obtaining a formula showing the effect that an existing cusped road wave modifies the impact force of the wheel, which does so much to originate this class of road deformation.

Mr. Beaumont appears generally to agree with me in my conclusion as to the nature of the movement of the road material which allows of the road deformation.

COMMUNICATIONS.

Lt. Col. D. J. Smith wrote: I should be glad if the author would make some statement regarding a point which has given me much thought recently and which is probably one which is occupying the attention of many other automobile engineers, but which has not been dealt with in the paper. The greater number of motor vehicles in this country are in use in or about our large cities and towns. It, therefore, follows that the greatest mileage is run over roads intersected by tram-lines, and I should be pleased to know whether the author thinks it is possible to construct a satisfactory road for motor vehicles if provision is also to be made for trams. It appears to me to be practically impossible. If this is so, it is a very serious matter, considering that motor users are being heavily taxed for the upkeep of roads, which are, to a large extent, rendered unsuitable and dangerous by the presence of tram-lines. At the present time there is hardly a mile of road in the London district on which tram-lines are laid, which is not being pulled up in one or more places for attention to these lines. The most frequent trouble appears to be the joints between the rails, and practically wherever these rail joints occur, the road will be found to have sunk deeply on both sides. The running of tram-cars, weighing as much as 17 tons loaded, at speeds often exceeding 25 miles per hour, on rails set rigidly in concrete, is totally opposed to all railway practice and must result in the breaking up of the road foundation. On railways the rails are laid on a flexible base formed by wooden sleepers laid on ballast, and I think that in view of the fact that the motor user will now practically have to pay for the maintenance of the roads in this country, some action is necessary to prevent the destruction of these roads by the presence of tram-lines and the very heavy wear and tear caused to all motor vehicles by the bad condition of the roads due to the presence of these lines, to say nothing of the danger involved to all users of the roads.

Mr. F. Strickland wrote: What the author says about the action of the vehicle on the road appears to me to emphasise the importance of good springing. During the war there were an

enormous number of breakages of vehicle springs and I made some calculations as to the various properties of the different springs in use. These calculations were not very accurate as it was impossible to get the exact weights on the axles, but I do not think the errors were enough to vitiate the conclusions.

It appeared that the main reason why springs broke was that they were too stiff and consequently when they deflected, as they were bound to do under road shocks, the stress became greater than any steel could be expected to stand. Now the pressure of the wheel on the road must be equal to that of the load on the spring or slightly greater. Taking 3-ton lorries for comparison as being vehicles of fairly the same type and weight, the deflection of the front springs under load, by calculation, varied from under 1 in. to practically 4 in. The deflection of the back springs varied from under $1\frac{1}{4}$ in. to 4 in. It is obvious that if a spring deflects 1 in. under load it will take three times the static load to deflect it 3 in., that is 2 in. beyond the static position. On the other hand if the deflection of the spring under load is 4 in. it will only take $1\frac{1}{2}$ times the static load to deflect it 2 in. beyond the static position. It is hardly possible that any vehicle can run at ordinary speeds over a moderate road without the springs deflecting 2 in. or so, and it will be seen that the greater the deflection of the springs, the less the pressure put on the road, the tyre and all the axle parts, etc.

The figures I worked out give a fairly accurate comparison, and show the very great diversity of practice among different makers. In the case of cars, weights had to be still more of an estimate, but I calculated that the deflection allowed in the springs of the W.D. vehicles varied from $\frac{1}{2}$ in. to $3\frac{3}{4}$ in. for the front springs and from 1 in. to 7 in. for the back springs. Making every allowance for errors this is quite an unwarrantable variation.

It appears to me that springs should in many cases be longer and broader, with thinner leaves, and that if this were done the wear of the road would be reduced, the vehicle would be made more comfortable, the wear of tyres would be reduced, and breakages of springs, etc. would be less frequent. This does not apply to the best cars which are very well sprung, but to many of the lorries and cheaper cars.

Mr. A. E. Parnacott wrote: I should like to quote, as expres-

(Mr. A. E. Parnacott.)

sing an opinion with which I agree, a statement made by Dr. Unwin,* in the discussion on another paper by the author on this subject, namely, "that the problem of the action of the wheel on the road is much more complicated than the author appears to assume." Fig. 2 represents a 4 ft. 8 in. gauge axle with a 32 in. wheel, and it will be seen that when the wheel drops say two inches there is a lateral movement of the tyre tread on the road of more than one inch. This lateral movement of the tyre tread on the road is, I feel sure, the chief cause of tyre tread wear. All road surfaces are very far from being even, and the least depression, or a projecting grating, cause lateral movement and consequent stress. Now we can well imagine that a load of some hundredweights imposed by the tyre tread on the road, coupled with this

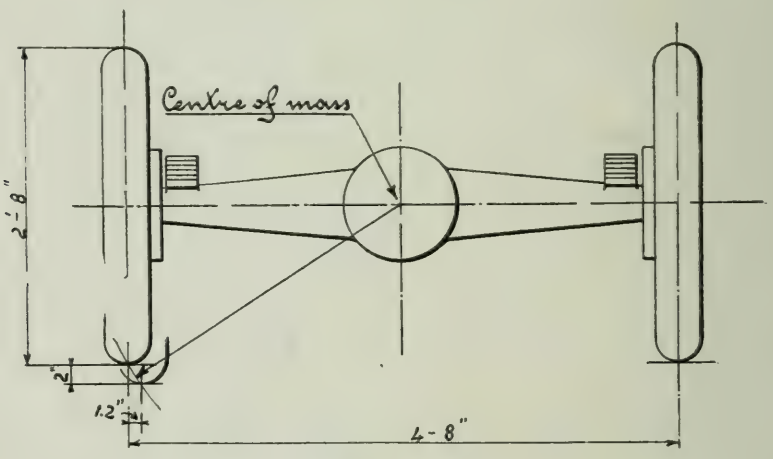


FIG. 2.

lateral movement, has a considerable "wriggling" effect on the stones forming the road surface. In 1912 I built a small vehicle with the axles so arranged that this lateral movement did not take place; the net result is that the power required to propel the vehicle is small and the tyre durability extraordinary, far exceeding that of others of similar power and load.

I consider that the author has exaggerated the benefits of kerbs to bituminous road surfaces. On the contrary I have noticed that kerbs are not the cure of wave formation, though I think that Mr. Clarkson's suggestion may be the nucleus of a practical

* See Proc. I.A.E., Vol. VII. p. 434.

scheme. With rigid axles the lateral movement of the wheel relative to road surfaces, which I have indicated above, will of course accentuate the movement of road surfaces, and must materially assist in wave formation.

I have noticed that the wave formation is commonly started by the pitching due to a depressed crossing, a grating, or even an irregularity of the road surface, and I think we automobile engineers ought to try and impress on road engineers the great importance of even surfaces. In my opinion the wedging action of stones mentioned by the author, coupled with the small resistance to shear existing between the crust of the road and its foundation and the lack of elasticity of the road surface when necessarily depressed by the load, tends to tilt over the stone wedges, which are double ended excepting the top surfaces, until they are piled into crust-headed waves, which cause the wheel to leave the ground and bump on to the following wave. Probably with rigid axles the greater the unsprung weight and the larger the diameter of a wheel of a given gauge the quicker will be the wave formation. I wish to suggest that we should learn a lesson in road making from the art of bricklaying. The load impressed upon a brick in a wall is distributed more or less through the whole of the bricks below and in a pyramidal form. What a contrast this is to the author's wedges! I would therefore suggest that roads for heavy traffic might be built up of concrete or small bricks grouted in with cement or bitumastic or equivalent grouting.

Wave formation is, in my opinion, the direct consequence of the necessary depression of the road surfaces due to rolling load. I do not believe, to quote the author, that the stones are pushed apart, but rather that they are tilted, and that this is the root cause of wave formation. Nor do I believe that stones can be forced into such mechanical contact that they cannot move; for is it not an axiom that nothing can resist a force unless it yields in doing so?

In reply to the written communications, Col. CROMPTON wrote: As regards Mr. Parnacott's remarks, I think that the action he describes does take place and does contribute to the pushing sideways of the material on all roads which have insufficient side support, but what surprised me when watching the effect of the experimental work, where we were using not only side but sub-

merged kerbs on the Renfrewshire roads, was how this prevention of side movement had the most extraordinary restraining effect in minimising wave formation. There is no doubt that these experiments proved that this side restraint has this effect, and this is one of the reasons why I speak so strongly in favour of it.

THE COMBUSTION OF NAPHTHALENE SOLUTIONS IN INTERNAL COMBUSTION ENGINES.

By L. S. PALMER, M.Sc.

I. Introductory.

The following work arose from the suggestion that the heat content of the usual oil fuel used in internal combustion engines would be increased by dissolving acetylene gas in the fuel.

The expected increase in B.Th.Us. per pound of fuel can readily be calculated if the coefficient of absorption of acetylene is known. This was measured experimentally by recording the volume (corrected to N.T.P.) which a known volume of solvent would dissolve. The coefficient of absorption a, or the volume of gas taken up by unit volume of liquid at the given temperature when the partial pressure of the gas was 760 mm. was then calculated. With commercial benzol as solvent, a for acetylene was found to be 4·1 and 1·6 with petrol as solvent.*

If the density of benzol is 0·88 grams per cc., one gram of solvent will absorb 0·00546 grams of acetylene. Taking the lower heat values of benzol and acetylene as 17,500 B.Th.Us. per lb. and 19,000 B.Th.Us. per lb. respectively, the increase in heat content of the benzol solution would be about 34 B.Th.Us. per lb. of fuel, or an increase of 0·02 per cent. In the case of petrol, where the coefficient of absorption is smaller, the increased heat value of the solution would be still less.

The two fundamental assumptions underlying these calculations are (1) that there is no volume change when absorption takes place, and (2) that the heat evolved upon complete combustion of the solution is the sum of the heat values of the solvent and solute.

From these figures it is apparent that in order to get a solution with an increase of any practical value, a greater weight of solute

* See also "Recueil de Constantes Physiques."

with high heat content must be used. Undue increase in expenditure must also be avoided if the resulting product is to be of any value commercially. Acetylene under pressure may fulfil the first condition, but the increased expense and the danger if used with copper pipes prohibits its application in practice.

Experiments were therefore carried out with naphthalene solutions in benzol, with the following objectives:—

(1) To obtain an efficient cheap fuel of high calorific value; and (2) to investigate the possibility of the efficient combustion of solid fuels by means of solution.

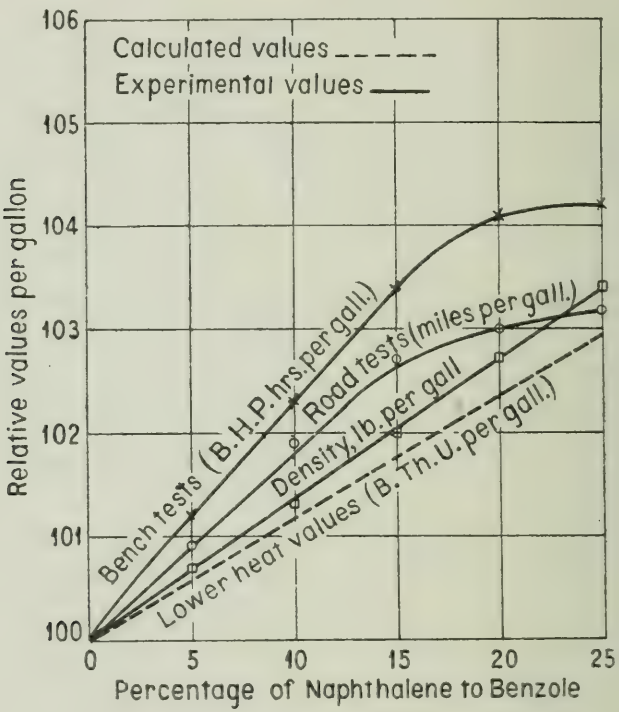


FIG. 1.

II. PRELIMINARY CALCULATIONS.

(1) *Solubility.*

The solubility of naphthalene in benzol and petrol is given in Table I., from which it can be seen that at ordinary temperature a solution containing as much as 30 grams of naphthalene to 100 grams of benzol might be used in practice. In carrying out the above measurements, the decrease in temperature during solution indicated that the assumption that there is no volume change is in this case not correct.

(2) *Density.*

The density of a 25 per cent solution (i.e., 25 grams of solute to 100 grams of solvent) would be 1·1 if volume changes were

TABLE I.

Temperature in degrees Centigrade.	Grams of $C_{10}H_8$ per 100 grs. Benzol.	Grams of $C_{10}H_8$ per 100 grs. Petrol.
10	27·5	8·7
15	31·5	10·9
20	36·0	13·8
25	40·5	17·0
30	45·5	20·6

TABLE II.

Fuel (lb. of $C_{10}H_8$ per 100 lb. of Benzol).	Density (grs. per cc.).	B.Th.Us per gallon.	Relative lower heat content per gallon.
0	0·865	126200	100·0
5	0·871	127000	100·6
10	0·877	127700	101·1
15	0·883	128500	101·8
20	0·888	129200	102·4
25	0·895	130000	103·0
30	0·900	131000	103·7

neglected. The actual density of this solution was found to be 0·895. The densities for different strength solutions are given in Table II. and are plotted for comparison in Fig. 1.

(3) *Calorific Values.*

Assuming 1 lb. of amorphous carbon combines with 32/12 lb. (i.e., 2·67 lb.) of oxygen to form 3·67 lb. of carbon dioxide and gives out 14,544 B.Th.Us. per lb., and that 1 lb. of hydrogen combines with 16/2 lb. (i.e., 8 lb.) of oxygen to form 9 lb. of steam with the evolution of 61,524 B.Th.Us. per lb., we have for the lower or effective heat content of benzol (C_6H_6):—

$$(72/78 \times 14544) + (6/78 \times 61524) - (9 \times 966 \times 6/78) \text{ B.Th.Us.}$$
$$= 17483 \text{ B.Th.Us. per lb. of benzol}$$

where 966 B.Th.Us. is the latent heat of condensation of steam.

Similarly, the lower heat value for naphthalene ($C_{10}H_8$) will be:—

$$(120/128 \times 14544) + (8/128 \times 61524) - (9 \times 966 \times 8/128) \text{ B.Th.Us.}$$
$$= 17200 \text{ B.Th.Us. per lb. of naphthalene.}$$

The effective heat content of a 5 per cent solution can then be calculated. Since 1·05 lb. of solution evolve 17483 + 17200/20 or 18343 B.Th.Us., 1·00 lb. of 5 per cent solution yields 17500 B.Th.Us. approximately.

Given the solution density to be 0·871 grams per cc., the heat content in B.Th.Us. per gallon is 127000. Similar data for the solutions to be tested are given in Table II., and the last column is plotted in Fig. 1.

The last column represents the B.Th.Us. per gallon reduced in such proportion that benzol is taken as standard and represented by 100.

III. Experimental Work.

In order to determine experimentally the possibility of burning naphthalene by conveying it in solution to the cylinder, and to see with what completeness the combustion can take place, a series of road and bench tests were undertaken. The engine available for this purpose was a $2\frac{3}{4}$ h.p. two-cylinder Douglas motor cycle engine.

(1) *Road Tests.*

Two series of road tests were undertaken, both following the same general lines, namely, a given distance was traversed at approximately constant speed, and the volume of each fuel consumed was accurately measured.

The second series differed from the first in two respects: (a) the speed was regulated in the first series by means of stop watch readings at frequent intervals, and in the second by means of a Bonniksen speedometer, which gave the rate every 4·98 seconds. This latter method proved quite satisfactory. (b) Samples of exhaust were taken during the second series and analysed for carbon and oxygen content.

Details of the experimental arrangements are shown in Figs. 2 and 3, Plate II, where H represents three small auxiliary tanks with gauges reading to 0·1 cc.; I is the tap cutting off the carburettor hot-air jacket from external air; J is the collecting vessel for exhaust sample connected by a small bore pipe to the silencer L: K is the main silencer with valve which closes either when gas is being collected in J or when back pressure in K tends to drive air into L, which is the mixing silencer ensuring average samples from both cylinders. M is the main exhaust, N the supplementary exhaust from J, and O the exhaust from the carburettor hot-air jacket.

Method. The collecting vessel J (Fig. 3) was exhausted, having been made air-tight by taps at either end. This method had to be adopted since collection by any of the usual methods over mercury was not practical on a motor cycle. Vessel J was then fixed as shown in Fig. 3 and the tanks H (Fig. 2) were next filled with the solution to be tested and the gauge reading noted with the cycle on the stand. A given level road from two to three miles in length was selected and chained. The carburettor and pipes were flooded and the run made to the starting point with fuel from one auxiliary tank. On passing the point, the fuel supply was taken from the tank of which the level had been previously noted. On completing the run, the taps were reversed. The volume of fuel in the float chamber, pipes, etc. was thus eliminated. The consumption was noted and the miles per gallon were determined (see Fig. 6). If a gas sample was required, a tap between J and L (Fig. 3, Plate II), was opened and the valve in K automatically closed whilst J was filled from L.

Before testing a different strength solution, the tanks, carburettor and pipes were emptied and cleaned. The gas for analysis was displaced from J and collected in an Orsat apparatus, in which the percentage of CO_2, CO and O_2 was recorded approximately.*

* See Proc. I.A.E., Vol. II., p. 101.

With this apparatus only about 80 to 90 per cent of the CO is absorbed.

(2) *Bench Tests.*

For these tests the engine was mounted as shown in Fig. 4, Plate III.*

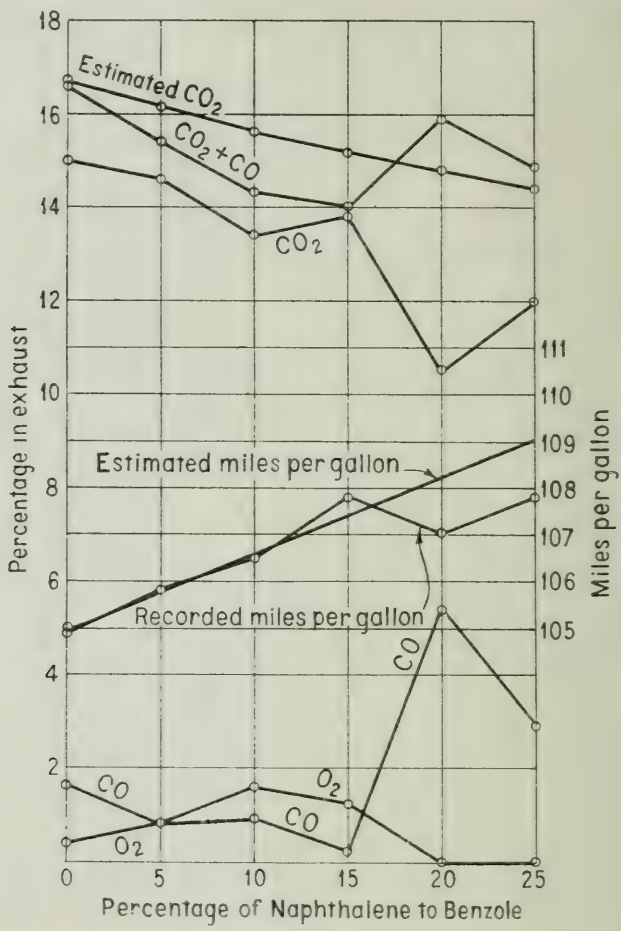


FIG. 5.

A—Fixed engine bed containing—
B—Floating engine bed pivoted in A.
C—Gear wheel of revolution counter.
D—Exhaust which can be connected to a graduated gasometer.
E—Three-way tap to fuel supply.
F—Fan brake attached to flywheel for absorbing power.
G—Fuel measuring cylinders and tanks.
W—Weight (variable) to balance counter-torque of rotating fans.

* See Proc. I.A.E., Vol. VII., p. 535.

A fan for air cooling is shown in the background.

The volumetric efficiency was first found with the floating engine bed clamped. The torque of the fans with the engine floating was then balanced by a weight W of such magnitude that the speed necessary was about 1500 revs. per minute. A constant torque was then assured as long as the engine was kept balanced, and for given atmospheric conditions the revolutions would be kept constant for a given weight.

The constant K in the expression B.H.P. $= K N^3$ (where N = revs. per minute)* was determined for each set of readings.

In these experiments the amount of fuel used was kept constant,

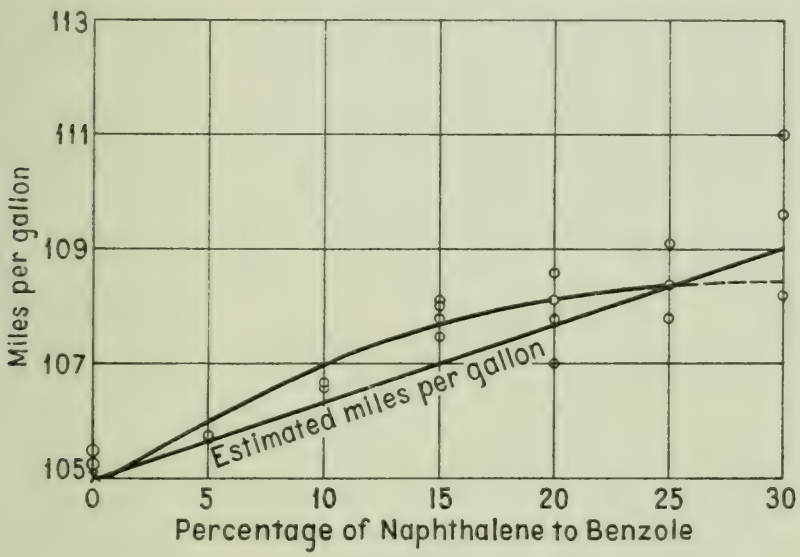


FIG. 6.

whilst the time taken was recorded on a 1/100 sec. stop watch. The total revolutions were also counted for the given test with a revolution counter—not geared to the engine shaft as shown in Fig. 4, Plate III.

From these observations the times taken for the consumption of 25 cc. of fuel at 1500 revs. per minute were obtained and the b.h.p. hours per gallon calculated.

IV. RESULTS.

(1) All results have been compared with benzol, which has been taken throughout as standard, and the actual and estimated

* See Proc. I.A.E., Vol. VII., p. 537.

TABLE III.

Test No.	Fuel.	Mean Rate miles per hour.	Consumption.		Exhaust Analysis.			Calculated percentage of CO_2 for complete combustion.
			cc. per 2·25 miles.	Miles per gallon.	Percentage of CO_2.	Percentage of CO.	Percentage of O_2.	
18	Benzol	16	130·0	104 9	15·0	1·6	0·4	16·7
22	5 per cent	15	128·9	105·8	14·6	0·8	0·8	16·1
26	10 ,,	15	128·3	106·5	13·4	0·9	1·6	15 6
28	15 ,,	15	126·5	107·8	13·8	0·2	1·2	15·2
20	20 ,,	15	127·5	107·0	10·5	5·4	0·0	14·8
30	25 ,,	16	126·5	107·8	12·0	2·9	0·0	14·4

improvements calculated on the assumption that benzol is represented by 100 (Table VI.).

(2) Table III. gives the data for a typical road test with gas analysis. The figures are plotted in Fig. 5. The mean results of 26 road tests are shown in Fig. 6, and from the curve the values in column 4 of Table VI. have been determined. Throughout the experiments the 30 per cent solution was found to be impractical on account of the separation of naphthalene in the colder parts of the carburettor which choked the air inlet. This happened slightly with the 25 per cent solution before a hot jacket carburettor was fitted to the engine. On a run of 24 miles with a 15 per cent solution a very slight deposition of naphthalene in the carburettor was observed. This was cleared easily by running a mile on benzol only. The solution which passed the cold air inlet did not deposit in the induction pipe and appeared to be completely burnt, giving a clean exhaust. When starting with a cold engine on 20 per cent or stronger solution a white exhaust was noticeable for the first one or two explosions. This rapidly cleared as the engine warmed up, and was probably due to unburnt but finely divided naphthalene. No overheating was noticeable, particularly when the magneto was advanced slightly above normal. There was no excessive deposition of carbon in the cylinders or exhaust pipes.

(3) For the bench tests the volumetric efficiency was determined by clamping the floating bed (Fig. 4, Plate III) and connecting the exhaust to a graduated gasometer which was allowed to fill at atmospheric temperature and pressure. The stroke and bore of the engine are 60 mm. and 61 mm. respectively, from which, knowing the total number of revolutions, the volumetric efficiency for 1500 revs. per minute was found to be about 70 per cent.

The constant K (see Section III.) was found to be about $0{\cdot}55 \times 10^{-9}$, giving a horse-power at $N = 1500$ of 1·86. According to the manufacturers the horse-power at these revolutions should be 1·9 with a new engine.

The mean results of a typical set of experiments are given in Table IV. and summarised in Table V. Table VI. is a comparative summary of all the results. These data are plotted in Fig. 1.

TABLE IV.

Fuel.	Mean Values per 25 c.c.				
	Observed Values.			Calculated Values for revs. per minute = 1500.	
	Time in secs.	Total revs.	Revs. per min.	Time in secs.	Total revs.
Benzol	121·4	3040	1492	120·0	2990
5 per cent	123·9	3098	1483	121·4	3024
10 ,,	124·3	3110	1490	122·7	3056
15 ,,	126·0	3150	1490	124·1	3100
20 ,,	123·2	3085	1510	124·9	3125
25 ,,	126·1	3156	1486	125·1	3135

Constants.	Fuel.	
	5 per cent to 20 per cent.	25 per cent.
K	$0{\cdot}555 \times 10^{-9}$	$0{\cdot}560 \times 10^{-9}$
W	5 lb.	5 lb.
Barometer	759·4 m.m.	762·1 m.m.
Temperature	16·0° C.	16·2° C.

TABLE V.

Fuel.	Time at 1500 revs. per minute.		B.H.P. hrs. per gallon (hrs. x 1·86).
	Secs. per 25 c.c.	Mins. per gallon.	
Benzol	120·0	362·8	11·24
5 per cent	121·4	367·0	11·38
10 ,,	122·7	371·2	11·50
15 ,,	124·1	375·0	11·62
20 ,,	124·9	377·8	11·70
25 ,,	125·1	378·2	11·72

TABLE VI.

Fuel (lb. of C_{10} H_8 per 100 lb. of C_6 H_6).	Density (lb. per gallon).	Lower Heat Values (B.Th.Us. per gallon).	Road Tests (miles per gallon).	Bench Tests (B.H.P. hrs. per gallon).
Benzol	100·0	100·0	100·0	100·0
5	100·7	100·6	100·9	101·2
10	101·3	101·1	101·9	102·3
15	102·0	101·8	102·7	103·4
20	102·7	102·4	103·0	104·1
25	103·4	103·0	103·2	104·2
Source of Data.	Table I.	Table II.	Fig. 6.	Table V.

V. Discussion.

There are three outstanding and closely related conclusions which are apparent from the curves of Fig. 5.

(1) When the volume of CO in the exhaust approaches a minimum, the mileage approaches the estimated value which is proportional to the lower heat contents of Table II. When the mixture was rich, as with the 20 per cent solution, the combustion was much less complete, with the result that the mileage per gallon was reduced.

(2) As the volume of CO_2 approaches the estimated quantity, the mileage per gallon increased, and although the actual percentage of CO_2 never reached that estimated, the agreement is in accord with the assumption made on page 1, namely, that the heat content of the solution, upon which the estimated mileage per gallon is based, is the summation of the heat values of the separate constituents.

(3) The naphthalene introduced into the cylinder in the form of solution burns completely when the correct amount of oxygen is supplied.

From Fig. 6 the persistent falling off in efficiency with the stronger solutions is noticeable. This falling off occurred whether "full air" was supplied or not, and seems to indicate that the solutions of concentration greater than 15 per cent were either not completely burnt or were not introduced into the cylinder as concentrated as was supposed. Since these solutions tend to deposit naphthalene, this lack of efficiency is probably accounted for in the latter way. The difficulty of retaining the naphthalene in solution prohibits the use of stronger concentration than 15 per cent for ordinary practice. With the bench tests this deposition was just noticeable with 20 per cent solutions, and the b.h.p. hrs. curve of Fig. 1 shows a falling off from the straight line at this concentration.

The fact that this curve is above the straight line graph means that the calorific value of the solution is apparently greater than that calculated from the complete combustion of all the carbon and hydrogen present; that is, the combustion takes place more efficiently when the two substances are present as a solution.

In general, the following conclusions are again indicated from the b.h.p. hrs. per gallon curve of Fig. 1, namely:—

(1) That the efficiency is at least that expected from the

assumption that the calorific value of the fuel is the summation of the separate calorific values of the constituents.

(2) That complete combustion takes place, giving apparently more than this estimated efficiency for naphthalene solutions in benzol, the concentration of which does not exceed 15 per cent.

(3) That solutions above 15 per cent are not usable in practice with ordinary internal combustion engines arranged for petrol consumption.

Applying these conclusions to the case of petrol solutions of naphthalene, we see from Table I. that by comparison with benzol solutions a concentration greater than 5 per cent is not likely to be of practical use at ordinary temperatures. Assuming the lower heat value of petrol to be 18500 B.Th.Us. per lb., and given the density of a 5 per cent solution to be 0·73 grams per c.c., the calorific value of such a solution would be 112200 B.Th.Us. per gallon. Taking the density of petrol to be 0·72 grams per c.c., the B.Th.Us. per gallon will be 111200, which if represented by 100 reduces the heat value of the solution to 100·8. This increase is too small to be of much advantage in practice.

It is of interest to note that a certain firm is producing balls composed mainly of naphthalene and claiming a great increase in mileage by the use of only a few in a tank of petrol. It is difficult to see how these claims can be justified in the light of the foregoing experiments.

Another aspect of the problem is the question of paraffin, petrol and benzol mixtures. Much work has been done on such mixed fuels, and considerable difficulty experienced in burning the heavier component of the mixture.* The tendency to the formation of mists in the induction pipe can be overcome readily, only by the application of additional heat. In the case of petrol and paraffin, the mixing of the two liquids produces practically no temperature change, and the resulting total volume is consequently unchanged, whilst the density of the mixture is the mean of that of the two constituents. This means that no true solution has taken place, and hence, according to the present work, there is no reason for expecting complete combustion of the heavier fuel. With only mechanical mixing there is nothing to prevent mechanical separation. This appears to be an essential reason for the effective combustion of naphthalene solutions and the

* See Proc. I.A.E., Vol. VI., p. 371.

ineffective combustion of paraffin mixtures. In the case of paraffin and benzol there appears to be some evidence of at least partial solution taking place, for a drop in temperature and a slight increase in the resulting total volume point to more than a simple mechanical mixture. This is in agreement with the fact recorded by Dr. Dieterich,* that such mixtures are more easily burnt than the petrol-paraffin mixture referred to above.

The combustion of naphthalene alone in the various forms of naphthalene carburettors which have been devised, is also closely related to the present problem. With pure naphthalene additional heat is necessary in order to efficiently burn the fuel after it has been melted. By dissolving the solid it appears that efficient combustion can take place without the high temperature previously necessary and without the addition of the latent heat of fusion for every gram of solid melted.

VI. Commercial Aspects.

Assuming a mileage of 50 to a gallon of benzol, we should expect an increase of 3·4 per cent with a 15 per cent naphthalene benzol solution. At a cost of 2*s.* 9*d.* (June, 1920) per gallon, the rate per mile on benzol would be 0·66 of a penny. A gallon of 15 per cent solution would cost 2*s.* 6*d.* if naphthalene cost £7 a ton. This perhaps unexpected reduction is due to the fact that slightly less than 0·89 of a gallon of benzol will, with 15 per cent by weight of naphthalene, form one gallon of solution. At 51·7 miles per gallon the cost per mile will be 0·58 of a penny, or a saving of 8*d.* per 100 miles. This estimate neglects the cost of manufacture of the solution, which would be very little since the addition of the naphthalene is all that is necessary to make the solution.

Another point for consideration is the relative quantities of materials available and in what way they are at present being utilised. Available data are subject to frequent fluctuations both with respect to price and quantity. The production of benzol has considerably decreased since the war, during which abnormal supplies were required. The following figures therefore only approximate roughly to the present conditions. In England in 1914† approximately 70,000,000 gallons or 225,000 tons of benzol

* See Proc. I.A.E., Vol. VIII., p. 27.

† See Proc. I.A.E., Vol. VIII., pp. 23 and 29.

were produced and about 126,000 tons of naphthalene, of which, to convert the benzol to 15 per cent solution, only 33,600 tons would be required. Before the war most of the benzol was shipped to the Continent, but it is now much more extensively used as a motor fuel in England. The naphthalene is largely utilised by the dye manufacturers, but the figures represented above do not indicate the possible output by many thousands of tons.

VII. Conclusion.

From the foregoing work it is concluded that:—

(1) Naphthalene dissolved in benzol to a strength of 15 per cent by weight forms an efficient fuel for ordinary petrol motors, and though about 3*d*. per gallon cheaper yields 3·4 per cent more mileage than commercial benzol.

(2) A solution, rather than a mechanical mixture, forms an efficient method by which solid or heavy liquid fuels may be conveniently burnt in ordinary internal combustion engines.

The expenses entailed in this research were defrayed by a grant from the Colston Research Fund of the University of Bristol.

The author wishes to express his thanks to Prof. W. Morgan of the Automobile Department of the Engineering Faculty for constant advice and guidance, and for the use of the facilities of the Automobile Laboratory in which the bench tests were carried out.

THE DISCUSSION.

Mr. E. B. Wood, in opening the discussion, said: I was concerned in some of the experiments which are referred to, i.e., mixtures of petrol and paraffin, and I well know the difficulty we had, and the ease with which they invariably separated. I think one of the most interesting points in this paper is the behaviour of the solution and the fact that there is a volume change, and I would like the author to tell us whether the air cooled engine was used on purpose or because it happened to be the only one available. I think there might be a better chance of the naphthalene separating out if it was used in a water cooled engine with water jacketed induction pipes.

Professor W. Morgan: In 1908 I had an opportunity of trying out a carburettor designed by a Frenchman, Leon, which was adapted to use naphthalene on the ordinary four-cycle engine. The naphthalene, which was supplied in a fairly finely powdered condition, was fed from a hopper controlled by a ball valve actuated by a float, and fell down into the float chamber and was there melted, and afterwards dealt with as the ordinary liquid fuel. As the melting point of naphthalene is 80° C., and the boiling point is about 218° C., that involved fairly high temperatures. The engine ran quite smoothly and cleanly, and the exhaust was free from unburnt constituents, with an absence of carbon monoxide if the fuel was cut down a little, but it gave all the symptoms of a paraffin engine, i.e., the mean effective pressure dropped to about 75 per cent. of that obtained with petrol, and pre-ignition troubles occurred, so that, although in those days the price of naphthalene was something like a halfpenny or three farthings per lb., the complexity of the apparatus, the trouble of keeping things just right, and the temperature effects, killed the invention and nothing further was done.

With regard to the paper itself, I was interested as to the possibility of the addition of some substance to paraffin which would modify the vapourising qualities of the fuel and give, in effect, a vapour which would hold up at a comparatively low temperature, and the author suggests that something of that sort is taking place.

At first I was inclined to agree with him, but I think it is possible to suggest another solution for the completeness of combustion, and an explanation of the fact that naphthalene could replace benzol as a fuel. As one explanation, we can assume that by some physical reaction the naphthalene does hold up as a vapour below the temperature at which it might be expected to condense. I would suggest, however, that the fuel goes into the induction pipe partly evaporated and partly liquid, and I believe that it is quite possible that a liquid very rich in naphthalene is deposited and then creeps along the induction pipe on to the hot valve chest and hot induction valve of the engine. The author's experiments were carried out on an air cooled engine, and the volumetric efficiency of the engine, measured merely as a pump, was 70 per cent, which should be sufficient to give, assuming the engine has that volumetric efficiency when running, a mean effective pressure of somewhere in the neighbourhood of 85 to 90 lb. per sq. in. with even a low compression. On the horse power given by this engine, however, it must have been running on a mean effective pressure of 45 lb. per sq. in., which gives some idea of the temperature at which the charge must have been going into the engine. Another suggestion occurs to me, and that is that the boiling point of this substance is comparatively low, and would represent the boiling point of rather light paraffin, and since it is comparatively easy to run an air cooled engine on a carefully selected pure paraffin, it would appear that the whole behaviour of the naphthalene can be explained by the fact that it is thrown down in a very concentrated solution in the induction pipe, and that this then creeps into the chamber above the induction pipe and on to the hot valve, where it evaporates and so is in a condition for combustion. In support of this there is a reference on page 203 to the disappearance of a deposition of naphthalene in the carburettor after running on benzol alone for a few minutes, which rather suggests that the benzol washed away the deposit. I think, therefore, that a hot induction pipe would enable a bigger percentage of naphthalene to be carried simply by ordinary evaporation. The fact remains that if anyone has access to naphthalene in the form of a waste product, it can be used quite efficiently and quite economically in an internal combustion engine.

Dr. W. R. Ormandy: It seems to me that the end result of the author's experiments was that the amount of energy output was increased to an extent which was exactly proportional to the

(Dr. W. R. Ormandy.)
increase in the number of heat units in the fuel employed. I think it is too frequently forgotten that in any internal combustion engine we might as well regard the air as the fuel and the other substance that is used as burning up the air. Given the same volumetric efficiency, running under the same conditions, only a certain amount of oxygen can be got into the cylinders, and it is a fact that practically all the fuels that are used in internal combustion engines give out essentially the same amount of heat per unit volume of oxygen which they require for their combustion,* so that the end result that can be expected in every fuel under the same conditions of volumetric efficiency and under the same compression conditions is that the amount of power given is exactly proportional to the number of heat units contained in the liquid.

I cannot understand what the author means by emphasising the difference between liquids which merely mix and liquids which in some way tend to enter into combination. Naphthalene and benzol tend to enter into combination, as is shown by the fact that the volume of the solution is altered when the two are brought together. If that is so it means that a certain amount of energy has been used. A greater amount of energy is required to break these combinations up again, and on that account a slightly less output of power might be expected, but the amounts in question, reckoned in calories, are so small that they are negligible quantities. In a mixture of petrol and paraffin there is no diminution in bulk, but the real difficulty is introduced merely by the fact that the two liquids have entirely different vapour tensions, and it is a difficulty with the ordinary carburettor to keep the proportions right until the time when the fuel gets into the cylinders, but I do not see any reason why two materials which vary in volume when they are mixed should be easier to use than two which do not. It is much more a question of similarity of the vapour tension curve which counts. I should like to know where the samples of gas for the analysis of the exhaust was taken, because in my experience of exhaust gas analyses the analysis varies continuously from the outlet valve right down the exhaust pipe. In other words, the exhaust gases combine in the exhaust pipe, and particularly when working with alcohol under low compression it is possible to get 5 per cent oxygen or 6 per cent oxygen and several per cent

* See Proc. I.A.E., Vol. VIII., p. 55.

of CO on a sample taken with a 1/16th in. or a 1/8th in. copper tube inserted through the exhaust pipe just close to the exhaust valve, whereas if a sample is taken simultaneously from the exhaust pipe 2ft. away little oxygen or CO will be left.

Capt. A. C. Burgoine: I have been interested in some tests made on one of the commercial petrol enrichers, and have the records of the official tests made under the instructions of the Air Ministry. First of all I would like to say that our tests were made with a very small proportion of the enricher. The author worked up to 25 or 30 per cent, but the instructions on the cardboard box in which the enricher was sold said that one tablet was to be used in each two gallons of petrol, and they claim, I think, 25 per cent increase of power, a decrease in consumption, better slow running, a cooler engine, etc. The author's tests were made on a $2\frac{3}{4}$ h.p. air cooled engine, but my tests refer to a 360 h.p. water cooled engine. The engine was run from 1,200 to 1,800 revs. per minute on a Froude brake. Speaking bluntly, the result was nil. The specific gravity of the petrol with one tablet to two gallons, was the same as with the original petrol. The material was analysed, and was found to consist of naphthalene containing a small amount of colouring matter. The calorific value was stated to be 17,400 B.Th.Us., whereas the petrol alone was said to give 20,333. The average weight of a tablet was $3\frac{1}{2}$ grammes, which was to be added to two gallons of petrol. The melting point of this particular sample was 79·5°C., whilst that of pure naphthalene was given as 80°. The material possessed all the chemical properties of naphthalene, and, finally, I may say that the price at which it was sold was twenty-five times the price of refined naphthalene. May I ask the author if he has made any tests using petrol and naphthalene? His paper only refers to benzol mixtures.

Mr. A. F. Evans: I should like to ask the author if he has found that the use of naphthalene, either with paraffin or petrol, has any effect on the prevention of the formation of carbon on the pistons.

The President: When I was in the United States in 1918 I saw a petrol enricher which by simply holding my handkerchief, on which a little of the fluid had been poured, over the induction pipe, gave very marked results on an engine of 5 h.p. As shown by diagrams, taken under both conditions, the explosion appeared to be complete at the beginning of the stroke instead of taking

(The President.)
place at several points. I take it the matter has not even yet become a commercial proposition or we should have heard more of it.

Mr. L. S. Palmer, in replying on the discussion, said: In reply to Mr. Wood, there was no particular reason for adopting an air cooled engine for the tests. It happened to be the engine available. However, I do believe that a water jacketed induction pipe would lead to a little more separation of naphthalene. Such an effect is indicated by the improvement recorded after a hot jacketed carburettor had been fitted to the engine.

Professor Morgan referred to the Leon naphthalene carburettor. I did not refer to that as such, but mentioned a somewhat similar carburettor. Another form, covered by a German patent, bears even more directly on the present work. In this, a lump of naphthalene is periodically lowered into a stream of benzol as it passes the carburettor. This method allows no control over the concentration of the resulting solution, and I have no doubt that it gives very unsatisfactory results in consequence, although I have never tried it. Professor Morgan's remark that the completeness of the combustion may be due to the naphthalene tending to remain as a vapour in the presence of the benzol is a very helpful suggestion which has a certain amount of physical support. If such an action does occur, then the naphthalene would be prevented from separating out to the same extent as might be expected from considerations of the normal vapour tension curves, and the unexpectedly steep slope of the experimental curves could then be readily explained.

Dr. Ormandy rightly points out that no more heat units can be got out than were put in, but by introducing such units in the form of a solution it appears from these experiments that it is possible to utilise the particular units more efficiently than when they are introduced by using a mixture of substances; that is, the present problem is one of completeness or incompleteness of combustion due to the different forms in which the fuel is introduced. With regard to the volume change, since this is an increase and not a decrease, the fuel contains more energy as a solution (hence the cooling which takes place during solution) and therefore Dr. Ormandy's criticism of the expected output of power should be reversed. With regard to his last point, the gas sample was

taken from the mixing silencer L (Fig. 3, Plate II) directly in conjunction with the two cylinders so as to avoid getting a sample from one cylinder only. The method of collecting by means of a vacuum rather than over mercury did not, I think, appreciably alter the oxygen content. The automatic valve in the main silencer was used in order to reduce this tendency to a minimum. The fact that the analyses are in agreement with the mileage curve in Fig. 5 supports this conclusion.

Capt. Burgoine's remarks were extremely interesting. That the result of the experiments he described were nil was to be expected, since one tablet in two gallons of petrol with a 360 h.p. engine would cause a difference far less than the errors involved in the experimental methods of testing. In fact, Capt. Burgoine's statements support the paragraph on page 207, namely:—"It is of interest to note that a certain firm is producing balls composed mainly of naphthalene and claiming a great increase in mileage by the use of only a few in a tank of petrol. It is difficult to see how these claims can be justified in the light of the foregoing experiments." I have not made any tests with petrol and naphthalene, for the calculations on page 207, preceding the paragraph just quoted, are not sufficiently encouraging to warrant experiments with a petrol solution.

In reply to Mr. Evans, I think I can say that there is some decarbonising effect. I cannot speak from any lengthy experience, but such an effect would be a natural consequence if Prof. Morgan's suggestion is correct. There is most certainly no increase in carbon deposit as might be expected from the completeness of the combustion.

In conclusion, I think the remarks made by the President concerning an enricher which he tested in America are quite in keeping with the fact that there must have been formed some solution which gave the unexpected results similar in many ways to those recorded in the present experiments.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

DECEMBER, 1920.

The Fifth Ordinary General Meeting of the Session was held at the Chamber of Commerce, New Street, Birmingham, on Thursday, 30th December, 1920, at 7 p.m., Sir Henry Fowler (President) in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"Motor Bicycle Frame Design," by G. DOUGLAS LEECHMAN.

There were present 56 Members and visitors.

MOTOR BICYCLE FRAME DESIGN.

By DOUGLAS LEECHMAN
Member of Council.

Functions.

In view of the facts (1) that the most common design of motor bicycle frame is a modification of the pedal bicycle frame, and (2) that the functions of the two frames are in most respects the same, it is worth while to trace briefly the development of frame design in the simpler machine.

As a preliminary, it should be stated that the functions referred to are,—

(a) to connect the two wheels;
(b) to allow for relative deflection of the wheels in steering;
(c) to carry the rider, engine, etc. safely and steadily;
(d) to connect the crank shaft and the driving wheel in such a way that the driving force is transmitted without loss arising from deflection of the connections.

Historical.

Although H. J. Lawson and others introduced rear-driving safety bicycles, Fig. 1, Plate IV, before J. K. Starley marketed his first example of this type, there can be no doubt that "the Rover set the fashion to the world." As first designed, Fig. 2, Plate IV, it was an obvious modification of the "Ordinary," or high bicycle, and, like most such modifications, was a poor construction from the design point of view.

Within two years the design was considerably improved, see Fig. 3, Plate V, the diameter of the front wheel was reduced, the steering made direct, and the upper part of the frame made continuous, but the lines still followed fanciful curves, and there was a painful reduction in depth of the frame section behind the steering head. The Swift, Fig. 4, was better in this latter respect.

No one at that time realised the importance of strengthening the frame with a diagonal tube.

On the other hand, a number of rival machines, such as the Ivel, Fig. 5, were promptly introduced with simple cross frames which had diagonals, but no outline apart from inadequate ties, and nearly all these machines also lacked struts between the bottom bracket and the rear axle.

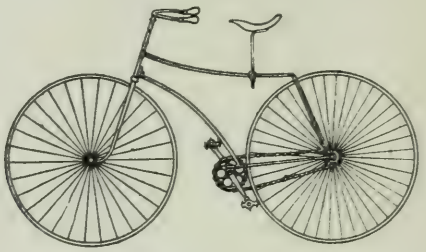


Fig. 4.

After two or three years, straight tubes replaced the curved ones (except in the case of the front fork, where the straight blades gave way to curved ones), and a straight diagonal was introduced between the saddle pillar lug and the bottom bracket. The long wheel-base Humber, Fig. 6, embodied these features, and, apart from a few details, has proved to be the permanent type.

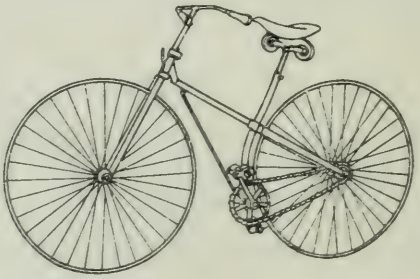


Fig. 5.

It was this so-called diamond frame to which the early motors were attached. The arrangement adopted on the Minerva, Fig. 7, at one time the leading machine, was to mount the engine in an inclined position in front of the bottom bracket, the crank case being provided with a split clamp which gripped the lower end of the bottom tube.

Most makers of that day adopted the Minerva position, but some preferred to place the engine above the bottom tube, and others set it behind the seat tube. There were points for and against all these positions, but they at least left the main frame a continuous contour.

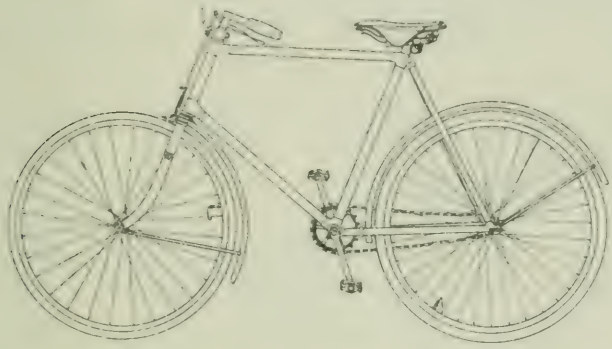


Fig. 6.

The Werner Modification.

The French firm, Werner Bros., also enjoyed a leading position in the industry, and had considerable success with their front-driven machine, but as engines grew larger, the position in front of the head became untenable, and they sought for something

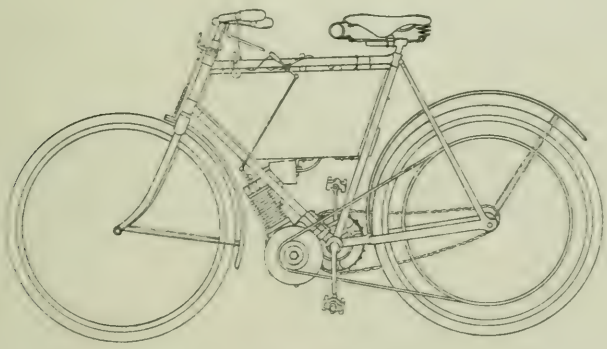


Fig. 7.

different and new. Unfortunately, their solution of the problem of mounting the engine vertically in front of the bottom bracket was accompanied by cutting adrift the lower end of the bottom tube, Fig. 8, and still more unfortunately this new design was speedily adopted by the trade generally.

Presumably with the object of mitigating the injury done to the lower part of the frame, Werners introduced a horizontal tube below the tank, fixing the front end at an indefinite point on the bottom tube, and the rear end at an equally indefinite point on the seat tube. This construction may well be described, like the Boudard gear, as "a mechanical offence," and it has exacted a heavy toll of the motor cycle trade.

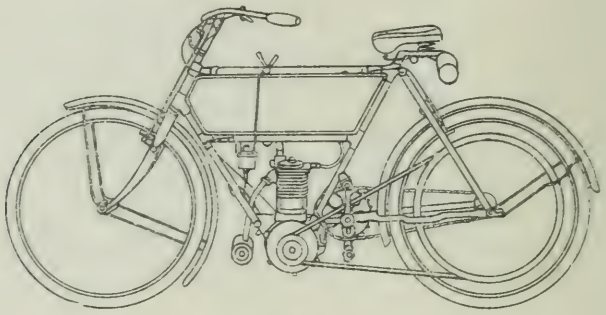


Fig. 8.

As in other branches of the motoring world, there was an insistent demand for more powerful motor cycles. This meant larger engines, larger sectioned belts and wider belt lines. This should have been met by wider rear frames and wider bearings, but instead of this, the old widths of bottom bracket, rear hub and frame were retained, the pedal cranks were violently distorted to clear the engine, and the back fork and stays were embellished

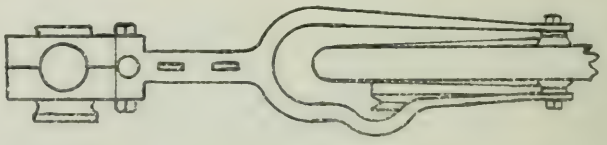


Fig. 9.

with local detours to clear the belt rim, Fig. 9. Such expedients could only be excused on the ground that owing to lack of time in which to produce parts specially designed for the work, existing components had to be temporarily resorted to. That these devious methods have been so largely perpetuated for something like twenty years is enough to justify, in the author's opinion, the strictures which he and others have passed upon the design of

modern motor bicycle frames, and that is by no means their worst feature.

The Rex Modification.

The Minerva and similar motor equipments came into use at the time when the fashion of horizontal top tubes was in full force for pedal bicycles. Buyers insisted on the same feature in motor cycles. They also insisted on a low position for the saddle. The Rex designer of that day solved the problem ingeniously and simply. He, in effect, sloped the seat tube so that its upper end was connected to the seat stays some inches below their junction with the top tube, somewhat as in Fig. 10. In other words, the main part of the top member was horizontal, but its back end was

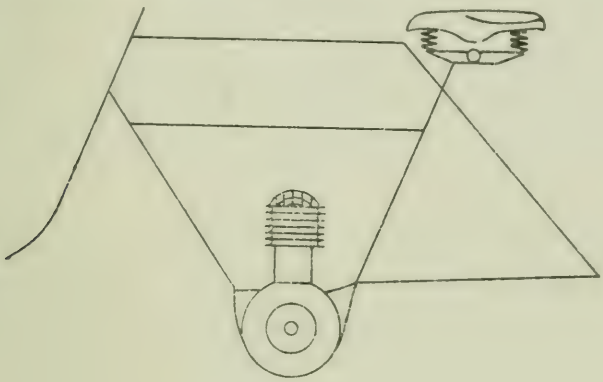


Fig. 10.

bent sharply downward, and the saddle was lowered to correspond. The popular cry was met, but at a sad sacrifice of mechanical principles. Combined with the Werner tank tube, the net result was a hexagonal construction for the top part of the frame. The continuous curved back tube soon followed, and entered on a period of popularity which it never deserved. We must all be glad to note that its course is nearly run, and the rider of to-day displays his mechanical discernment by demanding what his predecessor rejected, namely, a straight, sloping tube directly connecting the top of the head with the saddle pillar lug in its lowered position.

To avoid confusion it will be well, before going further, to name the different members of the frame, see Fig. 11, and in doing this the nomenclature adopted by the Cycle Engineers' Institute*

* See Proc. C. E. I., 1902, p. 29, &c.

for pedal bicycles has been followed so far as applicable. "Middle tube" is only put forward tentatively. It is hoped that some better terminology may be arrived at in the discussion.

The Problem.

The problem before the motor cycle engineer is to design a frame which shall be sufficiently strong and rigid, and at the

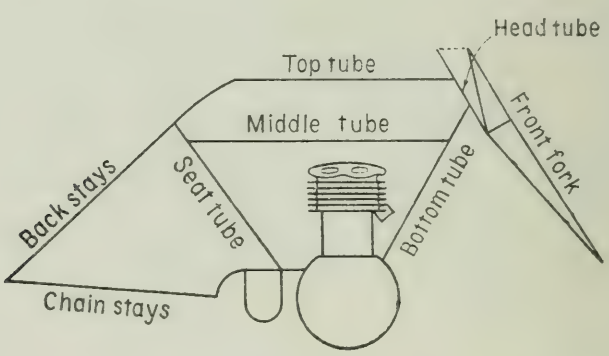


Fig. 11.

same time economical to produce as regards material and wages. Considerable reduction in weight is both desirable and possible; the rider's demand for lightness is becoming increasingly insistent, but the experience of the war as revealed by Mr. G. H. Savage in his recent paper before the Institution on the "Springing of Motor Cycles,"* and by others at the front, shows that weight reduction

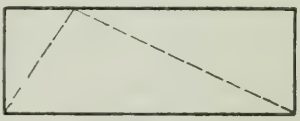


Fig. 12.

can only be secured by a closer adherence to established mechanical principles in the lay out.

Primarily, the designing of a motor bicycle frame is a bridge proposition. The simplest form of frame would consist of a single beam supported upon the axles of the two wheels, Fig. 12.

In addition to the ordinary function of a bridge of carrying

* See *ante*.

a load over a gap, the motor bicycle frame (apart from anti-vibratory and transmission adjustment considerations) must be capable of resisting longitudinal tensile and compressive stresses set up by forces tending to vary the distance between the wheel centres, and (apart from steering considerations) the frame must also be capable of resisting forces tending to deflect the wheels from the longitudinal vertical plane about vertical or horizontal axes or a combination of both.

As the principal part of the load, namely, the rider, is to be carried at a certain height, the beam may be made of such a depth that the top of it will reach up to the saddle, less a few inches for adjustment and spring motion. Behind this point the beam may be tapered off to the rear axle, to economise material and reduce weight. Were it not for steering considerations, the beam would be similarly tapered off right from the saddle towards the front axle, leaving an ideal form deepest where the concentrated load is applied and reducing regularly in depth as the bending moment decreases. (See dotted lines in Fig. 12.)

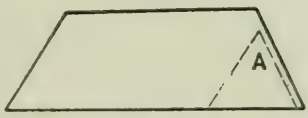


FIG. 13.

Here we are met with one of our most serious problems. The best kind of steering demands that a large gap shall be cut upwards in the front part of the beam to allow for the deflection of the steering wheel when the bicycle is describing curves, Fig. 13. Incidentally, it also allows of a certain amount of tapering at the extreme front end of the beam.

In this connection we have to remember that while the strength of a beam varies directly as its breadth and inversely as its span, it varies as the *square of its depth*. Thus, a cut only half way up from the under (tensional) side reduces the strength of a beam to one-quarter of what it is in its complete form. The demand for such a deep gap as is required by the steering is calculated to make the ordinary bridge engineer gasp. The importance of leaving the uncut section at A as deep as possible and of even inclining the upper surface of the beam towards the front in order to increase the effective depth at this point is surely suffi-

ciently glaring, and yet "awful examples" are not lacking in which the designer has actually gone out of his way to carry the gapping to extremities by reducing the section at A to practically a point. These designers are fortunately few, but there are many who fail to give practical recognition to the fact that the stresses on the head bearing are in inverse proportion to the distance between the two rings of balls.

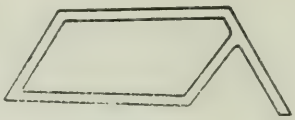


Fig. 14.

Having so far realised the broad essentials of the frame, we may hope to profitably replace the simple, solid beam by a far lighter structure of skeleton form following the outline of the beam, Fig. 14. The portion carrying the front wheel may be regarded as a cantilever anchored in the main portion. This main portion is a five-sided polygon, and some consideration of quadrilateral and other figures being called for, we will leave the

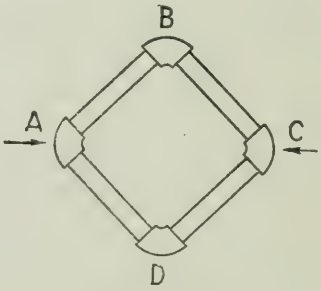


Fig. 15.

further development of the frame on these lines for the time being.

Plane Figures.

In any skeleton structure of more than three sides, and not triangulated by diagonal bracing, the permanence of the form depends primarily upon the resistance which the joints are able to offer to distortion, provided that the load is applied at one of the angles. For example, take a tube and lug rectangle such as ABCD, Fig. 15. If forces be applied at A and C as indicated

by the arrows, the points of intersection of the axes of the tubes at B and D may be regarded as fulcra, and the full leverage will be expended on the lugs, tending to close B and D and to open A and C. If the lugs be strong enough to resist the force, the portions of the tubes just clear of the lugs will be called upon to resist the bending moment, and if these be enabled to resist the force by means of suitable butting or liners, the force will be expended on the plain middle portions of the tubes. While the leverage is greatest at the lugs, it is least at the middle sections of the tubes, and in an ideal construction of this kind, the strength would be perfectly graded all along from the ends to the centres, in proportion to the leverage. Such a structure would resist any stress (up to the elastic limit) without fracture or even permanent set, and would spring back to the original form when the load was removed.

The danger of nicking the tube at the end of the lug in filing up a brazed joint, and the value of tapered lugs, butted tubes and graduated liners would, it might be thought, have been fully realised by now, but numerous examples offered to the public prove the contrary. Probably it is cheaper to raise the elastic limit of the whole—thereby increasing the strength of the weakest element in a series of parts having pronounced differences and sudden changes in strength—than to adopt the more scientific and far lighter system of construction just recommended. Even in the latter case the necessity of leaving a fillet of brass at the end of the lug should be insisted upon. In a valuable paper entitled "Steel Tubes, Tube Manipulation, and Tubular Structures for Aircraft,"* by Messrs. W. W. and A. G. Hackett, the authors record various experiments carried out on joints made with parallel lugs and tapered lugs respectively. The results were always greatly superior where the thickness of the lug was graduated. Owing to the small amount of metal involved in welded joints, the author is of opinion that they are unsuited to cases where the members joined are liable to angular distortion.

Since, if an incidental force be applied at one angle of a quadrilateral or a polygon in the plane of the structure, the figure will tend to collapse in the direction of the force, the author thinks it possible that most people would expect that if an excessive load were placed on an ordinary diamond framed bicycle when

* Proceedings Royal Aeronautical Society, May, 1918.

stationary, the frame and fork would tend to spread out lengthwise. But this expectation would not be quite fulfilled, in fact, at least at first, there would more likely be a longitudinal contraction, see Fig. 16. The reason is this: the load is carried on the apex of a triangle supported on the back axle. As the load increases, the triangle tends to rotate about this axis, the saddle turning forward and the bottom bracket downward; consequently, the top of the head is pushed forward and the bottom thereof is pulled back. This decreases the inclination of the head and brings the front fork into a more vertical position, and, as the front wheel is sustained by the ground, the straightening up of the fork means raising the head, lifting the bottom bracket and moving the saddle back and up. In other words, the structure is in balance. If a pin-jointed model of a safety bicycle be made to plan, and a load be applied to the saddle, it will be found that

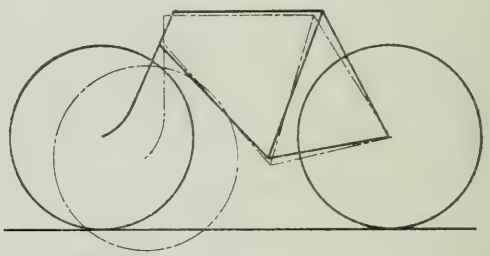


Fig. 16.

the front wheel will come back slightly and the parts will remain in this position while the load is increased up to a very substantial figure. When the actual machine is running on the road, the inequalities of the surface disturb this balance and the frame springs and so absorbs the vibrations to a greater or less degree. That this is true is confirmed in practice by the fact that a pedicycle with a triangulated frame is distinctly harsher in use than one with an ordinary open diamond frame.

The whole action is dependent upon the strength and rigidity of the front fork, particularly around the fork crown, where many failures occurred in the early days. The design has proved sufficiently satisfactory in the lightly stressed pedal machine, but it is open to grave doubt whether it is advisable to risk it in a motor bicycle, not only because the stresses are much more severe, but because a spring fork is not well adapted to produce the action above described. In other words, in an open polygonal motor bicycle

frame, the stresses on the joints differ little if at all from those generally characterising such figures, and the strength necessary to safety can only be secured with a weight greatly in excess of that easily attainable with a correctly trussed frame.

A "frame," in engineering parlance, means the kind of skeleton structure which we are now considering, and certain definite principles are essential to its success.

First, the frame members must be arranged not as an open quadrilateral or polygon, but as an assembly of triangles, so that the whole constitutes a single trussed beam.

Second, the load or loads must be applied at the angles, that is, at the joints, and not on and transversely to any of the sides.

Third, two triangles must not be connected only by one angle of each.

Fourth, the plane of the beam must be arranged in the plane of the load.

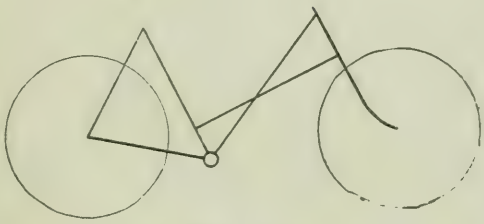


Fig. 17.

Let us examine the standard type of frame in the light of these principles. First, we observe that it is not an assembly of triangles only, but, apart from the front fork, comprises a triangle, a quadrilateral, and a polygon—all of doubtful lineage. Breaking the first law leads to infraction of the second, and we find some loads applied otherwise than at the angles. It is guiltless, fortunately, of infraction of the third law, being in this respect superior to certain pedal bicycle frames, Fig. 17, now doubly entitled to the characterisation of "dropped." The fourth law is very fairly complied with, so far as solo machines are concerned.

With reference to the principle that the forces must be applied at the angles and not on any of the sides, if it be desired to support a load on one of the sides, the point of application must be converted into an angle of a triangle by taking a tie or a strut, as the case may require, from the point concerned to the opposite angle of the triangle.

A force acting in a certain direction and applied at a point may be resolved into two other forces acting in the same plane and along any other two lines diverging from the point except at right angles to the said direction. Hence, a force applied to one angle of a triangular structure in the plane of the triangle is directly opposed by the resultant forces of the two sides of the triangle adjacent to that angle. These two sides of the triangle are therefore put purely in either tension or compression, and by following up the forces transmitted by these two sides to the other two angles of the triangle, it will be found that the third side also is in tension or compression only, none of the sides being subjected to any transverse stresses.

The matter is expressed geometrically in Euclid I. 7. "Upon the same base and upon the same side of it, there cannot be two triangles having their sides, which are terminated in one extremity

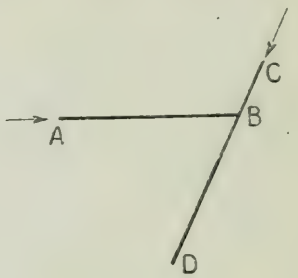


FIG. 18.

of the base, equal to one another, and likewise those which are terminated in the other extremity." Since it is impossible to make two different triangles with the same three sides, it follows that a triangular structure cannot be deformed except by stretching or compressing one or more of the sides. The stresses are coincident with the axes of the tubes, the figure is inherently rigid and of maximum strength for its weight.

The essential wrongness of arranging one load-carrying tube to bear on or be joined to another at an intermediate point on the latter may be shown as follows. Let the first tube, AB (Fig. 18), be joined to the second, CD, at B. Two forces, AB and CB, are acting on the point B. Whether the values of those forces be proportioned to AB and CB or whether they have any other practical value, it is evident that their resultant cannot be in the direction BD; that would be to assume that the value of the

force AB was zero. Hence, the arrangement AB, CD involves a transverse stress on CD, which will shear after the force has been applied a greater or less number of times.

The Rear Triangle.

Referring now to the rear triangle, a heavy casting is often found at the junction of the seat tube and back stays—*and a heavy casting spells bad design.* The casting is mainly horizontal, and its forward end extends to the engine, see Fig. 19. The gear-box is suspended from this casting by bolts passing up through holes in the casting, the holes being slotted to allow of adjustment of the front chain. The countershaft thus lies some inches below its point of support, and the stresses on the bolts are much more severe than they either should be or need be. Seeing that the

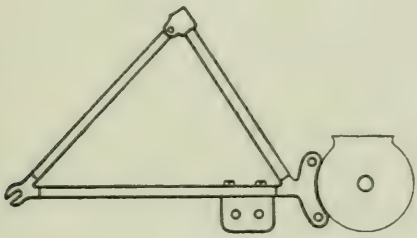


Fig. 19.

tension set up in the chains by the transmission of the driving force tends to draw the gear-box towards the crankshaft in the one direction and towards the axle of the driving wheel in the other direction, it follows that the struts between the engine and gear-box should lie in the plane of the crankshaft and countershaft, and that the struts between the gear-box and back axle should lie in the plane of that axle and the countershaft.

Correct design of these parts would not only relieve the gear-box bolts of undue transverse stresses, and the chain stays of a wasteful tendency to bow upwards, but it would also offer direct resistance to any tendency the gear-box may evince to twisting out of parallel with the driving wheel axle while, say, hill-climbing. Some machines exhibit a sort of mute acknowledgment of these principles, the rear end of the casting being curved down so that the chain stay tubes actually lie in the plane of the driving wheel and gear-box axes, Fig. 20, but as the forward ends of the

tubes do not reach the gear-box by some inches, little benefit is derived from the arrangement. A substantial improvement would be secured by providing the back of the gear-box with lugs which would extend to and overlap the forward ends of the chain stays themselves and be bolted thereto in a way which would allow for the necessary adjustment. The rearward staying of the box would

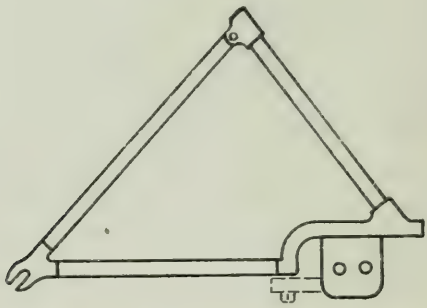


FIG. 20.

then be approximately direct, and some economy in weight would be possible.

A better method still would be to scrap the present plan instead of trying to patch it up, and to design this part of the frame afresh. The author would suggest that the chain stays should be duplicated, the upper pair running from the rear fork-ends to

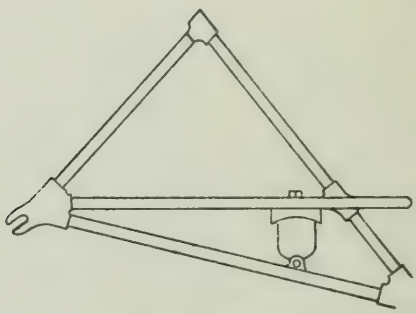


FIG. 21.

the top of the crank case, and the lower pair from the fork-ends to the bottom of the crank case or to a part of the frame adjacent thereto, see Fig. 21. The stays would thus act as torque tubes to the engine. The gear-box would lie between the pairs of stays, which would be provided with parallel upper and lower beds on which the box would be mounted adjustably. Or the box could

be hinged to one pair on a transverse axis and locked, after adjustment, to the other pair. In either case, the gear-box would form the third side of a triangle to both pairs of stays, and a very stiff, light and simple rendering of this important detail would be obtained.

The equivalent lesson in pedal bicycle construction, namely, the direct staying of the bottom bracket, was learnt thirty years ago, and yet it seems to have been lost on the motor cycle designers of to-day.

As already indicated, bending of the chain stays and seat stays should be avoided as far as possible. They should be straight in rear elevation and in plan as well as in side elevation. This may involve a longer back axle, but that is almost wholly an improvement, as it means a wider hub and stronger wheel; and it also gives the opportunity, which should not be missed, of bringing the sprocket or the belt rim, as the case may be, between the ball races of the wheel bearing, see Fig. 22. Where the sprocket or

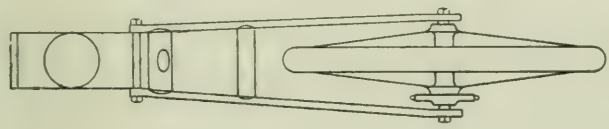


Fig. 22.

belt rim is overhung, the drive sets up a continuous (though varying) cross action in the bearings which is very undesirable. A straight stay, properly arranged, is lighter, more rigid and cheaper to make than a bent or cranked one.

The Polygon.

Unfortunately, the great majority of motor bicycles to-day embody an aggravated instance of the application of a transverse stress to a tube in the arrangement of the middle tube. The result is that when the front wheel encounters an obstacle, the head tube endeavours to rotate backward about a transverse axis drawn through, say, the top head bearing. As the bottom of the head tries to go back, the upper end of the bottom tube tries to swing back too, Fig. 23, but the tendency is resisted more or less completely by the middle tube, which in effect delivers a sharp kick to the

bottom tube every time it attempts to stir. This kind of action can only be repeated a limited number of times before fracture ensues. To guard against it we find a ponderous casting comprising the whole of the head and often extending along the down tube to below the joint with the middle tube. Notwithstanding the mass of metal, the frequent cracks prove that the angles at the head lugs still open and close in a dangerous manner, and the bottom tubes are still sheared by the middle tubes. If the casting is massive enough to preclude the occurrence of either of these failures, the defect exhibits itself elsewhere, as, for example, by breaking the seat tube where the rear end of the middle tube is joined to it, or the top tube (particularly if curved) behind the head lug. This is in accordance with the experience that if a structure be incorrectly designed, it will fail first at some particular point; local strengthening will cure the local trouble, but only to start another

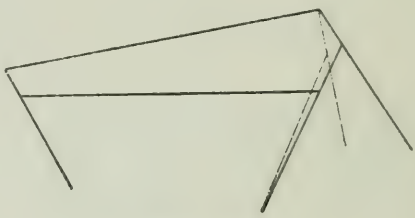


Fig. 23.

one, and so on till a vicious circle is travelled and repeated. Nothing short of correcting the design will ensure a reliable and permanent cure.

Provided that the top tube is neither curved nor kinked, the polygon can be converted into a triangle by the simple expedient of raising the middle tube until it reaches from the saddle pillar lug at one end to the bottom of the head at the other. This arrangement relieves the head of all angular stresses, the angles of a triangle being constant. It also necessarily relieves the bottom tube and seat tube of all transverse stresses due to the present position of the middle tube.

The Quadrilateral.

To describe the lower panel of the main frame as a quadrilateral is an euphemism, but the term will serve in place of something

which could only be more veracious by being less polite. Three of the sides are constituted by the middle tube, and portions of the seat tube and bottom tube, Fig. 24: the fourth side is constituted by the engine in a manner which the author believes, and hopes, is unique in constructional engineering.

It is admitted that the crank case, owing to its hollow cylindrical formation, is in itself a very strong article, and is unlikely to give trouble as a member of the frame. To that extent it may be well adapted to act as, say, a king-post, the function which it performs in the Scott frame. But that is not all; the engine is no inert mass, but a powerful generator, struggling at every explosion to rotate backwards around its crankshaft, and wrenching some thousands of times a minute at the fixings which attempt to retain it in place. A body of such an active nature is essentially unsuited to form one of the elements of a frame the functions of

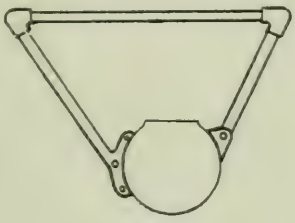


FIG. 24.

which are characteristically those of a passive resister.

The opening words of Mr. H. Davies' work on "Foundations and Machinery Fixing" are:—"The functions of a foundation are two in number. Firstly, a bed must be provided which will permanently maintain the machinery firm and level with all its parts in true alignment. Secondly, it must be so constructed and proportioned that it will absorb shock and vibration so far as it is possible to do so." The common practice in motor cycle engineering is a total disregard of these axioms. No bed justifying the name is provided for the motor cycle engine, and the means of attachment, such as they are, so far from maintaining the position of the prime-mover, allow it to oscillate at every impulse. The frame should be a continuous unit, affording a bed quite independent of the engine, and supporting the engine in such a way that the cylinder can neither nod nor twist about

its axis. Shock and vibration should not expend themselves in contorting the frame tubes, though washers or jointing of rubber cloth might be interposed between the holding down lugs of the engine and the bed to absorb metallic jar. The conditions indicate an arrangement of the bolts in a vertical plane rather than in transverse ones, as now general.

A modification, for the worse, of the quadrilateral is illustrated in Fig. 26. The engine is supported by an arm extending some 8 in. forward of the foot of the diagonal, which may be taken as a fixed point. Half the weight of the engine, say 30 lb., is carried by this arm, the bending moment on the fixed end of which is therefore 30×8 inch-lb. $= 240$ inch-lb. That is quite apart from the stresses due to the mass of the rider, etc., and is when all is still; what it is when the engine is running and the machine is travelling over a rough road would be difficult to calculate. Whatever the actual transverse force acting on the root of the arm attains to in practice, it can be eliminated by the simple expedient of inclining the diagonal tube so that its foot reaches to the engine connection. The arm then becomes part of one of the sides in the rear triangle, and all transverse stresses due to the carrying of either the engine or the rider are replaced by tensile and compressive forces sustained longitudinally by the chain stays and diagonal. In practice the arm probably consists of a heavy casting; a substantial saving in weight and cost is attained by the more correct construction.

With or without the arm, this panel of the frame remains an open quadrilateral, and it will not be right until it is triangulated. The frame being both longer and lower than when it was mutilated by Messrs. Werner Frères, the author suggests that it may now be possible to restore the continuity of the lower parts and so secure the triangular form as shown in Fig. 25 without raising the engine unduly high. Perhaps it is only practicable with the small crank case associated with an outside flywheel; if so, it will be helping in another good cause.

Roll.

The engineer on first dealing with the subject of bicycle frame design is apt to disregard the possibility of roll. He imagines, perhaps not unnaturally, that as the bicycle is a balanced vehicle,

there are no material forces acting on the frame other than those of which the directions are in the plane of the frame. As practical riders, we know that even when riding along a straight road, a frame which is weak torsionally will exhibit an unsteadiness which may be not only uncomfortable but actually dangerous. This weakness may arise to some extent from the front fork being indirect in

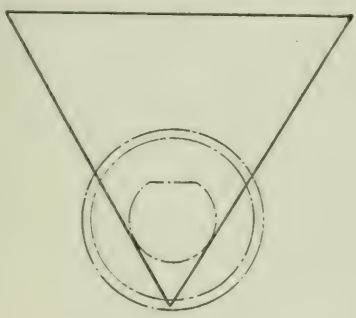


FIG. 25.

form, or constructed of too light members, or too flimsily connected to the front spindle.

The main rolling action appears to take place, however, about an axis coincident with a line extending from the bottom of the head to the point of contact between the rear wheel and the ground. It may be due to too short a head, to a reduction of the depth of

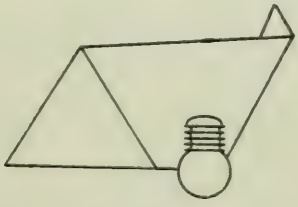


FIG. 26.

the frame section behind the head (Fig. 26), or to the use of tubes which are too small in diameter or in gauge or which are bent instead of straight. Apart from these points, there is another which is, perhaps, of more importance. A tube offers great resistance to a moment tending to twist it on its own axis. It so happens that the bottom tube of some motor bicycle frames lies approximately in the line extending from the bottom of the head to the rear point of contact, and it may be said that, other things being equal, a

frame in which the axis of that tube is coincident with that line will roll least, while other frames will roll more according to the amplitude of the angle between the axis and the line. Also, a frame having either a curved or a duplex lower tube may be expected to roll more than one in which this member is straight and single.

To ascertain the torsional strength of a tube, the fourth power of the external diameter minus the fourth power of the internal diameter is multiplied by the shearing resistance of the material and by a constant (which may be taken as 0·196), the product being divided by the diameter of the tube and the radius at which the load is applied. Thus where—

W = Load in tons applied at end of radius.
R = Radius in in.
D = External diameter of tube.
d = Internal diameter of tube.
S = Ultimate shearing resistance of material in tons per sq. in. of section, say 25.

Then $W = \frac{0{\cdot}196(D^4 - d^4)S}{R\,D}$

Hence, a single tube of a given diameter will offer more than twice as much resistance to torsional stresses as two tubes each of half the diameter but otherwise similar to the single tube, even supposing that it were possible to arrange the two smaller tubes on a common axis. To put it another way, a single tube of $1\frac{1}{4}$ in. dia. has a greater torsional resistance than two similar tubes of $\frac{3}{4}$ in. dia. and is only about two-thirds their total weight.

The fourth power of the difference between the internal and external diameters is a characteristic of formulæ relating to steel tubes. For example, with reference to the transverse and compressive strengths respectively, the moment of resistance, commonly denoted by $Z = 0{\cdot}0982\,((D^4 - d^4)/D)$ and the moment of inertia, commonly denoted by $I = 0{\cdot}0491\,(D^4 - d^4)$.

These qualities are very high in tubes owing to the hollow circular form in which the metal is distributed, and there is some temptation to increase the diameter and reduce the wall thickness in order to secure the maximum results. It should be remembered, however, that tubes with very thin walls will fail in practice long before they are stressed to their theoretical limits owing to local buckling.

Dr. Lilly, of Dublin University, the great authority on columns, is of opinion that a length of about 20 to 24 times the diameter gives a good working proportion for the members of a tubular structure.

In view of claims put forward on behalf of pressed steel for motor bicycle frame construction, it may be of value to note that a steel tube of a given size is as efficient when functioning as a long strut as a mild steel channel section of rather more than five times the weight.

The Front Fork.

There is an unfortunate tendency for general expressions to become specialised in connection with machinery, and an instance is found in the subject under treatment. In motor cycles the frame has now come to be understood as the portion extending

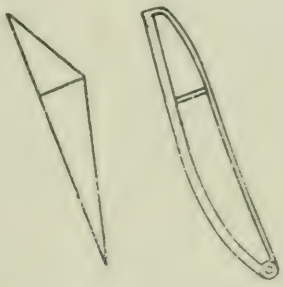


Fig. 27. Fig. 28.

from the rear fork ends to the head socket, while the fact is that the front fork is just as essentially a portion of the frame as any other member of the structure connecting the axles of the two wheels.

In side elevation, see Fig. 27, many front forks are far better designed than most rear frames. They are good examples of triangulation, straight sides, clean angles, with maximum depth and a substantial king-post where the load is heaviest. On the other hand, there are many so-called girder forks, such as those shown in Fig. 28, which suggest that their designer may have been an artist but was certainly not an engineer. The tubes are curved instead of straight, they are spread at considerable distances apart at the top and bottom lugs, and the third "angle" is in fact a bend and does not coincide with the end of the king-post. Such forks distort readily on encountering a force that a

properly designed fork will resist without the slightest deformation; the curved parts of the former will bend or unbend with comparative ease, while the members of the latter will be subject to stresses of compression and tension only and will resist them accordingly.

In front elevation, Fig. 29, nearly all front forks are, if the front spindle be included, of a long rectangular form. Their lateral rigidity depends, therefore, mainly upon the stiffness of the joints at the extremities and at the lower bridge. This quality of lateral strength is of considerable importance when a sidecar is fitted to the machine, and it is remarkable that it has been principally aimed at by widening the fork as a whole. Oval blades are still used in many front forks, and the major axes are set fore and aft. This is quite unnecessary, and round tubes

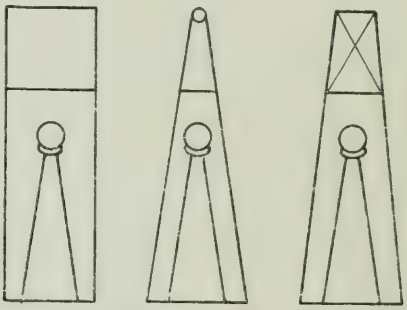


Fig. 29. Fig. 30. Fig. 31.

fulfil all requirements fore and aft if the fork is properly trussed; increased lateral stiffness might be obtained by using oval tubes and setting the major axes transversely.

Better results should be secured, however, at less weight by triangulating the fork in front elevation as shown in Fig. 30. This may involve some increase in width of the front hub and length of the head, but these are both modifications in the right direction. If it is not considered practicable to triangulate the fork in this simple manner, the next best thing is to get as much lateral inclination on the members as possible, and to complete the triangulation by cross bracing the head as shown in Fig. 31.

We may now return to the structure of skeleton form, the primary members of which, it will be remembered, follow the outline of the beam (Fig. 14). The figure, being a polygon, will require bracing, which may be accomplished in various ways,

but as there are five sides and angles, at least two ties or struts will be necessary. One, BD, Fig. 32, may be well employed in connecting the angles corresponding to the two main items of the load: the other must cross the remaining rectangle and may be arranged either as BE or DA: but the author would emphasise the fact that the frame can not be properly trussed without at least three members running to the head.

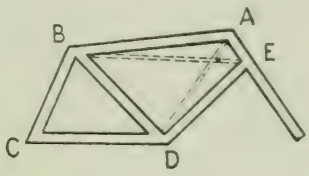


FIG. 32.

SUMMARY.

By way of practical summary, a design of motor bicycle frame is submitted in Fig. 33 which, without any claim to perfection, may be considered as avoiding most of the defects above referred to and as according with the elementary principles of a tubular structure.

Every machine is a compromise, and this being so, it is obvi-

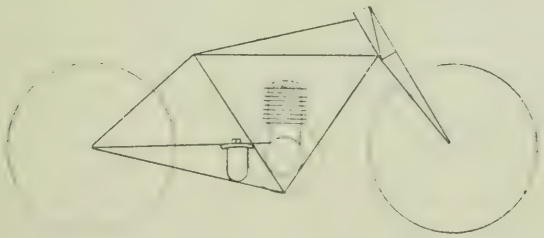


FIG. 33.

ously necessary to determine the relative importance of the parts which have to be adapted to one another. Individual opinion will greatly affect the result, but the author feels confident in affirming that such a vital item as the frame should not suffer in favour of mere accessories and fittings like mudguards, tanks, saddle-peak connections and so forth.

In conclusion, the author wishes to make it clear that he does not assume any exclusive or special right to dogmatise on the sub-

ject he has chosen. Rather, he is fully aware that motor cycle designers and manufacturers generally recognise that motor bicycle frames have developed on lines which are not satisfactory, and that the time has arrived when the subject should be fully reconsidered with the object of producing improved designs in the future. Hence it has been thought desirable to trace the history of the commonest type of frame and to restate a few of the well known mechanical principles bearing on the subject.

It is felt that the best way to secure practical results from a discussion of this kind is to concentrate on a comparatively small subject and thrash it right out, and for this reason the author has abstained from any lengthy reference to other than the usual type of frame. His thanks are due to Messrs. Accles & Pollock, Ltd., and to Messrs. Stewarts & Lloyds, Ltd., for valuable information most generously rendered in the preparation of the paper.

THE DISCUSSION.

Mr. J. L. Norton, in opening the discussion, said: The paper is so correct that it leaves very little room for discussion or criticism except in connection with the nomenclature of the members of the frame. What the author calls the middle tube I call the cross rail, and what he calls the bottom tube I call the front down tube, and I think that in the trade those are the usual terms. Otherwise I can do nothing but agree with what the author has to say. I think we all know that our frames are mechanical, or rather unmechanical, monstrosities. Nevertheless, we have manufacturing considerations as well as mechanical considerations and theories to contend with, and it is for that reason that we make very many departures from what, I suppose, is correct in theory. The author says that heavy castings spell bad design, but we cannot very well do without castings or stampings unless we make the manufacturing costs absolutely prohibitive. Unfortunately, however, we can scarcely get really reliable light malleable castings to-day, and in some deliveries of castings we have had to scrap 25 per cent, and in one particular case no less than 80 per cent, and hence we are forced to adopt more or less heavy castings. The very interesting frame design in Fig. 33, put forward by the author, does not take into consideration the very serious difficulty of chain clearances, though I do not say that these difficulties cannot be surmounted. For instance, by running a tube straight from the axle of the engine, and suspending the gear-box from that, we should be able to clear the chains all right, but we should make the gear-box ground clearance very small. If we were to put a tube above the gear-box and bring it to the top of the crank case, and another below the gear-box to the bottom of the crank case, as the author suggests, we should have great difficulty in clearing the chains unless we were to make an excessively wide chain line, or, taking the opposite view, use very small tyres and very small mudguards, or trap them excessively, which is very undesirable. Although the author speaks very strongly, and quite rightly, in favour of straight tubes, we

(Mr. J. L. Norton.)
are compelled to put right-angle bends or lugs in these tubes as they approach the engine, viewed in plan. I am not in agreement with any of three types of forks shown on page 240. There are manufacturing advantages in the one shown in Fig. 29, because we can make both sides alike and bolt them together. Those shown in Figs. 30 and 31 are not so good as two triangles with their bases meeting at the crown. This gives a fork which is narrow at the top and the hub, and widens out at the crown, which makes the stiffest fork laterally that it is possible to make, and it also has the advantage of enabling a broad mudguard to be fitted without trapping. I do not think we could use the type of frame shown in Fig. 25 without very materially raising the centre of gravity or lessening the ground clearance. Even if we used an outside flywheel engine, which the author advocates, we should still have to raise the centre of gravity. In fact, it necessarily follows that if we use an outside flywheel, the centre of gravity goes up at once. There is one other point. The author tells us that we should raise what we term the cross rail to the head and the seat tube. That necessitates either a very small tank and unnecessary clearance between the engine and the cross rail, or a saddle tank, which is not so easy to make as an ordinary tank and which is a little bit heavier, a little more liable to air locks, and is rather more expensive.

Major B. W. Shilson: The paper is, I think, one of the best that has been written on the subject of the motor cycle pure and simple, and I do not think it leaves very much room for discussion on the question of the design of the frame itself. Speaking from the motor engineer's point of view, as apart from the motor cycle engineer's point of view, we have always felt that the motor cycle has been gradually built up in practice rather than by theory. It is a difficult problem to get out a machine with such a small engine as is found on the average motor cycle which will do the work that the motor cycle of to-day does. The problem is one which I think is hardly met with outside of automobilism, which involves relatively high powers having relatively high loads at high speeds over an unprepared track.

The author has referred to the subject of tapered lugs, and the nicking of the tubes, etc., and I hardly think that he has emphasised that point sufficiently. If the tubes are in any way injured when filing off the brazing, their strength is decreased

enormously. If a hole is drilled in one side of the tube, no matter how small that hole may be, the strength of the tube is decreased to one-tenth or one-twentieth of that which it originally possessed. One of the common practices in tube manufacture for breaking hard drawn tubes is to file a nick on the tube and hit it a heavy blow on the anvil, the tube then fractures right across. In a motor cycle of my own I had one side of the handle bar break off in my hand when starting up, and examination proved that it was due to this very cause.

In the case of graduated liners, I think these ought to be brazed or soldered, or, at all events, mechanically attached to the tube into which they are fitted, as this considerably increases the strength. The straight-sided socket or tapered socket mentioned by the author is a question of very great importance as tending to make the stress more uniform throughout the length. It is a feature which is not carried out in practice as completely as it should be, either in motor cycle or in motor car work. It is generally thought to be cheaper to have what I might call a square instead of a tapered lug, whereas actually the latter is cheaper, and is certainly very considerably stronger. The author gives us formulæ for the tubes, but I think the addition of the following formula, which is the usual one for the weight of the tube, would make this part of the paper more complete:—

If D = outside dia. in in.
d = inside dia. in in.
t = thickness in in.
$= \frac{1}{2}(D - d)$
w = wt. in lb. per cu. in.
$= 0{\cdot}284$ for steel
W = wt. in lb. per ft. run

Then

$$W = 12\,\pi\,w \times t\,(D - t)$$
$$= 12\,\pi\,w \times t\,(d + t)$$

Where

$12\,\pi\,w = 10{\cdot}7$ for steel.

The author refers to the fact that one tube is very much stronger than two tubes of half the diameter, but it is not always possible to put in what is the most desirable. The design of motors is always a matter of compromise, and something has to be sacri-

(Major B. W. Shilson.)
ficed. The question of cost should also be borne in mind, as, particularly at the present moment, it is not so much a question of design as of cost in the endeavour to retain the market. Tapered lugs and graduated liners ought to be mechanically connected to the tubes in every case, and all pegs should be tight fitting tapers. The author refers to thin walls of tubes buckling in practice, but there is also the question of corrosion or rusting, and I should like to ask the author what he would suggest for adequate protection against this action. I think in practice "Cosletising" is found to be one of the most satisfactory methods. I should also like to know if the author has any views on the question of rustless steel tubes for motor cycles, and whether in this case any special precaution or method should be adopted in brazing. I should also like to ask him about the material of which ordinary tubes are made, and whether he advocates the use of nickel chromium tubing.

Mr. F. A. S. Acres: The front forks shown in Figs. 30 and 31 look to me upside down. Many years ago the front forks of the James carrier cycle came straight away up from the front spindle to the hand grips, with a cross stay at the top; this fork was designed to meet the lateral strains applied at the front wheel spindle, and is exactly the converse of the design shown by the author. I can very heartily endorse what has been said about malleable castings. My own firm have recently had to abandon them entirely and adopt stampings, or, where the parts are unsuitable for stampings, to use mild steel castings. With regard to the buckling of the thin walls of steel tubes, the author states that Dr. Lilley is of opinion that a length of about 20 to 24 times the diameter gives a good working proportion for the members of a tubular structure, but the author does not tell us anything about the thickness of tube it is possible to use, or whether the relation of length to diameter becomes a function, not only of diameter, but of thickness as well. With regard to the question of the height at which the rider has to be carried referred to by the author, it is quite true that a low position means that the rider has less distance to fall, but I am perfectly convinced that it makes a machine a great deal more liable to side slip. A friend of mine who had ridden from the very earliest days, when the pneumatic tyred safety came into being had an ordinary bicycle, with a 56 in. wheel fitted with pneumatic tyres, built

for him which he kept for riding in greasy weather, and he kept his safety for fine weather. With the old ordinary side slip was unknown. My earliest experience of motor cycling was with a firm who were making engines in sets and buying frames to put them into. One of the frames we had was a very high B.S.A. motor cycle frame, and another was a very low Eadie frame. The latter used to side slip on the least provocation, but with that high B.S.A., and also with a Triumph and a Minerva—this was in about 1902 or 1903—I could ride over any roads and in any grease with perfect safety. I put this down to the fact that if the wheel was deflected a small amount it would incline the low built machine to a greater angle than the other. At all events, I do feel strongly that a low centre of gravity, from the safety point of view, is not anything like so essential that it is made out to be.

Mr. P. J. Joyce: I should like to ask the author how the engine will be attached to the frame shown in Fig. 33, because it appears to me that if the crank case is bolted to the main triangle A. B. D. it breaks up the triangle, as the stresses caused by the engine put a side load on the bottom tube and the seat tube. Therefore, the main triangle in Fig. 33 is no better than the one in Fig. 24.

Mr. Maurice Holroyd: The top panel of the design shown in Fig. 10 has six sides, which I do not think is good design. At the same time I consider a top panel of four or five sides to be far more practicable than a panel of only three sides. The latter is of a very strong section, but as one side cannot alter its length without one of the other two sides doing likewise or otherwise buckling, it is far more likely to break than a figure of four or five sides as in the former case. The flexibility of the frame with a top panel of five sides makes it a very comfortable frame to ride. Why should a frame be absolutely rigid? Why should it not be allowed to "breathe"? A casting fitted to the bottom end of the seat tube forming the rear engine cradle is, in my opinion, an advantage over cradle plates (pressings). Pressed cradle plates make a very neat job, but unfortunately the chain stays are apt to whip at high speeds, which is most noticeable on machines having a long back.

"Cosletising" of nuts and bolts is essential, and "Cosletising" the inside of petrol tanks is also a great advantage.

Mr. Maurice Holroyd.)

I should also like to suggest that a very excellent name for the tube running from the bottom of the steering head to the front of the crank case carrying the front cradle plates would be "the engine tube."

The manufacturers of the "Beardmore-Precision" motor cycle have had every satisfaction from welded joints, and have yet to have a broken frame. The whole machine only boasts of two malleable iron castings in the whole of its frame, and these will shortly be replaced by pressings.

Mr. T. R. Speck: There is a type of frame that nearly approaches that shown in Fig. 33 which has not been mentioned, that is the Loop Frame, as adopted by the Indian and, I believe, by the Minerva or the Werner in the early days. The Rex practice of about 1907 of fitting a malleable iron engine cradle was also a creditable attempt, which avoided making the engine function as a bottom connection for the seat tube and down tubes.

I should have very much liked to have heard the author's views

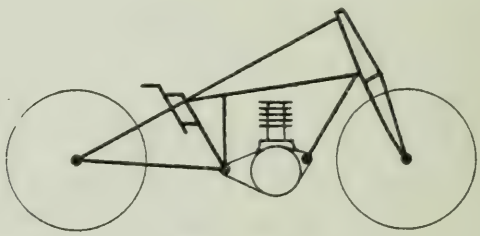


Fig. 34.

on the various types of spring frames which are now becoming very popular.

Mr. Chas. M. von Eugen: I show in Fig. 34 a frame for which I was partly responsible, which was built towards the end of 1910, at a time when we were not troubled with gear-boxes. The ballhead was at least 3 in. longer than the usual ballhead of to-day, which gave the machine an unusual appearance.

Duplex tubes, running straight from the top of the head to the back spindle, were used for the top member, in order to drop the tank between them and to obtain maximum tank capacity.

The following are the advantages which the makers claim for this construction: (1) The centre of gravity is extremely low;

(2) The rider is placed approximately midway between the wheels. The consequence of No. 1 is, that the machine ensures great security in steering, is very stable on the road at all speeds and does not easily slip. The consequence of No. 2 is that the rider is less subjected to the roadshocks, and in this respect the design resembles a spring frame.

The makers also informed me that they have experimented with every type of spring frame, and have come to the conclusion that no matter how such a frame is designed, it always causes slipping, owing to the driving wheel being sprung, and in addition great insecurity in steering is experienced.

Further, they also informed me that all British and American frames fracture in Holland, as the roads are extremely bad.

Mr. J. F. Berry: There is another type of frame which is very similar to the one mentioned by Mr. von Eugen, known as the Cotton frame, which is now being made. In this frame there are duplex tubes running from the top and bottom of the head down to the centre of the back spindle, and a duplex tube also running down from the head to the front of the crank case, and from the back of the crank case to the centre of the rear wheel. I should like to know whether the author would regard this as being a mechanically correct frame, because it seems to me to be triangular throughout, as it should be. The only two bad things about this frame are that if a very long stroke engine is fitted, an unduly high frame will result, and also that the two stays running from the back of the crank case interfere very largely with the chains. Apart from this, however, I think it is a mechanically perfect, triangular frame.

Mr. F. A. S. Acres: I should be glad if the author could make a little bit clearer the first sentence on page 230: "A force acting in a certain direction and applied at a point may be resolved into two other forces acting in the same plane and along any other two lines diverging from the point except at right angles to the said direction." I think it is true that both lines cannot be at right angles to the original line, but it is quite possible to have one at right angles to it.

The President: Before asking the author to reply, I would like to say that I think the reason we have not had a long discussion is that there is no one who would like to question the facts which the author has put forward, and which are absolutely

(The President.)

true theoretically. As Mr. Norton has pointed out, however, there are many practical difficulties to be met with on the theoretical path, not only in this case, but in many other instances of motor car and motor cycle design. With regard to the question of the effect of heat and the file marks on the strength of the joint, Dr. Hatfield has pointed out how very essential it is not to have a sharp change in section, however small.

In connection with torsional stresses, I would like to call the attention of designers to what I believe to be one of the most remarkable papers which has been written on this subject in recent times. I refer to that given by Messrs. Griffith and Taylor before the Institution of Mechanical Engineers on December 14th, 1917, on "The use of Soap Films in solving Torsion Problems," in which the soap bubble method of determining torsional stresses was described. This bears out in a very marked degree the point which has been raised.

I cannot think why it should be so difficult to obtain satisfactory malleable castings, although it seems a generally well-known fact. I believe suggestions have been made for the formation of a Malleable Castings Association, and in view of the excellent work which has been done in the United States on this point it does seem desirable that something on these lines should be carried out. I would refer any one interested in the matter to a paper by Mr. Enrique Touceda on "Research Work on Malleable Iron," read before the American Society of Mechanical Engineers, June, 1919.

Mr. Leechman, in replying on the discussion, said: I take it that on the whole members agree with the suggestions made in Fig. 11 in regard to the nomenclature of motor bicycles. Mr. Norton, however, referred to what I called the middle tube as the cross rail. To me "rail" is suggestive of something less mechanical than we are accustomed to deal with in frame construction, and "cross rail" suggests transverse location, and would be more applicable to the handle bar than the middle tube. Some call it the tank tube, but if the tank were to disappear from its present position, that term would then be a misnomer. Nor do I agree that the bottom tube should be called the down tube, since the seat tube is just as much a down tube. Perhaps in the course of time we shall evolve something better.

I did not object to castings, in the sense of lugs, generally, but to those big, heavy, compendious castings, which cover a multitude of sins.

There may be some difficulty in arranging straight duplex chain stays, but it should be possible to get over that. In a machine I had built up some time ago, I found it was at least possible to get only very slight bends in these lower tubes.

As to the front forks, I think Mr. Norton's design is good, that is to say, he forms two triangles for the two sides, bringing them out sideways and then down to the axle. It must be remembered that the wheel spindle in connection with Mr. Renouf's fork destroys the triangulation. One of the American machines has three members in each fork side, two of them running straight, while the middle one is bent out so as to present a triangular section running to points at the top of the head and the fork end. Judging by its rapidly increasing use, the superior appearance and other advantages of the saddle tank outweigh all the objections raised by Mr. Norton.

Major Shilson commented on the evils of filing up brazed work, and in that connection I should like to point out that nicking is a very serious defect, and is liable to occur with any joint which has to be filed. This defect is not so serious when the tube held in the lug is in tension and compression only. It is where the tube is subjected to transverse stresses that nicking is so vital, although it ought not to be permitted in any case. I am obliged to Major Shilson for giving his formula for the weight of tubes.

Again and again we have had machines brought out, the great feature of which was that the tubes were duplicated, but in almost every case these frames have disappeared, and I think it is for the reason that I have pointed out, namely, that duplex tubes in themselves are not economical from the torsional point of view. That, I think, is an objection to the Cotton frame, which, however, apart from that, is a very excellent attempt to triangulate at least the upper part of the frame. The fact that it gives rather a small space to the engine is a minor difficulty, easily got over by inclining the engine, and there is a good deal to be said in favour of the inclined engine as against the vertical engine. It is a pity that the lower part of the frame is not continuous, and I understand the bottom stays are cranked. I did not propose to say much on such subjects as the prevention of weakening of tubes by rusting, because I was very anxious to keep to the motor

(Mr. Leechman.)
bicycle frame as we know it to-day, and avoid general questions, but I think we might with great advantage have a paper some time on the methods of building up tubular structures, and such practical points as brazing. With regard to the other point mentioned by Major Shilson, high carbon tubes are much more rigid than ordinary tubes, and should be advantageous. I quite agree with Mr. Acres that my quotation from Dr. Lilly is extremely narrow, but I did not consider that this paper was the proper place for a detailed mathematical treatise on the subject. I would refer those who wish to pursue it further to Dr. Lilly's paper on "The Economic Design of Hollow Shafts," read before the Junior Institution of Engineers, Oct. 18, 1907.

I would also refer those who wish to study the effect of the height of the centre of gravity in motor bicycles to an article in the "Automobile Engineer" of July, 1914. It is the fact that the opinion of riders generally is strongly in favour of a low centre of gravity, and I think that there may be something in having the engine low that we do not appreciate. If the popular view is right, I feel sure it is not purely a question of the position of the centre of gravity. Theoretically, I agree with Mr. Acres; there is all the difference between a stable and an unstable or balanced vehicle in this connection. I accept Mr. Acres' correction of the passage on page 230.

In reply to Mr. Joyce, the engine in Fig. 33 should be mounted on proper bed plates in the bottom angle of the triangle. The construction is better than that shown in Fig. 24, because the tubes form a triangular unit instead of a discontinuous quadrilateral of which the crankcase forms the fourth side. The next best thing, perhaps, would be the loop frame, but I would point out that the liability of the engine to rotate backwards is overcome in Fig. 33 by the duplex chain stays, which practically form a torque member for the engine.

I am afraid it would take a long time to convince Mr. Holroyd that a three-sided figure is better than a five-sided one, but it is an elementary fact which has long passed out of the region of argument.

Mr. Van Eugen showed a Dutch design which was good in the upper part, and had a splendidly long head, but it was not so good in the lower part.

Sir Henry Fowler referred to Messrs. Turner and Griffiths' paper on soap bubbles in connection with torsion tests, and that

is certainly a paper which everyone should read. Another somewhat allied subject is photo elasticity, in which by passing polarised light through celluloid models and stressing these models, variations of colour in different parts of the model on the screen indicate the stresses and the degrees of stress to which the different parts are subjected. The particular value is that it shows not the results of testing to destruction, but the effect of stresses as they are actually encountered in practice.

COMMUNICATIONS.

Mr. Eric Caudwell wrote: My own feelings are that the author has not been severe enough with the defects of the majority of conventional frames being manufactured to-day. I once heard an inventor make the statement that he loved tubes because he could do anything with them. Such an attitude of mind gives a clue to the origination of many of the defects from which motor cycles suffer. Perhaps it is because it is so easy to do things with tubes that we are afflicted with so many bad frames, and if tubes could be neither bent nor brazed we might be much nearer mechanically correct frame structures than we are to-day. The conventional tube and malleable cast iron brazed structures show a lamentable lack of originality which has now gone on so long that it can only be justified by immediate considerations of commercial necessity. We commenced by fitting a motor to the pedal cycle, and few people appear to have got beyond it, and the time appears overdue for the question to be thought out afresh on original lines.

Supposing we adopt a frame of the type shown in Fig. 33. On paper it looks quite sound, but I submit it would still suffer from the worst features of the conventional types, i.e., lugs and brazed joints. It is fairly well known by now that the great majority of frame breakages occur at the lugs, particularly where these are of malleable cast iron and not steel, and the point to be noted is that the breakage is due to the weakening of the tube in the process of brazing and possibly of filing up afterwards. I have recently examined a well-known 4 h.p. single-cylinder machine, and discovered that in the front forks, frame, handle bars, and stand, there were just fifty brazed joints, every one of which is a cause of weakness and possible point of breakage. While the weakness due to brazing can be overcome by the use of solder, which is quite satisfactory and reliable if the joint is long enough, when all is said and done lugs form a very heavy and expensive way of connecting tubes, and it would mark a very much greater step in progress if the author's frame could be pin-jointed together with bolts and nuts.

The author draws attention to the well known properties of the tube in resistance to torsion, but in a correctly triangulated frame the stresses are all tension and compression, except those due to rolling, to which the author refers. I should be glad of some further explanation of this point. The only rolling I know anything about is that arising from sloppy forks and loose wheel bearings, or else running with soft tyres, particularly those which have heavy treads and thin walls.

For acting as ties and struts other sections besides tubes are equally suitable, and there is no reason why channels could not be used if employing a frame in which bending loads are applied to its members, as, for instance, the ordinary beam of the motor car side member.

There does not appear much scope for a new solution of the frame problem if we retain the conventional disposition of power plant, gear-box, tank, etc., but why should we, if any considerable advantages are to be derived from a re-arrangement? A fundamental fault is the employment of the engine as a link in the frame. If it must form part of the frame at all it should be a member, as in the P. & M. design, which could very easily be completely triangulated, or a king post, as in the Scott, mentioned by the author. Another difficulty with the tubular frame is in the attachment of loose parts, which must be effected by split clamps or lugs brazed on the tube. The latter are highly objectionable, the only thing worse being to drill holes in the tubes, which I have seen done, particularly on American machines, though a reference to Messrs. Hacketts' paper should cure anybody who has been guilty of such a practice.*

At the first meeting held in Birmingham, nearly two years ago, Mr. Peters demonstrated, by means of models, the effect of the torque reaction of the engine in putting bending stresses on the bottom and seat tubes, which ultimately result in their fracture, and I should like to point out that the author's design of frame would not overcome this very real fault, as his engine is not attached at the point of junction of the two tubes, and I do not well see how it could be.

In conclusion, I will revert to my earlier remarks as to the probability of a solution being found in a re-arrangement of the power plant, and would refer to the well known 2½ h.p. F. N.

* See Proc. I.A.E., Vol. XIII., p. 289.

(Mr. Eric Caudwell.)

machine, in which the engine is placed with its crankshaft in the plane of the wheels instead of at right angles to them. The frame of this machine as now constructed is by no means ideal, but I think any designer who sympathetically studies it will appreciate that it could be very easily triangulated, while at the same time the disposition of the engine would enable the torque reactions of the engine and gear-box to be taken as torsion on the tubes if such were employed instead of as a bending load as in most other frames. Such an arrangement also leaves the way clear for the employment of a shaft drive, which I consider will prove the final solution of the transmission problem.

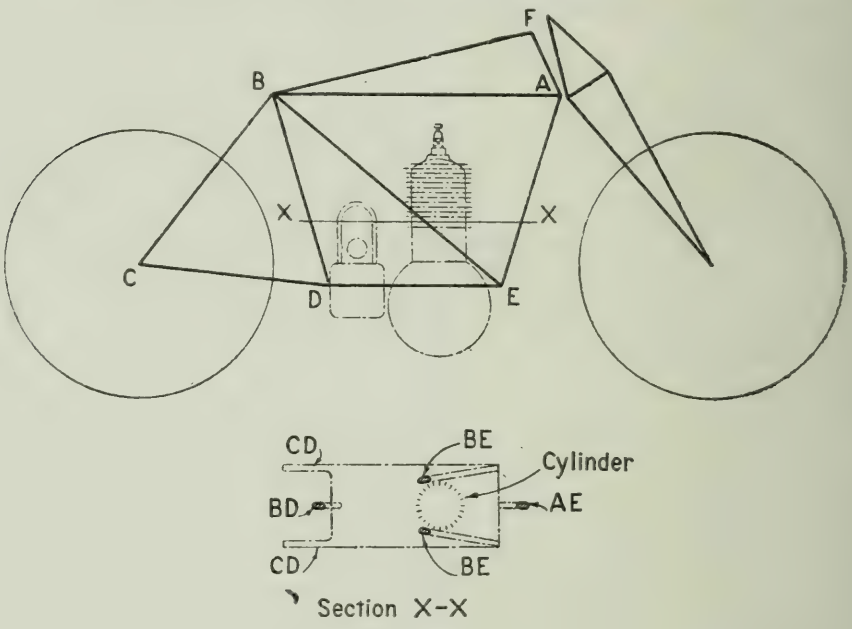


FIG. 35.

Regarding the horizontally opposed twin, it appears that the most mechanical frame is likely to be obtained with the engine placed across it as in the case of the A.B.C., though unless an unusually short stroke be employed this could not be done with an engine much over 650 c.c. capacity. However, this arrangement offers other undoubted advantages in the direction of ever cooling of both cylinders, and the possibility of employing a shaft for the final drive.

Mr. P. J. JOYCE wrote: The frame shown in Fig. 33 is undoubtedly an improvement on the majority of frames, but I think it could be improved and simplified in the following manner:—

The tubes AB, AF, BF and BC in Fig. 35 are precisely as shown in Fig. 33. The horizontal platform DE is supported by the seat tube BD and the bottom tube AE, and is held on each side at the rear by the chain stays CD, whilst the forward end is held on each side by the front stays BE which terminate at the seat lug casting. This method of construction provides the engine and gear-box with a "bed" or foundation, upon which they are mounted by bolts in a vertical plane, the gear-box being adjustable by elongated holes and also makes the frame a complete structure without the engine, points which the author advocates as constituting "the ideal frame."

The platform DE may be a rigid plate provided with slots for the crank case and gear-box, or a built-up rectangular tubular structure.

The transmission stresses between the engine, gear-box and rear wheel lie in a plane which practically coincides with the tubes CD and DE, and as the transverse and torsional stresses are greatest at the point D, it has five tubes converging there to resist them.

The side elevation of the frame comprises four triangles, the whole forming a theoretically perfect structure.

In reply to the written communications, Mr. LEECHMAN wrote: I agree with Mr. Eric Caudwell's objections to brazed joints; soft soldering is better as involving a lower temperature and reducing the risk of nicking. Pin jointing is all that is necessary up to a point, but I do not care to depend merely on the friction grip of a nut; trapping does not form an ideal end for a tube, and where several tubes meet at an angle, a bolt and nut connection is liable to lead to serious displacement of the tubes from the plane of the frame. Mr. Caudwell may learn more about roll if he will ride some of the duplex frames I have referred to in the paper. Roll may also arise from lateral spring of the frame, owing to the chain stays giving under the transmission of the driving force. Channels are not as good as tubes for struts, as I have shown at page 239.

Mr. Joyce's design (Fig. 35) is nicely triangulated, but it is rather complex and the tubes BE obstruct access to the engine.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

JANUARY, 1921.

The Sixth Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1, on Wednesday, January 12th, 1921, at 8 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following list of Members who had completed their election since the previous Meeting was posted up:—

MEMBERS.

ALBERT RICHARD NORMAN HEATH (Dartford).
WILLIAM JOHN MORISON (Chelmsford).

ASSOCIATE MEMBERS.

STEWART JOHN BURT (London).
HENNETH ADAIR HELLON (Ponders End).
ROBERT EDGAR MORGAN (Birmingham).
JOHN GUNTHER RICKARDS (Ipswich).
GEOFFREY HERBERT SAVAGE (London).
DONALD STEVENSON (High Wycombe).

ASSOCIATE.

WILLIAM PERCY JOHNSON (Loughborough).

Graduates.

Colin Malcolm Dormer (London).
John Clifford Jaeger (Birmingham).
Robert Hicks Kidd (Cambridge).
Edna Joan Linford (Miss) (Loughborough).
Alfred Cyril Lovesey (Bristol).
Joseph Harry Victor Wood (Birmingham).
Edgar Pountney Willoughby (Birmingham).

Probationary Graduates.

Herbert Edmund Browne (Wembley).
Francis George Eric Clarke (Birmingham).
John Akhbar Douglas Mahomed (London).

Transfers.

Henry Elvidge (Coatbridge), Associate Member to Member.
Charles Carleton Saunders O'Mahony (Aldershot), Associate Member to Member.
Arthur Tate Simpson (Hull), Associate Member to Member.

The following paper was then read and discussed:—

"Suggested Lines of Development in Transmission," by Capt. S. Bramley-Moore.

There were present 108 Members and visitors.

SUGGESTED LINES OF DEVELOPMENT IN TRANSMISSION.

By Capt. S. BRAMLEY-MOORE, M.C.

The author's intention in this paper is to discuss certain developments in clutch and gear-box design with a view to reducing cost of upkeep. It has been said that more money is spent on the repair and maintenance of the average transmission system than on the engine itself. This may or may not be true, but it is certain that modern high-powered engines, capable of quick acceleration, can easily overload all parts of the transmission gear. Probably, however, more trouble is caused by difficulty in gear-changing and careless clutch manipulation, the latter providing a steady source of revenue to tyre manufacturers.

Clutch Design.

If the ingenuity expended on modern disk and plate clutches had been bestowed on cone clutches, the latter would have proved so cheap, simple and satisfactory that the former would have been regarded in the nature of an extravagant luxury. The development of the cone clutch has been sadly neglected, whilst other types have been "nursed" into public favour.

Clutches drive by friction, and surely there can be no more effective form of friction drive than the tapered wedge action of two conical surfaces. The disk clutch is, moreover, theoretically incorrect, due to the extra spring pressure required to drive by friction between flat plates, as compared with the wedge-shaped cone, and the uneven wear due to different rubbing speeds, owing to the fact that at every different radius of the plate there is a different velocity. On the cone clutch this variation in rubbing speed is so slight that it is generally considered negligible.

When clutch springs are evenly spaced round the face of the disk it is probably impossible to guarantee that all these shall exert the same pressure, and any difference in pressure will cause uneven loading and uneven wear, the portion adjacent to the strongest spring attempting to carry more than its proper share of the drive. In spite of these inherent drawbacks the disk clutch has been so carefully developed that certain designs give very satisfactory service.

The ordinary plate clutch runs in a bath of oil, and its successful action depends upon efficient lubrication. If the oil gets too thick or congeals it will prevent the driving and driven plates from separating properly when the clutch is disengaged. This will cause the car to "creep" in traffic although the clutch pedal may be fully depressed, and it will also greatly increase the difficulty of gear-changing. When starting from rest, the teeth of the first speed gears are liable, unless really efficient clutch stops are fitted, to grate and rub together as they are brought into engagement, owing to the continued rotation of the driven portion of the clutch. This difficulty is worse in cold weather, especially if the car has been standing for a few days. In the case of a well-known American car, in which the plate clutch has to be disengaged before starting up the engine, the danger is so great that it is frequently necessary, during the winter season, to jack up the rear axle before "winding up." Thin oil, on the other hand, is liable to make the clutch too fierce.

The Cone Clutch.

The ordinary leather-faced cone clutch—the pioneer type—is simple, cheap and reliable, but frequently fails to provide for a sufficiently gradual engagement when starting up from rest. This is due to the fundamental absurdity that the moment the clutch surfaces come into contact, the full pressure of the clutch spring comes into immediate operation. When the driver begins to release his clutch pedal, the clutch surfaces have not begun to make contact, and then, all too suddenly, the cones engage. At this critical point the driver very often lacks sufficient control, or the requisite delicate touch, to prevent a sudden and jerky picking up of the load.

Figs. 1 to 3 show a clutch designed to overcome this most

objectionable characteristic of the ordinary type. The clutch cone B is operated by the sleeve B1 controlled by the collar B2. The

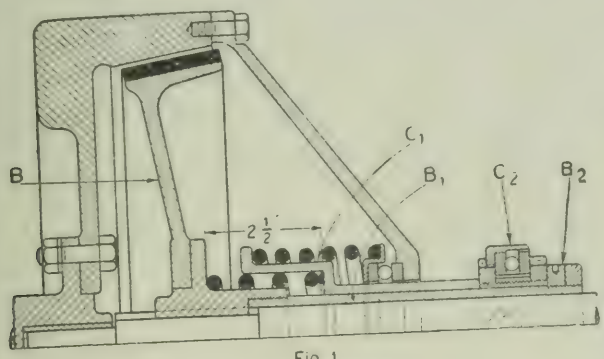


Fig 1
Clutch disengaged

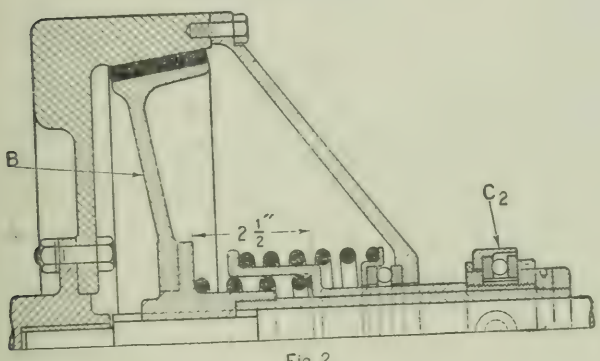


Fig 2
Partly engaged

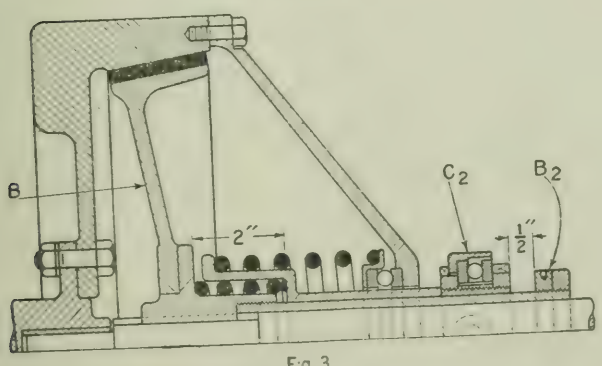


Fig. 3
Fully engaged

FIGS. 1, 2 AND 3.—Suggested design for double spring clutch, three positions.

inner and outer clutch springs are separated by the sleeve C1 controlled by the ball thrust collar C2, operated by a clutch pedal

in the usual way. The inner spring acts directly on the clutch cone B, whilst the outer spring acts on the sleeve C1.

Fig. 1 shows the collar C2 withdrawn, thereby disengaging the cone B through the medium of the collar B2.

Fig. 2 shows the collar C2 partially released, thus allowing the cone B to engage with the flywheel. The inner spring is, however, as yet exerting no pressure on the cone B, so that the

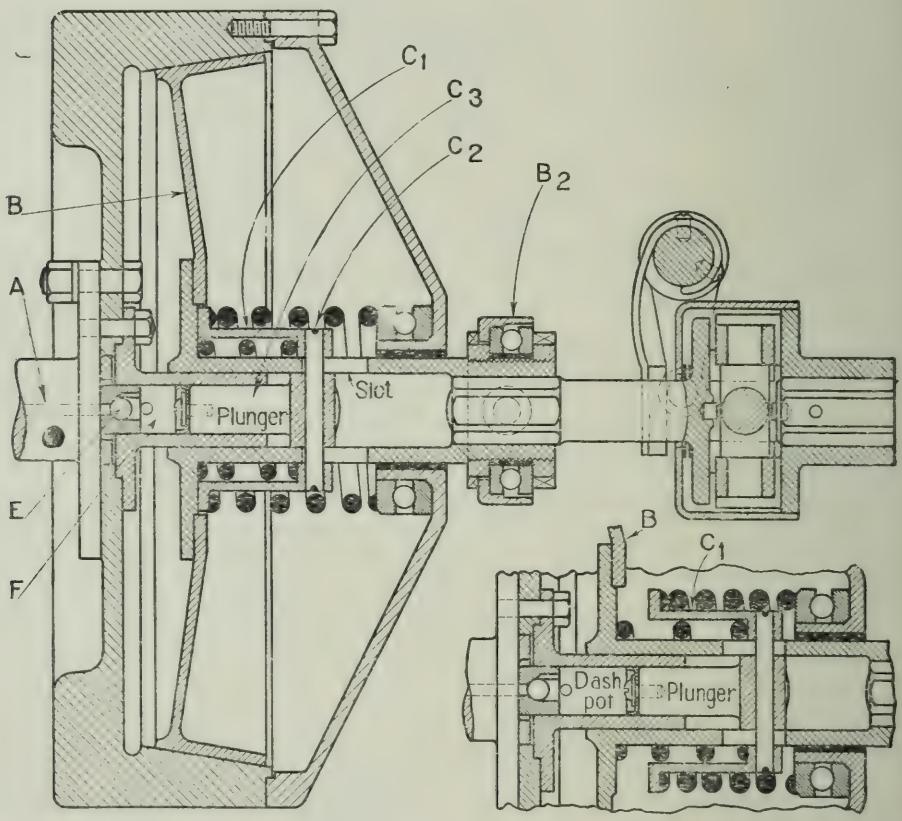


FIG. 4.—Suggested design for clutch with automatic engaging device.

latter is only able to transmit a small torque—not sufficient to cause any jerk or jar. If, from this position, the clutch pedal be gradually released, the pressure of the inner spring will gradually increase, thus permitting the clutch to pick up its load in a smooth and uniform manner.

Fig. 3 shows the collar C2 entirely released and the clutch fully engaged. Note that the inner spring is now only 2 in. long, thus

exerting its full pressure, as against $2\frac{1}{2}$ in. in length in the previous figure. Also note the gap of $\frac{1}{2}$ in. between the collars C2 and B2, whilst these were previously in contact. This large travel is only shown for illustrative purposes, and is not required in actual practice.

Fig. 4 shows an additional device fitted to this type of clutch with a view to eliminating the "human element." In this case the sleeve C1 is secured by a pin C2 to a boss forming part of the plunger C3. When the cone B is withdrawn (by means of its operating collar B2) it simultaneously moves the sleeve C1 and plunger C3, thus allowing oil to be sucked through the passage A and ball valve E into the chamber F, the engine being provided with a hollow crankshaft and forced feed lubrication. If the clutch pedal be suddenly released, the cone B will re-engage with the flywheel, but the sleeve C1 will be left behind owing to the dashpot F. This position of C1 is illustrated in the bottom right-hand corner of Fig. 4, and in this position the inner spring is extended so that it is exerting little or no pressure on the cone B.

As, however, the oil from the chamber F is gradually forced back into the hollow crankshaft (through a narrow groove always open) by the pressure of the outer clutch spring, so the pressure of the inner spring, acting directly on the cone B, will gradually increase, thus allowing the clutch to gradually pick up its load without any possibility of jerk or jar.

It is now proposed to show how the drawbacks of the ordinary cone clutch can be overcome by means of a double cone and single spring, in place of the double spring and single cone already described.

In the early days of motoring, when the cone clutch was almost universally adopted, many experiments were made to decide the most suitable angle of cone to give the best all-round results. The problem was how to employ a light spring without unduly increasing the diameter of the clutch. The larger the angle, the less the possibility of a fierce clutch, but the greater the difficulty in transmitting the drive, notwithstanding the use of a powerful spring. The smaller the angle, the lighter the spring, but great care was necessary in engaging the clutch, otherwise the engine would frequently be stopped, or the car jerked into motion, owing to the sudden taking up of the load.

Fig. 5 shows a clutch combining the good points of both these

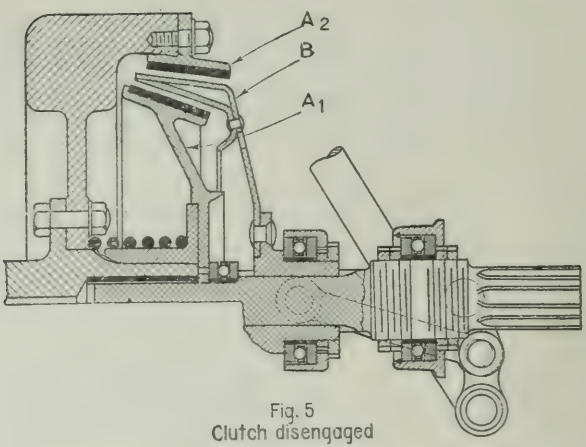


Fig. 5
Clutch disengaged

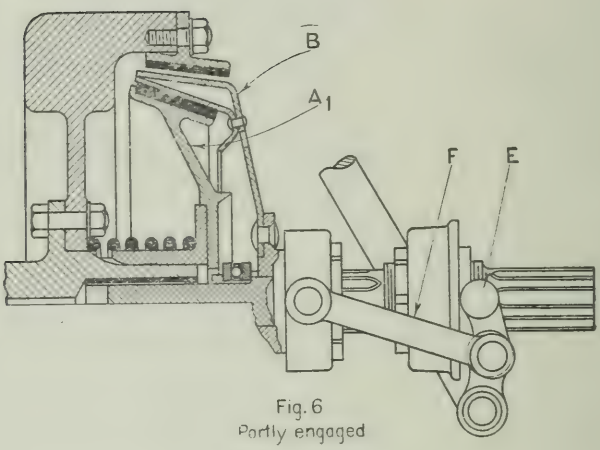


Fig. 6
Partly engaged

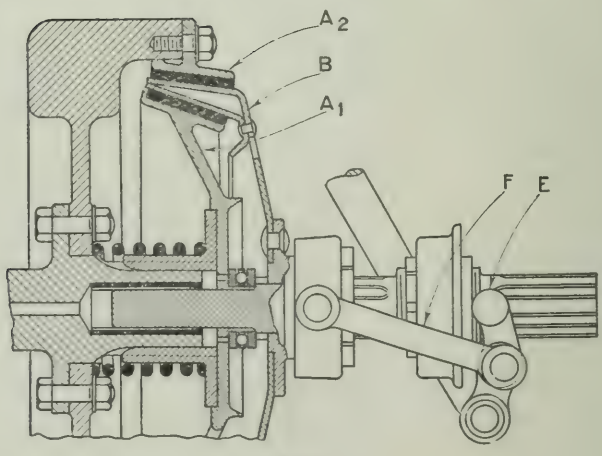


Fig. 7
Fully engaged

FIGS. 5, 6 AND 7.—Suggested design for double cone clutch, three positions.

two extremes. It comprises two driving cones, an inner and an outer, the inner cone having a large angle, and the outer cone a small one. On first releasing the clutch pedal only the inner cone, with the large angle, comes into operation (see Fig. 6), and, owing to the use of a light clutch spring, this part of the clutch is only able to transmit a small portion of the power—not sufficient to cause any shock, no matter how suddenly applied. In other words, the inner cone will be compelled to slip, and this slip will allow the clutch to pick up its load gradually. Once the car is in motion the clutch pedal is fully released, thus allowing the outer cone, with the small angle, to pick up the drive (see Fig. 7), and this without any risk of further slip, and with a minimum of both spring pressure and clutch diameter, which could not possibly be obtained with the orthodox pattern of single cone clutch.

In Figs. 5 to 7 the cones A1 and A2 are the driving members, both rotating at the same speed, and B is the driven member.

Fig. 5 shows the clutch entirely disengaged, all three cones A1, B and A2 being out of contact with each other. In this position the clutch pedal is, of course, fully depressed.

Fig. 6 shows the position of the clutch for starting the car from rest. Only the cone A1 has engaged the driven member B, and this portion of the clutch is so designed that it does not possess sufficient "grip" to jerk the car, although it is able to set the vehicle in motion. This characteristic is due to the large cone angle and light clutch spring.

Fig. 7 shows the clutch fully engaged, the driven member B being sandwiched between the two driving cones A1 and A2. In this position the clutch is able to transmit the full power of the engine. There is no possibility of slip because of the powerful grip of the outer cone A2 due to the small cone angle.

When the clutch pedal is first released, the cone A1 engages with the cone B before B engages with the cone A2. This is due to the fact that the cones A1 and B travel at a different rate. The driven member B is operated by means of the "slow moving" link F, which is pivoted half-way up the "fast moving" operating lever E, so that the end of E will move approximately twice as far as the link F, because the radius of the former is twice that of the latter. The terms "slow moving" and "fast moving" are, of course, used only in a relative sense.

Universal Joint Design.

It may surprise some to hear that the "universal joint" in common use is not universal, and in all probability there is no automobile on the road provided with a coupling which gives a universal movement. The ordinary pivoted form of coupling permits of movement in an angular direction only, and is known as a Hooke's joint. One such joint is unable to convey motion between two parallel axes not in the same straight line, in which case two joints must be used. For this reason two joints are generally fitted between the engine and gear-box of a motor vehicle when these two units are independent of each other. The Oldham coupling, on the other hand, is designed to convey motion

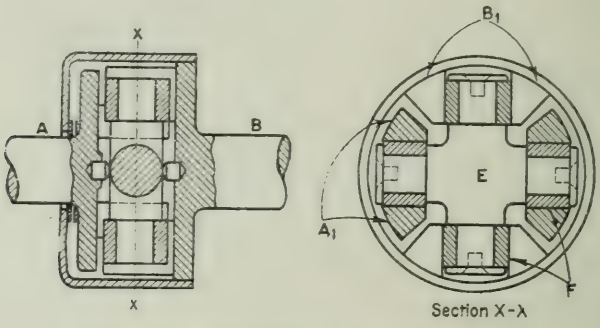


Fig. 8.—Suggested universal joint.

between any two shafts of which the axes are parallel but not in the same straight line, but is not adapted to accommodate movement in an angular direction.

Fig. 8 shows a coupling which is a universal joint in the true sense of the word, as it combines the movements of the two types of couplings already referred to. The driving shaft A carries jaws A1 and the driven shaft B carries jaws B1. Motion is communicated from one to the other by means of the cross piece E which is fitted with driving blocks F. If the blocks F were rigidly fixed to the jaws A1 and B1 respectively, then an ordinary type of Hooke universal joint would result, providing angular movement only. If, on the other hand, the blocks were fixed to the arms of the cross-piece E, so as to prevent any rotary movement, then the cross-piece and blocks would be left with a sliding motion only,

this movement corresponding to that of an ordinary Oldham coupling.

But as the arms of the cross-piece E are free to pivot about the driving blocks F, and as the blocks F are also free to slide in the jaws A1 or B1, it will be seen that this design combines the characteristics of the Hooke and Oldham joints in one coupling. It therefore follows that one such coupling, placed between the engine and the gear-box of a motor vehicle, will meet all the requirements of both angular and parallel displacement.

Gear-box Design.

There is no need to waste time in introducing this subject—it

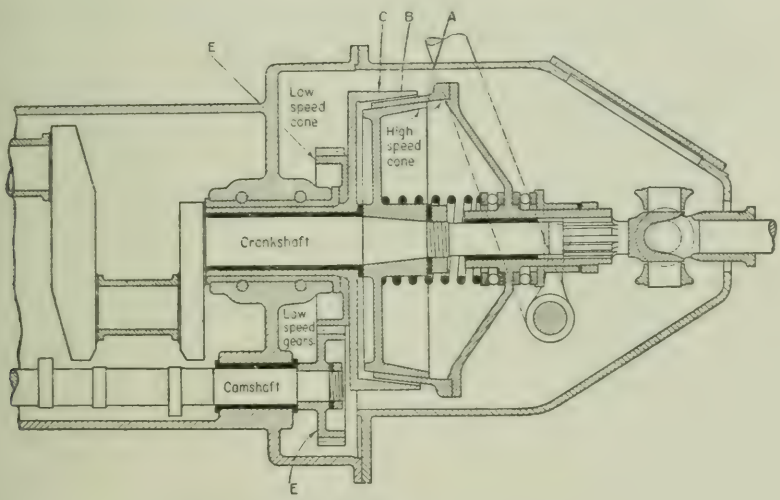


Fig. 9.—Suggested two-speed gear.

is an acknowledged fact that the ordinary clash type of gear-box is unmechanical and unsatisfactory. Experts pretend that there is no difficulty in gear-changing, but owner-drivers know otherwise. During five years' Army experience the author has come in contact with thousands of drivers, being at one time responsible for the technical training of nearly two thousand men per week, and he is in a position to state definitely that the ordinary man regards the ordinary gear-box as an extraordinary abomination.

Fig. 9 shows a simple type of two-speed gear designed to

eliminate unnecessary gear wheels. The driven clutch cone A is provided with two faces, an inner and an outer. The inner face is able to engage with the high-speed cone B, which is keyed to the crankshaft of the engine. The outer face is able to engage with the low-speed cone C, which is mounted loosely on the rear end of the crankshaft, being driven from the camshaft by the low-speed gears E, E.

Under ordinary conditions the clutch spring keeps the cone A in engagement with B, thereby providing the high-speed direct drive. For the low speed the clutch pedal must be depressed,

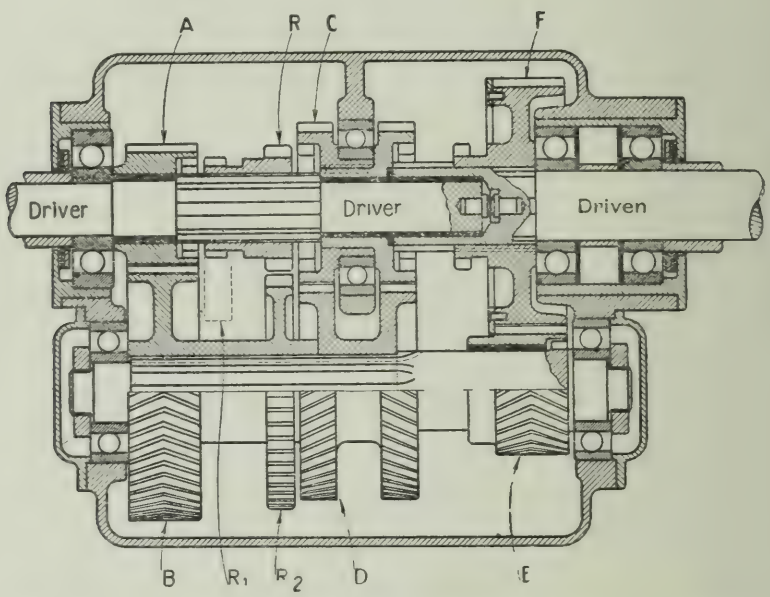


FIG. 10.—Suggested design for double-helical gear-box, 1st and 2nd speeds.

thereby causing the cone A to engage with the low-speed cone C. In this position the drive will pass through the two-to-one timing gears (which are not shown in Fig. 9, being at the front end of the engine), and then along the camshaft and through the low-speed gears E E to the low-speed cone C. The camshaft is designed to carry this extra load, or alternatively the timing gears can be placed alongside the gears E E. With the arrangement shown in Fig. 9, the flywheel is located at the front end of the engine, and the reverse-speed mechanism forms part of the differential gear in the back-axle. The design of this gear is both

cheap and simple, the only extra parts required being two gear wheels and an outer cone.

Figs. 10 and 11 illustrate the application of double-helical gears to a four-speed gear-box. Double-helical gears, unlike the ordinary straight spur type, give continuous rolling contact on the pitch line. They can also withstand a greater stress, for there is, at any instant, a stress on one portion of the tooth only, the other portions being out of engagement, whilst on a straight spur the stress is continuous along the whole length of the tooth.

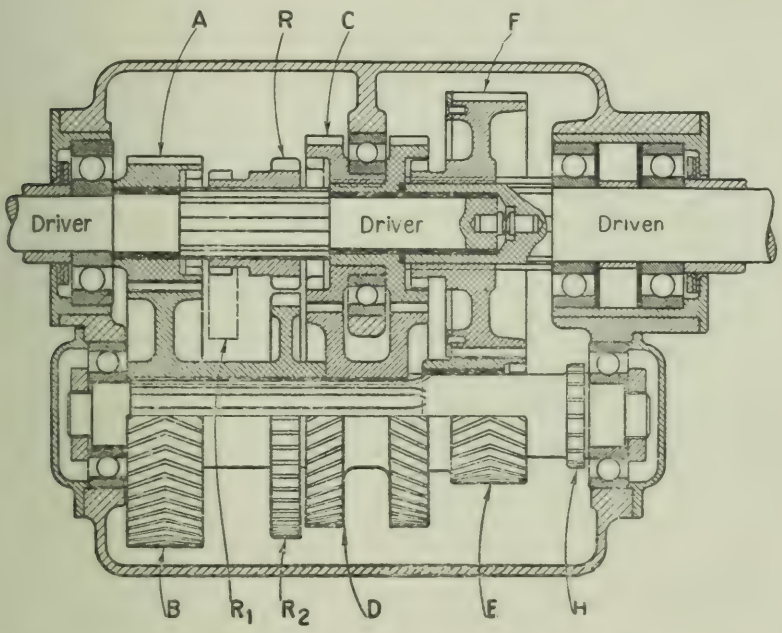


FIG. 11.—Double-helical gear-box, 3rd and 4th speeds.

They also possess the great advantage—for automobile work—of silent running, the general design of the gear enabling a finer pitch to be employed than would be possible with a corresponding straight spur. A Double-Helical Generator has recently been made by Messrs. David Brown & Sons adapted to cut gears suitable for automobile work with teeth cut from the solid in one continuous piece.

In the ordinary way, double-helical gears are not adapted to an automobile four-speed gear-box owing to the difficulty of restricting the length of the box to reasonable dimensions. Every-

body knows that the ordinary four-speed gear-box possesses a *minimum number* of eight gear wheels, excluding the reverse-speed gear wheels. A unique characteristic of the present design, however, is the fact that only six gear wheels are required, in order to provide four forward speeds, in place of the usual eight. The four speeds are obtained in the following manner:—

With the position of the gears as shown in Fig. 10, move R to the left for the 1st speed and to the right for the 2nd speed. The drive will then pass through AB–EF and CD–EF respectively.

With the position of the gears as shown in Fig. 11, move R to the left for the 3rd speed and to the right for the 4th speed direct drive. The drive will then pass through AB—DC and RCF respectively. Between Figs. 10 and 11, the gear wheel F has been moved to the left so as to engage with C, carrying the gear pinion E with it owing to the employment of double-helical gear teeth.

A reverse speed is obtained by moving the gear pinion R1 to the right when the gears are in the position shown in Fig. 10.

The chief characteristic of this gear, four speeds forward with only six wheels, is obtained by loosely mounting the gear wheel C at the junction of the driving and driven shafts, so that it may be locked to the driving shaft, or to the driven shaft, or to both together, thus enabling it to transmit power to, or receive power from, the countershaft in addition to transmitting the top speed direct drive. Gear-changing is also greatly facilitated, as it is much easier to engage two sets of internal teeth, of which the shafts are rotating in the same direction, than to engage two spur gears, as in the ordinary type of box, of which the shafts are rotating in opposite directions.

The right- and left-hand helicals of gear wheels C and D are shown separated by a gap, in order to incorporate the ball bearing supporting C. These are, of course, two gear wheels, not four, as both right- and left-hand helicals are generated simultaneously, and the gears could just as easily be joined together, like B or E, if so required.

Fig. 12 illustrates a new type of four-speed gear-box, and Figs. 13 to 16 show the manner in which the various speeds are obtained. The characteristic feature of this box is a double-acting cone friction clutch in conjunction with six gear wheels arranged in the special manner already described so as to provide for a

range of four speeds. The double clutch provides for a change of speed from 1st to 2nd or from 3rd to 4th (or *vice versâ*) without touching the gear-box change-speed lever, thereby greatly facilitating the operation of changing speed.

The driving member of the clutch, A, is provided with two faces, an inner and an outer, so that it is able to engage with either of the driven cones B or C. The cone B is keyed to a sleeve carrying the gear pinion B2, and the cone C is mounted on a castellated shaft C1 to which the gear pinion C2 is also keyed. The pinions B2 and C2 engage with two gear wheels E1 and E2 mounted on

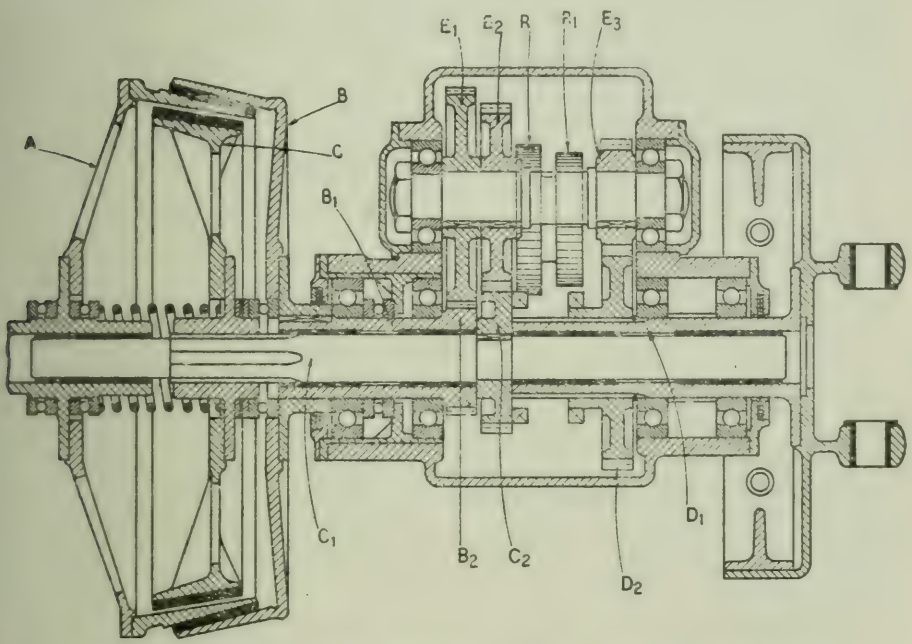


FIG. 12.—Suggested design for Four-speed gear-box.

the countershaft; the latter also carries a gear pinion E3 engaging with a gear wheel D2 mounted on the castellated driven sleeve D1.

For the 1st and 2nd speeds the gear wheel D2 is engaged with the gear pinion E3. For the 3rd and 4th speeds the wheel D2 is moved over to the left, so that it engages, by means of dog clutches, with the gear pinion C2, which is thus locked to the driven sleeve D1. Thus the change-speed lever controlling the gear D2 has only to be moved when changing from 2nd to 3rd speed or *vice versâ*.

First Speed—Fig. 13. The clutch-pedal is pushed right forward so that the cone A drives the cone B. Then B2 drives E1 and E3 drives D2.

Second Speed—Fig. 14. The clutch-pedal is released so that

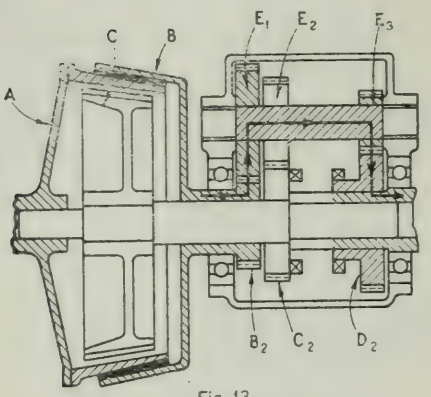


Fig. 13
First speed AB–B_2E_1–E_3D_2

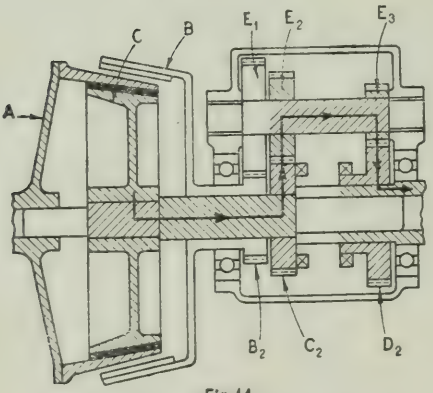


Fig. 14
Second speed AC–C_2E_2–E_3D_2

FIGS. 13 AND 14.—Diagram of proposed gear-box, 1st and 2nd speeds.

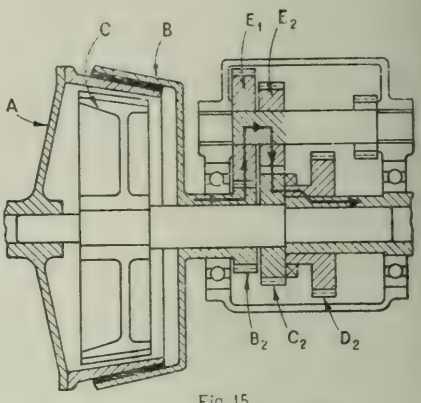


Fig. 15
Third speed AB–B_2E_1–E_2C_2

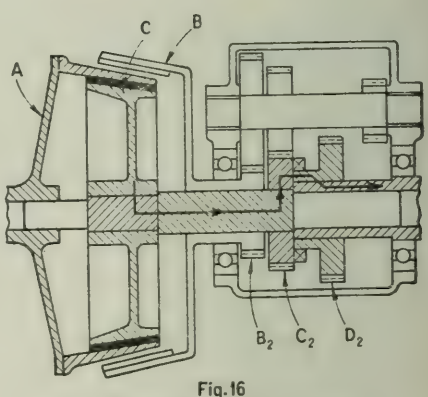


Fig. 16
Fourth speed AC–C_2D_2 direct

FIGS. 15 AND 16.—Diagram of gear-box, 3rd and 4th speeds.

the clutch spring forces the cones A and C into driving engagement. Then C2 drives E2 and E3 drives D2.

Third Speed—Fig. 15. The gear D2 has been moved over so as to lock the gear C2 to the driven sleeve D1, and the clutch-pedal is pushed right forward so that the cone A drives the cone B. Then B2 drives E1 and E2 drives C2.

Fourth Speed—Fig. 16. The clutch-pedal is released so that the clutch spring forces the cones A and C into driving engagement. Then C2 drives D2, thus giving a direct through drive.

For a reverse speed the gear D2 is left in a neutral position and the pinions R and R1 (see Figs. 12 or 17) are moved into mesh with the two gear wheels D2 and E3 respectively. In this position if the clutch-pedal be pushed right forward it will give a low reverse speed for traffic work, and if released will give a higher reverse speed for use on a country road—having, for example, overshot the desired turning by perhaps a hundred yards. The clutch is, of course, in neutral when the clutch-pedal is in any intermediate position.

The gate, as far as the forward speeds are concerned, is merely a single slot, one end of which is marked "slow" and the other end "fast." The "slow" position provides for 1st and 2nd speeds and the "fast" position for 3rd and 4th speeds. A ratchet device, capable of instant release, automatically holds the clutch-pedal forward when on 1st and 3rd speeds. For a momentary change, however, as when starting from rest, or when dropping into 3rd speed for a few seconds for traffic congestion, this ratchet would not be brought into operation.

It will be observed that the clutch angles of the inner and outer cones are different. As the inner cone is *never used for starting* the car from rest, a small angle may be used, although naturally the angle must not be decreased below that point at which the clutch might "stick," rendering it hard to disengage. A small angle enables a weak clutch spring to be employed, and this reduces the pressure required to hold the clutch-pedal in a neutral position.

The outer cone is used for starting the vehicle from rest, and a corresponding angle may be chosen, thus facilitating easy clutch engagement. The outer cone may be engaged *without transmitting any* drive, and then, as the pressure on the clutch-pedal is *gradually increased*, the clutch will *gradually pick up* its load without jerk or jar.

Clutch adjustment is rarely necessary owing to the fact that two clutch faces take the drive, one on the 1st and 3rd speeds and the other on the 2nd and 4th speeds, thus distributing the wear and prolonging the life of the clutch lining. In Fig. 12 suitable

packing washers are provided, however, which regulate the distance between the two clutch faces B and C should adjustment ever become necessary.

Fig. 17 shows a somewhat different design adapted to bolt up to the rear end of the engine crank case. In this case the gate and change-speed lever are carried by the lid of the gear-box, and self-locking clutch adjustment is provided as follows:—The disks M and N are screwed together, and can have no relative movement

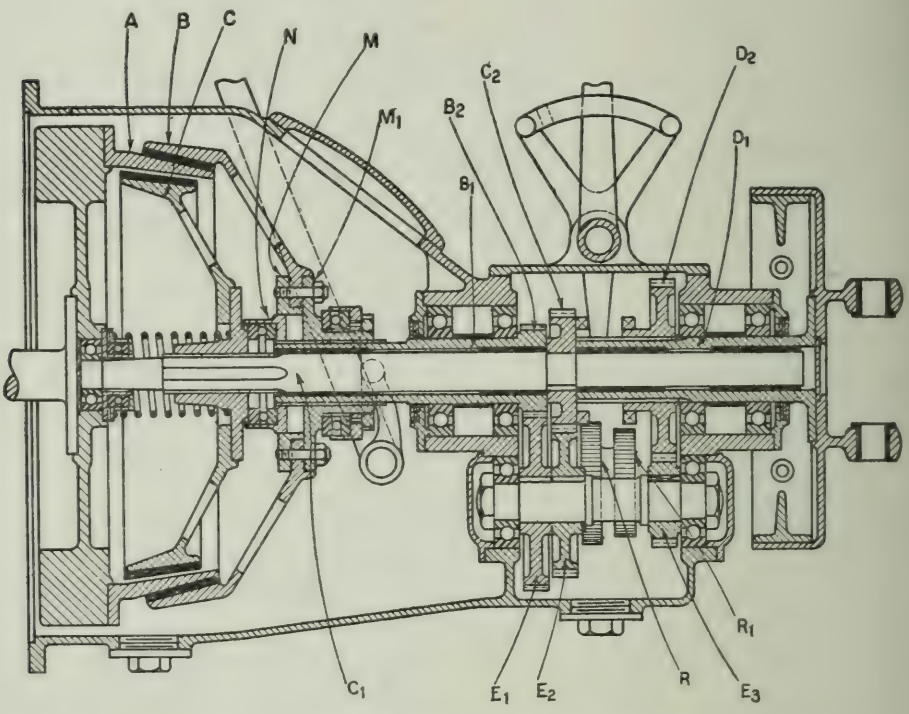


Fig. 17.—Suggested Four-speed gear-box (unit construction).

when the bolts M1 are in position owing to the castellations of the sleeve B1, the disk N being castellated to correspond. When, however, the bolts M1 are withdrawn, the cone B can be rotated about N, thus drawing the driving surfaces of B and C closer together to compensate for wear.

It may be remarked that this gear-box is 35 per cent shorter than the ordinary type, and requires only two selector rods in place of the usual three.

Epicyclic Gears.

The advantages of epicyclic gears are too well known to need emphasising in this paper. In the past, however, they have suffered, when designed to transmit three or four speeds, from two serious defects—weakness of construction and a general complication of parts. It is hoped that Figs. 18, 26 and 27 will open

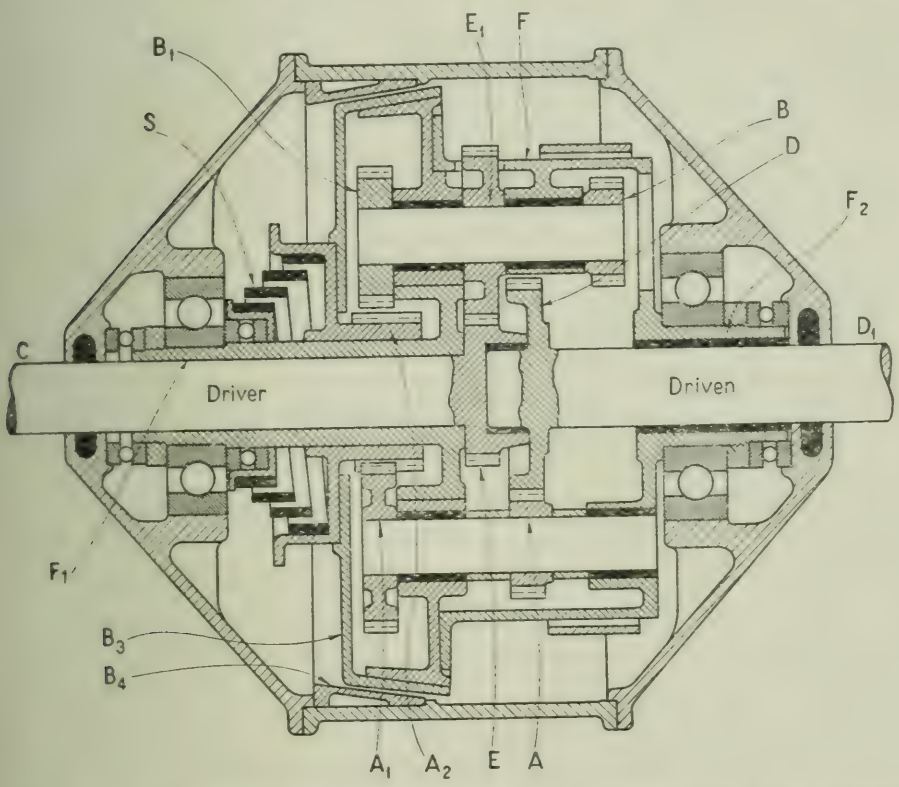


Fig. 18.—Suggested Epicyclic gear-box. Four speeds and reverse.

up a new point of view, and the more thoroughly these designs are discussed, the more helpful it will be for the future development of this type of gear.

In Fig. 18 the driving shaft C carries the sun wheel E meshing with the planet E1 mounted in bearings carried by the cage F. This cage is supported by the sleeves F1 and F2, the parts F and F1 being rigidly bolted together. Thus the cage F is of rigid construction, being carried by two large ball bearings, one at

each end of the gear-box casing as shown. The spindle of the planet E1 carries two additional planets, B and B1, the latter meshing with the planet A1 which engages with the sun-wheel A2 controlled by the clutch cone B3. For the sake of simplicity, the drawing shows the planets B1 and A1 separated from each other, although it must be understood that the spindles are actually arranged so that these two planets are in continual engagement with each other.

The driven shaft D1 carries the sun-wheel D shown in mesh with the planet A, but the shaft D1 is arranged to slide (being controlled by external operating mechanism), so that in its other extreme position the sun-wheel D can engage with the planet B.

The clutch cone B3 is provided with two faces, an inner and an outer, and for the top speed the clutch spring S forces the inner face into contact with the cage F, thus causing the cage, planets and sun-wheels to rotate as one solid mass, thereby providing a direct through drive. This position is illustrated in Fig. 18.

If, however, the clutch pedal controlling B3 be fully depressed, it will cause the outer cone of B3 to engage with the stationary ring B4, thereby bringing the sun-wheel A2 to rest. The drive will then pass from E to E1, B1 to A1 and A to D. If the pedal controlling the brake band which encircles the cage F be depressed, the action will automatically, by means of a cam, throw the clutch cone B3 into an intermediate or neutral position, simultaneously bringing the cage F to rest. The drive will still pass through the same gear wheels as before, only in this case the cage F will be stationary and the sun-wheel A2 idly rotating, thus bringing a different gear ratio into action.

The shaft D1 carrying the sun-wheel D should now be moved so as to bring D into engagement with the planet B. In this new position two more speeds can be obtained, one by bringing the clutch cone B3 to rest against the stationary ring B4, and the other by the operation of the brake band on the cage F.

The ordinary type of epicyclic gear, if designed to provide for five different speeds, becomes both clumsy and complicated. The design in Fig. 18 is rigid and compact, and, as shown, gives four speeds forward and one reverse.

It may be objected that the sliding movement of the sun-wheel D eliminates that simplicity of operation which is one of the great

features of the epicyclic gear. This objection is not, however, very serious owing to the fact that the 4th, 3rd and 2nd speeds are all obtained with D in mesh with A as shown in Fig. 18. It is quite easy, except on hills, to start up on 2nd speed, so that D need never be moved except in case of emergency or for the purpose of reversing. It is believed that this design is more or less unique, and it is put forward, not so much as a solution of the problem, but rather as an incentive to stimulate designers to investigate new possibilities hitherto unexplored.

It is not everyone who is familiar with the process by which epicyclic gear-ratios are calculated. Those who rely on cut-and-dried formulæ will not find the gear-ratios of Fig. 18 easy to solve,

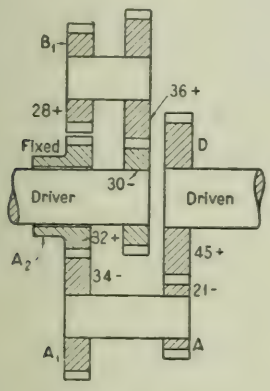


Fig. 19.

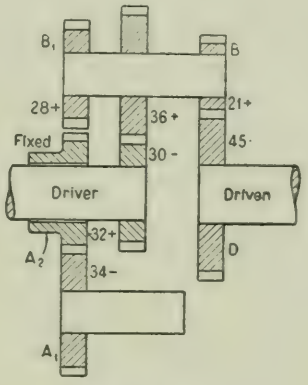


Fig. 20.

Diagram of epicyclic gears.

as this design is not to be found in the usual reference- or text-book. But by the application of first principles, which is better than a bookful of formulæ, the value of the ratios can be quickly ascertained. Thus:—

(1) Required to find the gear-ratio when the sun-wheel A2 is held fast, with the driven gear D in mesh with the planet A, as shown in the diagram Fig. 19.

To facilitate calculation imagine the cage to be fixed and assume the fixed member, A2, to make one revolution in a positive direction. Insert the plus and minus signs as shown in Fig. 19, from whence it will be seen that E is going in a negative and D in a positive direction. Now work out, by simple proportion, the relative speeds of E and D, taking care, in each case, to subtract 1 from the answer so as to eliminate the fictitious plus 1 revolution

which the fixed member A2 was assumed to make. The figures in the diagram indicate the number of teeth in the gears. Then:—

$$\text{Revs. of driver} = -\left(\frac{32}{34} \times \frac{34}{28} \times \frac{36}{30}\right) - 1 = -2{\cdot}37.$$

$$\text{Revs. of driven} = +\left(\frac{32}{34} \times \frac{21}{45}\right) \qquad -1 = -0{\cdot}56.$$

$$\text{Gear ratio} = \frac{\text{driver}}{\text{driven}} = \frac{-2{\cdot}37}{-0{\cdot}56} = +4{\cdot}23.$$

The final answer, being positive, indicates that the gear-ratio is 4·23 to 1 in a forward direction.

(2) Required to find the gear-ratio when the sun-wheel A2 is held fast, with the driven gear D in mesh with the planet B, as shown in the diagram Fig. 20.

Insert plus and minus signs on the assumption that the fixed member A2 makes plus one revolution with the cage stationary. Then:—

$$\text{Revs. of driver} = -\left(\frac{32}{34} \times \frac{34}{28} \times \frac{36}{30}\right) - 1 = -2{\cdot}37.$$

$$\text{Revs. of driven} = -\left(\frac{32}{34} \times \frac{34}{28} \times \frac{21}{45}\right) - 1 = -1{\cdot}53.$$

$$\text{Gear ratio} = \frac{\text{driver}}{\text{driven}} = \frac{-2{\cdot}37}{-1{\cdot}53} = +1{\cdot}55.$$

(3) Two more speeds can be obtained with a stationary cage, one with D in mesh with B, and the other with D in mesh with A. Obviously the former will be a forward speed, and the latter, owing to the introduction of an intermediate gear, is bound to be in the reverse direction, the gear-ratios being as follows:—

$$\text{Forward speed} = +\left(\frac{36}{30} \times \frac{45}{21}\right) = +2{\cdot}57 \text{ (B driving D).}$$

$$\text{Reverse speed} = -\left(\frac{36}{30} \times \frac{34}{28} \times \frac{45}{21}\right) = -3{\cdot}12 \text{ (A driving D).}$$

The three forward speeds which can be obtained with D in constant mesh with B are, therefore, as follows:—2·57, 1·55 and 1·0 (direct), equivalent to 15·5, 26·0 and 40 miles per hour. For the reverse speed and the emergency forward speed, D has to be moved into mesh with A.

In order to calculate any epicyclic gear-ratio, without resorting to formulæ, which are sometimes unobtainable, the following system, common to all types, may be employed:—

(1) Assume the planet arm to remain stationary.

(2) Assume the fixed member to make one revolution.

(3) Ascertain directions by inserting plus and minus signs.

(4) Calculate, from (2), the revolutions of the driver and subtract one.

(5) Calculate, from (2), the revolutions of the driven gear and subtract one.

(6) Divide (4) by (5). The answer gives the gear-ratio.

A simple but interesting example is that of the Ford epicyclic gear. With a dismantled gear-box on the workshop table, the author has asked competent mechanics to explain the working of the gear. Pitying his ignorance, they have cheerfully undertaken the task, but after ten minutes of unnecessary preamble they often scratch their heads, being quite unable to explain why the car should go backwards on the reverse speed. Neglecting formulæ,

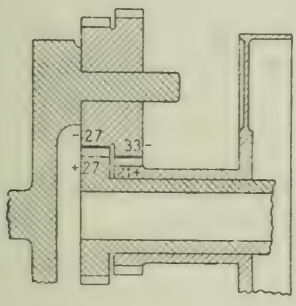


Fig. 21.

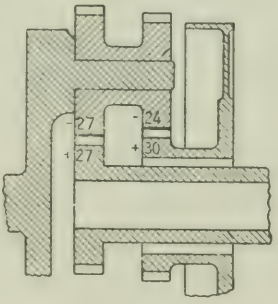


Fig. 22.

Diagram of Ford epicyclic gears.

and working from first principles, the proposition is, however, extremely simple. Fig. 21 shows the train of gears used to obtain the low forward speed, and Fig. 22 shows the train used for the reverse speed.

In both cases insert plus and minus signs, as shown, imagine the planet arm fixed (in this case it is really the driver), and assume the fixed member to make one revolution in a positive direction.

Then, in Fig. 21:—

$$\text{Revs. of driver} = 0 \qquad -1 = -1{\cdot}0.$$

$$\text{Revs. of driven} = +\left(\frac{21}{33} \times \frac{27}{27}\right) - 1 = -0{\cdot}36.$$

$$\text{Gear-ratio} = \frac{-1{\cdot}0}{-0{\cdot}36} = +2{\cdot}76 \text{ forward.}$$

And in Fig. 22:—

$$\text{Revs. of driver} = 0 \qquad -1 = -1{\cdot}0.$$

$$\text{Revs. of driven} = +\left(\frac{30}{24}\times\frac{27}{27}\right) - 1 = +0{\cdot}25.$$

$$\text{Gear-ratio} = \frac{-1{\cdot}0}{+0{\cdot}25} = -4{\cdot}0 \text{ backwards.}$$

From an examination of the above it will be obvious that if both sun gear wheels have an equal number of teeth, then the gear-ratio will be zero; in other words, if the brake-drum were held fast the driven shaft would remain stationary because:—

$$\text{Revs. of driver} = 0 \qquad -1 = -1{\cdot}0.$$

$$\text{Revs. of driven} = +\left(\frac{27}{27}\times\frac{27}{27}\right) - 1 = \quad 0.$$

It is equally important to note that, in the above case, when the fixed sun is smaller than the driven sun, the resulting speed will be forward, but if the fixed sun be the greater (as in Fig. 22), then the driven shaft will rotate backwards.

The ratios of an epicyclic train with an internal gear can also be instantly calculated. Three gear wheels can provide six different combinations, and three different diagrams are necessary in order that the plus and minus signs may be inserted. Proceeding exactly as before, the gear-ratios of Figs. 23 to 25 can be quickly ascertained as follows:—

Fig. 23. Sun fixed. Planet arm driving. Internal driven—

$$\left.\begin{aligned}\text{Revs. of driver} &= \quad 0 \quad -1 = -1{\cdot}0\\ \text{Revs. of driven} &= -\frac{80}{100} - 1 = -1{\cdot}8\end{aligned}\right\} \text{Ratio} = +0{\cdot}56.$$

Fig. 23. Sun fixed. Internal driving. Planet arm driven—

$$\left.\begin{aligned}\text{Revs. of driver} &= -\frac{80}{100} - 1 = -1{\cdot}8\\ \text{Revs. of driven} &= \quad 0 \quad -1 = -1{\cdot}0\end{aligned}\right\} \text{Ratio} = +1{\cdot}8.$$

Fig. 24. Planet arm fixed. Sun driving. Internal driven.

As the planet arm is fixed, epicyclic motion is eliminated. The gear-ratio is therefore:—

$$-\frac{100}{80} = -1{\cdot}25.$$

Fig. 24. Planet arm fixed. Internal driving. Sun driven.

As the planet arm is fixed, epicyclic motion is eliminated. The gear-ratio is therefore:—

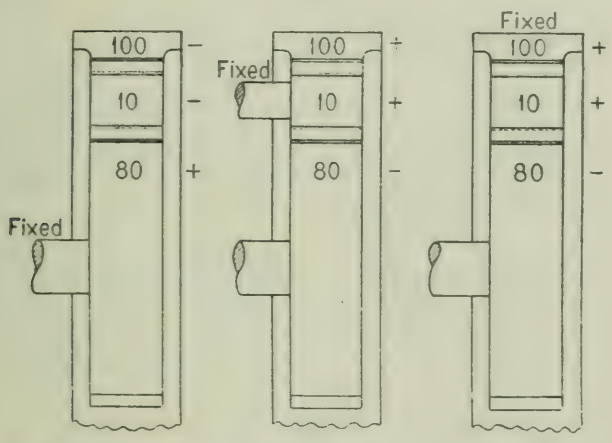
$$-\frac{80}{100} = -0{\cdot}8.$$

Fig. 25. Internal fixed. Planet arm driving. Sun driven.

$$\left.\begin{aligned} \text{Revs. of driver} &= 0 - 1 = -1{\cdot}0 \\ \text{Revs. of driven} &= -\frac{100}{80} - 1 = -2{\cdot}25 \end{aligned}\right\} \text{Ratio} = +0{\cdot}45.$$

Fig. 25. Internal fixed. Sun driving. Planet arm driven—

$$\left.\begin{aligned} \text{Revs. of driver} &= -\frac{100}{80} - 1 = -2{\cdot}25 \\ \text{Revs. of driven} &= 0 - 1 = -1{\cdot}0 \end{aligned}\right\} \text{Ratio} = +2{\cdot}25.$$



Figs. 23, 24 and 25. Diagram of internal epicyclic gears.

Fig. 26 illustrates an epicyclic gear adapted to bolt up to the rear end of the engine casing. The flywheel F is bolted between the two sleeves F1 and F2, carried by two large ball bearings as shown. This type of gear is quite different from that shown in Fig. 18, inasmuch as in this case the planets are carried by a continuously rotating cage, F1, which is keyed to the crankshaft of the engine.

To simplify the drawing, the two planets A and B are shown separated from each other, although their spindles are actually mounted so as to enable the two planets to engage with each other over approximately half their face width, similar to the arrangement of an ordinary spur type differential gear. The driven shaft D1 is controlled by operating mechanism so that the sun-wheel D may be brought into mesh with the planet A alone, or, in a midway

position, with planets A and B simultaneously, or with the planet B alone.

The spindle of the planet A carries another planet A1 meshing with the internal gear wheel A2 controlled by the brake drum A3. The spindle of B carries another planet B1 in constant mesh with the sun-wheel B2 which can be brought to rest by means of the clutch cone B3, which is capable of engaging with the fixed ring B4.

Only seven gear wheels are shown (although additional gears

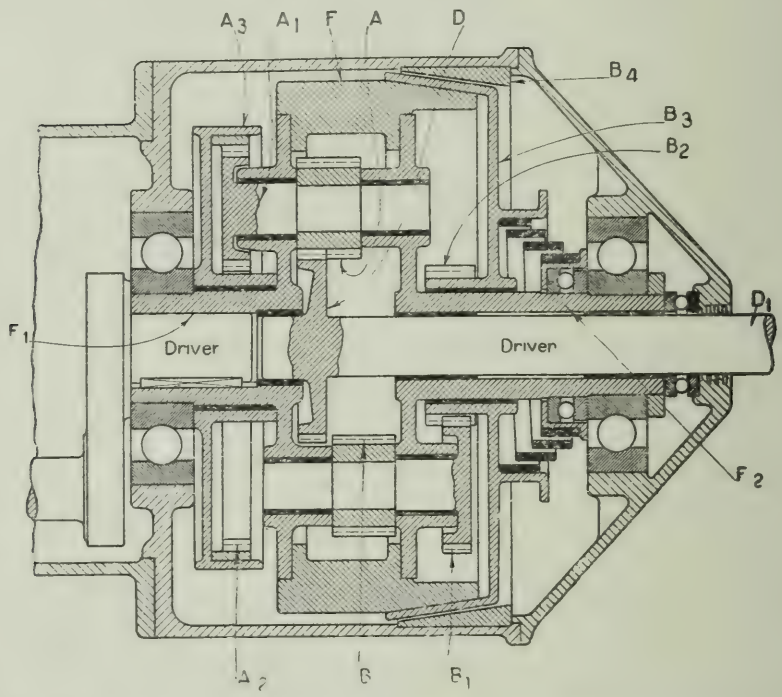


FIG. 26.—Suggested Epicyclic gear-box (unit construction).

should be embodied for the sake of balance and even load distribution) and with these few parts the gear is capable of providing four speeds forward and one reverse. Having already explained Fig. 18 in detail, the action of the present design may be briefly summarised as follows:—

For the direct drive release the clutch pedal so as to allow the clutch cone B3 to engage with the flywheel F, thereby causing the planets and sun-wheels to rotate as one solid mass. Still leaving the sun-wheel D in mesh with the planet A, two further speeds can be obtained, one by engaging the clutch cone B3 against the

stationary ring B4, and another by holding the brake-drum A3 with the clutch cone B3 in neutral.

To obtain the remaining two speeds, slide the sun-wheel D into mesh with the planet B, and hold the clutch cone B3 for the lowest forward speed and the brake-drum A3 for the reverse speed. Whilst correctly proportioned gear ratios are easily obtainable with the design shown in Fig. 18, the ratios in Fig. 26 are more difficult to arrange, it being impossible to obtain a direct drive on the top speed. It may be remarked, however, that a direct

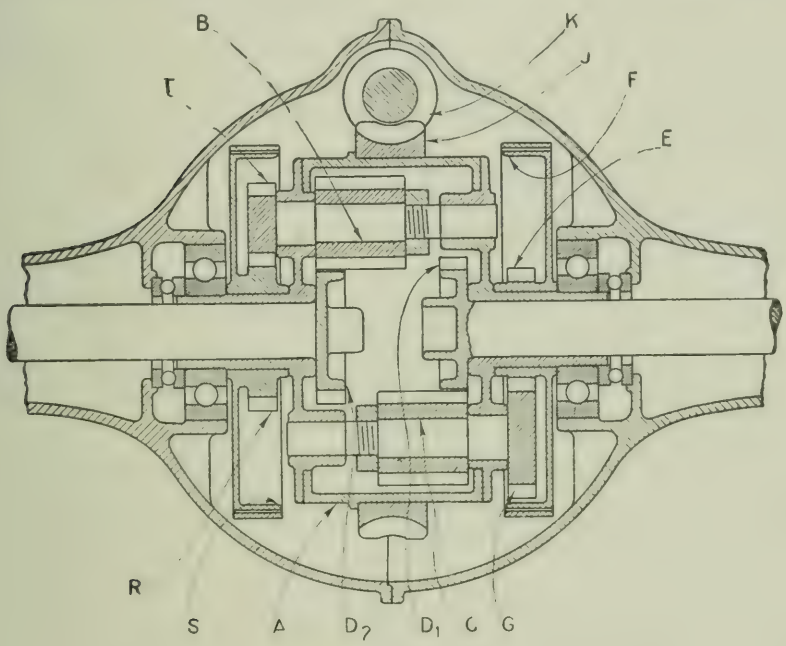


FIG. 27.—Suggested Epicyclic back-axle.

drive on the third speed with an indirect top has been successfully employed by certain automobile manufacturers.

BACK-AXLE DESIGN.

Fig. 27 shows an epicyclic gear designed to form part of a back-axle spur type differential. Apart from the ordinary differential gears, the extra parts required are two suns and two planets (these latter may be added to as desired), and with these few parts the gear is capable of providing four speeds forward and one reverse. The top forward speed, geared up, is, however, too high a ratio for ordinary road use.

The worm K drives the worm wheel J fixed to the differential casing. The spindle of the differential planet B is extended to carry the planet T meshing with the sun-wheel R which can be brought to rest by means of the brake-drum S. In the same way the spindle of the differential planet C carries the planet G meshing with E controlled by the brake-drum F. The back-axle cross shafts are of the floating type, and are so arranged that the sun-wheels D1 and D2 can be moved sideways by means of a change-speed lever and the usual selector mechanism. The brake-drums S and F are controlled by means of two foot pedals.

The position of the gears in Fig. 27 shows the direct drive in operation, the differential gear functioning as such in the usual manner, the drums S and F revolving at the same speed as the differential cage. When D1 is moved to the extreme left, so as to engage with the planet B, two more speeds are available by locking the brake-drums S and F respectively. When D2 is moved to the extreme right, so as to engage with the planet C, two more speeds are available by once again locking S and F respectively, thus providing a total range of four speeds forward and one reverse. On the indirect speeds the gears B, C, D1 and D2 cease to function as a differential.

Many motor lorries have been tested under the most strenuous conditions after their differential gears have been removed. It has been stated that "these gave surprisingly good results, and instead of decreasing the tyre mileage, as would be expected, the mileage appeared to increase. This could only be attributed to the decreased slip of the wheels on the granite block pavements and in the mud and holes near the front. These trucks would pull over almost impossible places, and were the best evidence of the need of limited action" (Lessons of the War in Truck Design, by W. O. Thomas, *The Automobile Engineer*, Oct., 1917).

One of the earliest examples of a live axle designed to eliminate the bugbear of unsprung weight was the De Dion Bouton motor car, and the same design is still adhered to in the 1921 De Dion Bouton commercial vehicle. The design unfortunately necessitates the use of four universal joints, two on each side of the back-axle casing, without taking the propellor shaft into account. Figs. 28 and 29 illustrate a method of eliminating unsprung weight in which only one universal joint is required in place of the usual four.

In Fig. 29 the bevel casing D2 is suspended by means of a pivoted link B from the rear cross-member A of the chassis. The axle sleeve S2 terminates in a spherical ball E which allows the sleeve to have a universal movement with respect to the case D2. The other half of the bevel casing, D1, carries the axle sleeve S1, the outer extremities of the two sleeves S1 and S2 carrying the rear road wheels in the usual manner.

It will be seen that this design not only insulates the transmission gear from all road shocks, and reduces the unsprung weight of the vehicle, but in addition it provides each road

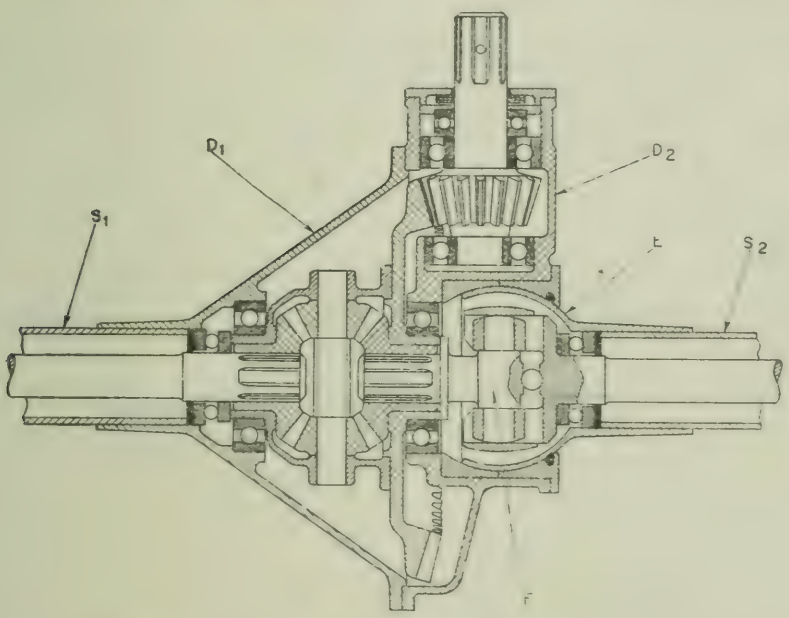


FIG. 28.—Suggested Jointed back-axle, end elevation.

wheel with independent motion when rising or falling over obstructions through the universal joint F which is concentric with the ball joint E and which allows the shafting to adapt itself to the angularity of the axle sleeves caused by the irregular movement of the road wheels.

The disadvantages of the ordinary back-axle suspension are illustrated by Fig. 30, where the dotted line shows the movement of the ordinary live axle when the left road wheel is passing over a four-inch obstacle.

The exact movement of the body cannot be determined on paper,

but obviously the ideal arrangement is for the body to remain absolutely horizontal, so that the passengers may not be thrown from side to side. In Fig. 30 the body is purposely drawn stationary in order to show the relative movements of the two axles. It will be noted that the centre point of the ordinary axle has been displaced sideways as indicated by the letter E. The springs have also moved sideways a distance A and B (or alternatively the body must be thrown sideways) whilst with the jointed axle the road springs remain practically vertical. There can be little doubt but that the independent motion of the road wheels, obtained by the use of a jointed axle, will provide greater comfort

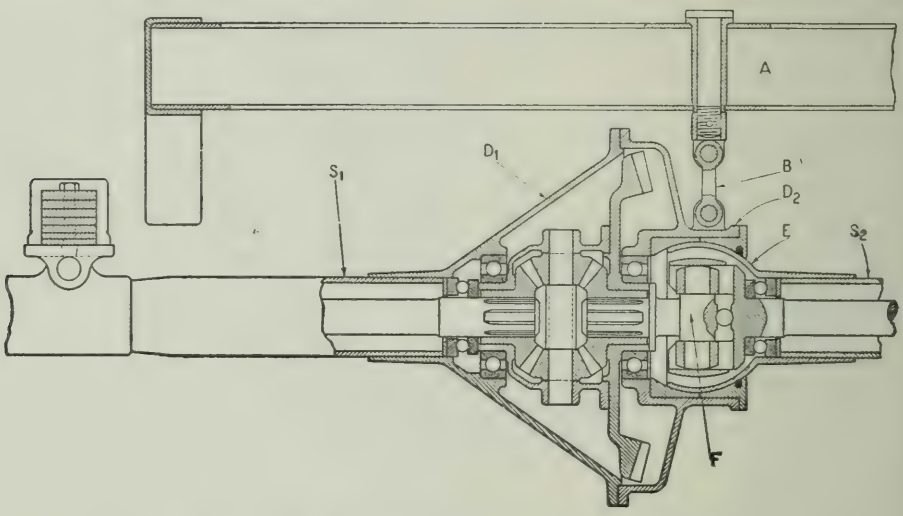


FIG. 29.—Jointed back-axle, plan view.

for the occupants of the car and the reduction of unsprung weight will considerably prolong the life of the tyres. In the De Dion Bouton design the only advantage appears to be that of the reduction of unsprung weight, as the movement of the road wheels resembles that of an ordinary axle.

WORM GEAR DESIGN.

Although the advantages and characteristic features of worm gear are now well understood, the author has never yet come across any text-book which explains the real nature of the tooth contact between the worm and the wheel. Before discussing this

subject in detail, the ground may be cleared by a few introductory remarks.

Circular pitch is measured circumferentially along the pitch circle of the worm wheel.

Linear pitch is measured in a straight line parallel to the worm axis.

Lead angle is the angle between a worm thread and a line drawn at right angles to the worm axis.

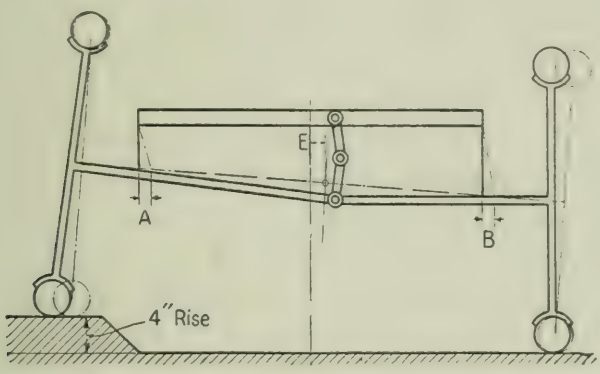


Fig. 30.—Diagram of suggested back axle movement.

Lead is the distance travelled by any point on the pitch circumference of the wheel for one revolution of the worm.

$$\text{Lead} = \text{Linear pitch} \times \text{Number of threads.}$$

$$\text{Tangent lead angle} = \frac{\text{Lead}}{\pi \times \text{Pitch diameter of worm}}.$$

$$\text{Pitch dia. of wheel} = \frac{\text{Number of teeth} \times \text{Circular pitch}}{\pi}.$$

In ordinary worm gear the axes of the worm and the wheel are at right angles, and under these conditions the circular pitch of the wheel is equal to the linear pitch of the worm.

The gear-ratio of worm gear is found by dividing the number of teeth in the wheel by the number of threads in the worm, yet, unlike spur or bevel gearing, the ratio is independent of the pitch diameters of the worm and the wheel. One concrete example will make this perfectly plain. Let a worm of 1·585 in. pitch diameter mesh with a worm wheel of 15·915 in. pitch diameter (100 teeth, $\frac{1}{2}$ in. circular pitch) at 8·75 in. centre distance. Then the gear-ratio may be varied by varying the number of threads in the worm

without altering the pitch diameters or the centre distance. Some of the possible variations are shown in Table I.:—

TABLE I.

Threads in worm.	Gear-ratio.	Lead.	Worm angle.
		in.	deg. min.
1	100·0 to 1	0·5	5 44
2	50·0 to 1	1·0	11 21
4	25·0 to 1	2·0	21 53
6	16·7 to 1	3·0	31 04
8	12·5 to 1	4·0	38 47
10	10·0 to 1	5·0	45 07

In the above Table the pitch diameters and centre distance remain unaltered. It will be seen that a large gear-ratio (100 to 1) necessitates a small lead angle (5 deg. 44 min.) and *vice versâ*. The most efficient lead angle lies between 40 and 45 degrees.

Some engineers calculate worm gear on the hit or miss principle. This is quite a mistake. The proposition is really very simple, if attacked in a systematic manner. Thus, required to design a worm gear with a ratio of 10 to 1, the size of the worm wheel being restricted to 16 in. pitch diameter on account of ground clearance.

$$\text{Lead} = \frac{\text{Pitch diameter of wheel} \times \pi}{\text{Gear ratio}}$$

$$= \frac{16 \times \pi}{10} = 5 \text{ in. approx.}$$

In other words, the lead *must* be five inches, no matter what linear pitch, lead angle or pitch diameter of worm be chosen. The next most important factor is the lead angle. To obtain high efficiency select a high angle, say 40 degrees. Then:—

$$\text{Pitch diameter of worm} = \frac{\text{Lead}}{\text{Tangent lead angle} \times \pi}$$

$$= \frac{5{\cdot}0}{0{\cdot}84 \times \pi} = 1{\cdot}9 \text{ in.}$$

Hence a lead angle of 40 degrees cannot be used, because 1·9 in. is too small a diameter for such a large gear. Now ascertain the smallest diameter of worm shaft to withstand torque and

bending, and select a pitch diameter to correspond. say 2·5 in. Then:—

$$\text{Tangent lead angle} = \frac{\text{Lead}}{\text{Pitch diameter of worm} \times \pi}$$

$$= \frac{5{\cdot}0}{2{\cdot}5 \times \pi} = 0{\cdot}64$$

And the tangent of 0·64 is approximately 33 degrees. Thus, under the conditions given, the lead angle cannot exceed 33 degrees, and the efficiency of the gear can be calculated accordingly.

The manner in which the lead angle varies according to the pitch diameter, with a given gear ratio and centre distance, is illustrated in Table II.:—

TABLE II.

Fixed gear-ratio, 10 to 1.		Fixed centre distance, 8·75 in.	
Wheel pitch diameter.	Worm pitch diameter.	Lead.	Lead angle.
			deg. min.
16·0	1·5	5·03	46 40
15·75	1·75	4·95	42 0
15·5	2·0	4·87	37 49
15·25	2·25	4·80	34 13
15·0	2·5	4·72	30 57
14·75	2·75	4·64	28 12
14·5	3·0	4·55	25 47

It need hardly be said that no gear should be selected without first consulting a maker's hob list in order that standard tools may be brought in.

When the shape of, or forces acting on, worm gear teeth have been theoretically considered, it has been usual to take a linear section through the centre of the worm at right angles to the wheel axis. On this axial plane it has been customary to make the shape of the gear teeth to correspond with those of a rack engaging with an ordinary spur, thus providing an involute tooth profile on this particular section. But what is taking place on adjacent parallel planes?

The shape of the teeth of a rack-shaped worm, on one of these corresponding parallel sections, is peculiarly curved, and the wheel teeth, on these planes, should be conjugate to suit, but this is impossible owing to interference. On these other planes the worm wheel teeth are concave, in order that they may wrap round the threads of the worm, so that corresponding sections, on either side of the axial plane, in no way resemble the involute shape of tooth. Hence it follows that the transmission of true uniform velocity, which is a characteristic of the involute gear, only applies to a section on the axial plane where the worm section is that of a rack. The constructive problem of the nature of the tooth contact

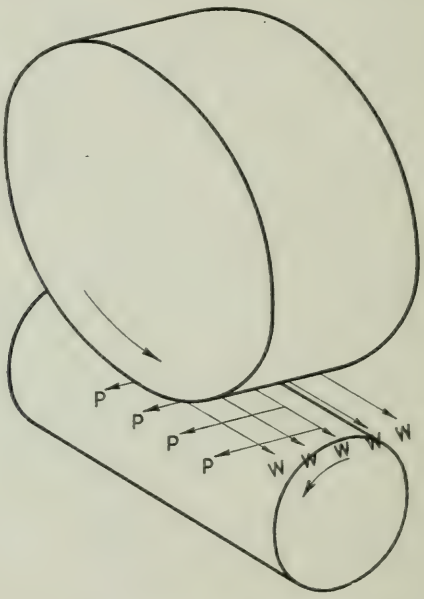


FIG. 31.—Worm and wheel pitch cylinders.

on these other planes has, it is believed, never before been properly investigated. It can, however, be successfully solved by the application of the principle of the triangle of forces which must hold good at every point where tooth contact takes place, and the application should be applied to every single phase of tooth contact, and not merely limited to one axial plane as has been the practice heretofore.

The pitch surfaces of the worm and the wheel are both cylinders, and can therefore only touch at one point. The torque on the worm can be represented by a force P, acting anywhere longitudinally on the pitch surface, and in a tangential direction, as shown in

Fig. 31. In the same way the torque on the wheel can be represented by a force W acting in the same manner.

In order to obtain the transmission of uniform velocity between the worm and the wheel, it is necessary that the resultant force R, normal to the tooth surfaces at their point of contact, shall be capable of being resolved into two components, P and W, both of which must always be tangential to their respective pitch cylinders and at right angles to their axes, these three forces all lying, of course, on the same plane. For if the resultant R can be resolved into two components either of which do not touch their respective pitch cylinders; then the instantaneous torques on the two gears will not correspond to the velocity ratio. But the velocity ratio is determined by the threads of the worm and the teeth of the wheel, which is obviously a constant quantity; hence, when the respective torques of the worm and the wheel

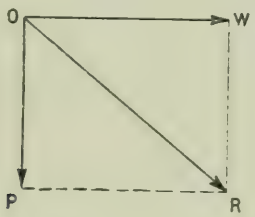


Fig. 32.—Resolution of forces.

do not correspond to the gear ratio, then the transmission of true uniform velocity will be impaired, the slight accelerations and retardations having to be absorbed by the elasticity of the gear teeth, thus lowering the efficiency of the gear and causing undue wear.

To secure uniform velocity transmission between the worm and the wheel, the constructive problem of building up the correct tooth contours may, therefore, be stated as follows:—

(1) Select a series of resultant forces capable of being resolved into components which must always be tangential to their respective pitch cylinders.

(2) From a series of such resultants build up a tooth contour, working on the well known fact that all resultants must always be normal to the tooth surfaces at their respective points of contact.

In Fig. 32, P and W represent the tangential forces acting on the pitch cylinder of the worm and the wheel respectively about

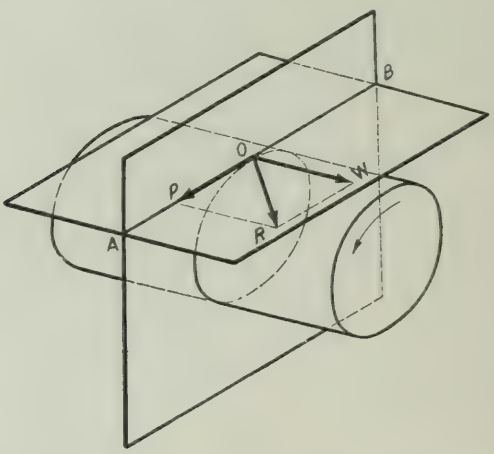


Fig. 33.—Forces acting in a horizontal plane.

the point O, and R is the resultant of these, acting in the plane containing P and W. The relation between P and W, being

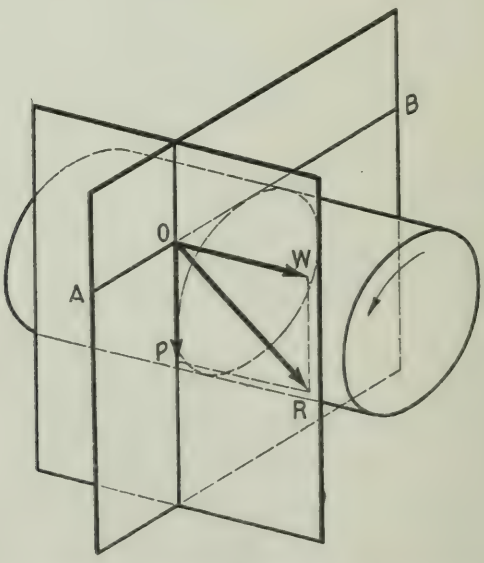


Fig. 34.—Forces acting in a vertical plane (near side).

determined by the ratio of the gears, is bound to remain constant, so that the angle which R makes to the axis of the worm will also remain constant.

Determination of Points of Contact on Central Plane.

Fig. 33 is a simple application of these forces, the point O being the contact point of the two cylinders lying on a "central plane" at right angles to the worm axis and containing the wheel axis. In this particular case the plane PWR is tangential to both cylinders.

Fig. 34 represents a view of the same plane when moved through 90 degrees round the worm pitch cylinder, the point O still being on the pitch cylinder of the wheel and also on the central plane so that W may still remain tangential to the wheel.

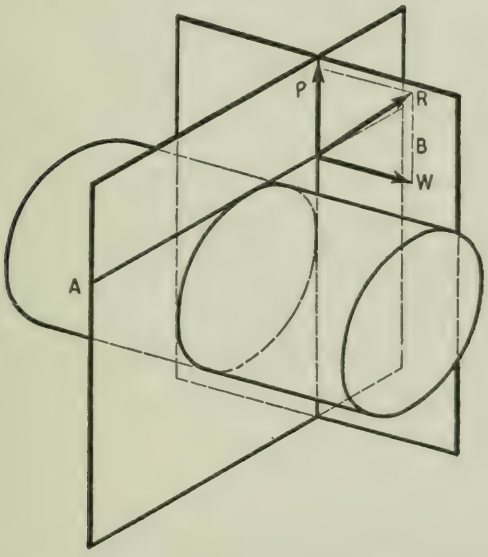


Fig. 35.—Forces acting in a vertical plane (far side).

Fig. 35 represents a similar view when the plane is moved round the pitch cylinder in the opposite direction.

In all these three cases it will be observed that W is tangential to the wheel cylinder, and that both P and R are, in every case, tangential to the worm cylinder. It will be further noted that the point O in each case lies on the line of intersection of the wheel cylinder and the central plane. When these conditions are fulfilled, and the angle β remains constant, three points of contact are obtained.

It is now necessary to ascertain all the possible intermediate positions in which the fundamental conditions still obtain. Let AB be a line on the central plane tangential to the worm cylinder. Then the requisite conditions are fulfilled if the centre O of the forces always lies on AB, and the plane is always tangential to the worm cylinder and in such a position that the force P, acting at right angles to the axis of the worm, is always tangential to its cylinder, the same relation holding good between W and its wheel cylinder.

Fig. 36 illustrates such an intermediate example, and Fig. 37 shows a series of resultants forming a "zone of contact" which

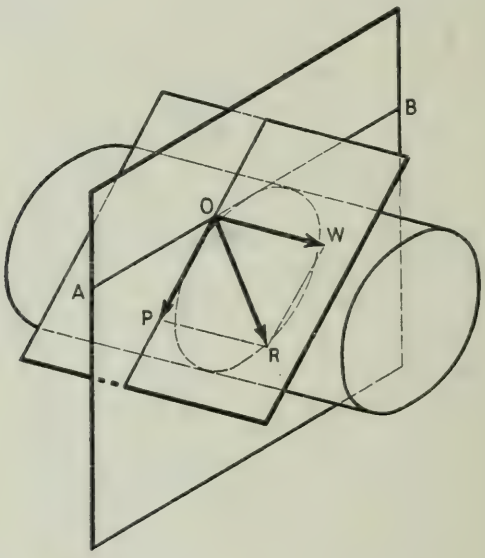


FIG. 36.—Forces acting in an inclined plane.

contains all the possible points of contact and provides for the correct relation between P and W. As the resultants R are tangential to the worm cylinder, it follows that they only touch at one point, hence beyond this point they project outwards and provide for points of contact outside or beyond the periphery of the worm cylinder. These external points of contact necessitate the building up of a tooth, the contour of which is controlled by the zone of contact, the latter being normal to the tooth surfaces at their respective points of contact. From the above it will be obvious that the zone of contact does not lie on the periphery of the worm cylinder, but stands proud in the form of a twisted

ribbon. It is, in fact, a series of innumerable straight lines which, combined together, provide a curved surface, all these lines being tangential to the worm cylinder and lying at a constant angle β to the worm axis in addition to cutting the line AB on the central plane.

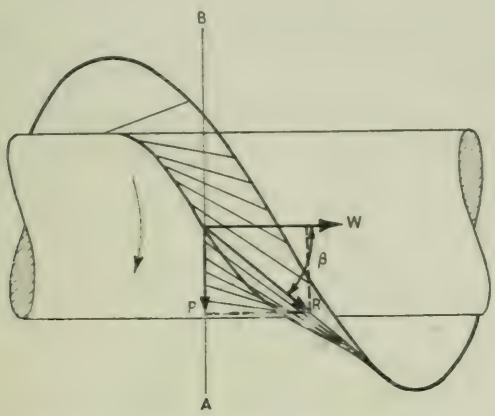


FIG. 37.—Series of resultants.

DETERMINATION OF OTHER POINTS OF CONTACT ON PITCH CYLINDER.

In Fig. 38 it will be obvious that one point of contact is on the central plane at the point of contact C between the two cylinders. Move the contact point through an angle θ equal to 90 degrees. Then the point of contact will have moved in the end view to the point D1.

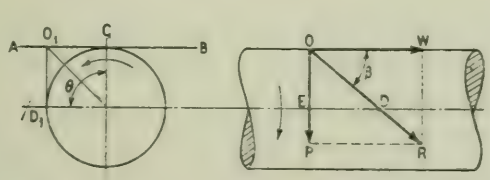


FIG. 38.—Determination of point of contact on pitch cylinder when $\theta = 90$ degrees.

In the same view draw AB tangential to the worm cylinder at the point C, and draw D1 O1 at right angles to it. Project a side view looking in the direction AB and let O be the intersection of the forces P and W, as before. The point O lies on the line AB which is contained by the central plane. Draw the resultant

R and project D1 until it cuts OP at E and intersects the resultant R at D. Then D, being on the resultant R, and also on the pitch cylinder, is the position of the point of contact after it has been moved through 90 degrees, and the length ED is its distance from the central plane.

$$\text{Then ED} = \text{OE} \cot \beta$$

$$\text{And OE} = \text{O1 D1} = \text{Radius} \times \tan \frac{\theta}{2}$$

$$\text{Therefore ED} = \text{Radius} \times \tan \frac{\theta}{2} \cot \beta.$$

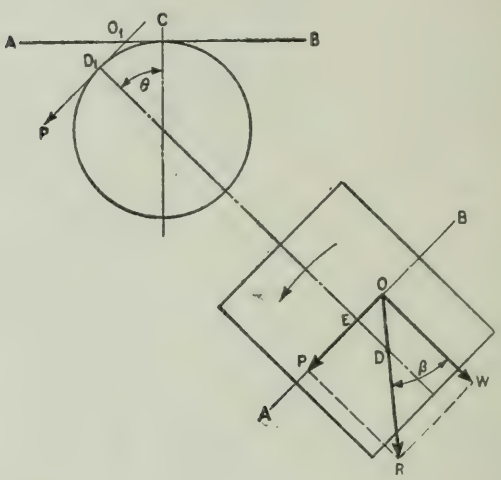


Fig. 39.—Determination of point of contact on pitch cylinder when θ is less than 90 degrees.

In Fig. 39 the angle θ is less than 90 degrees. Draw AB as before and from D1 draw D1 O1 tangential to the worm cylinder at the point D1 and cutting AB at O1. Project a view on a plane parallel to D1 O1 and project O1 to cut AB at the point O. From O draw the forces P, W and R as before. Project D1 until it cuts OP at E and intersects the resultant R at D. Then D is the point of contact and ED its distance from the central plane.

Then $\text{ED} = \text{Radius} \times \tan \frac{\theta}{2} \cot \beta$ as before.

Contact Points External to Worm Cylinder.

As R is the single force which produces the same effect as the combined action of the two forces P and W, and as R may be

resolved into any two forces such as P1 and W1 or P2 and W2, it follows that P and W may act at any point (in the same plane) along R as shown by the diagram in Fig. 40. Hence the contact point between the worm and wheel can lie anywhere along the resultant R, and the effective result will be the same as if the components originated from O on the line AB. It is therefore possible to have contact outside the worm cylinder at any point along R.

Although the contact point lies along a certain resultant, the position of this point will vary according to the rotation of the worm. The zone of contact will always remain stationary, owing

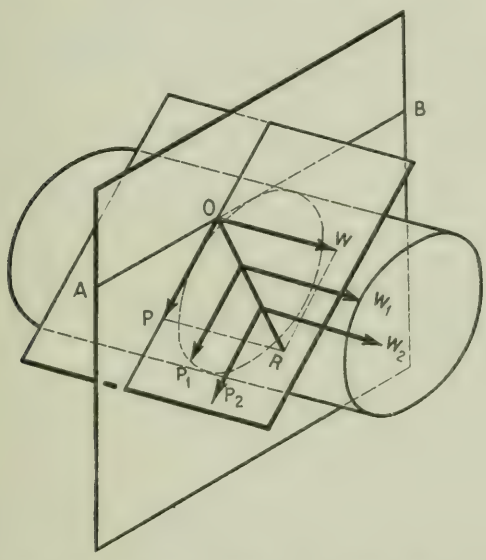


FIG. 40.—Inclined plane with a series of components.

to the fact that the positions of the resultants are definitely determined by their intersections with the stationary line AB, the latter being independent of any rotary movement of the worm gear. The actual points of contact will be the intersection of the worm tooth and the zone. It is therefore necessary to ascertain, in accordance with the rotation of the worm, the location of the points of contact along the resultants R on any outside portion of the worm thread.

Let D1 (Fig. 41) be any such point located at any radius MD1 from the axis of the worm and making any angle θ with a plane MN containing the worm axis and at right angles to the wheel axis.

From D1 draw D1F tangential to the worm pitch cylinder in a direction opposite to the supposed rotation of the worm, so that FD1 represents the force P. This line intersects AB at O1. Project a view on a plane parallel to FD1 and complete the construction as in the case of Fig. 39. From D1 draw D1G parallel to AB meeting MN at G. Draw GH parallel to D1O1, intersecting the line AB at H. Then, as before, D is the new point of contact and ED its distance from the central plane.

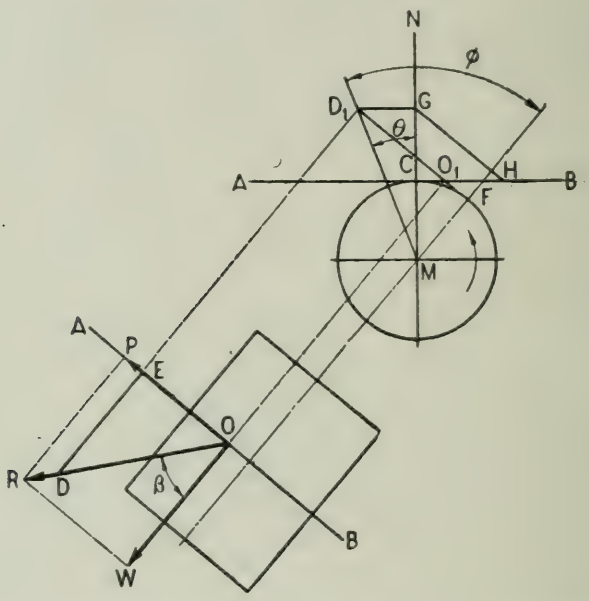


FIG. 41.—Determination of point of contact outside pitch cylinder when θ is less than 90 degrees.

Then ED $= OE \cot \beta$

And EO $= O1D1 = GH$

Let the angle FMD1 $= \phi$

Then the angle FMG $= \phi - \theta$

But the angle FMG $=$ the angle GHC

Therefore the angle GHC $= \phi - \theta$

Now HG $= \dfrac{GC}{\sin(\phi - \theta)}$

But GC $= GM - MC$

And GM $= MD1 \cos \theta$

Therefore GC $= MD1 \cos \theta - MC$

Hence HG $= \dfrac{MD1 \cos \theta - MC}{\sin(\phi - \theta)}$

$$\text{As HG} = \text{OE}$$

$$\text{Therefore ED} = \frac{\text{MD1} \cos\theta - \text{MC}}{\sin(\phi - \theta)} \times \cot\beta$$

$$\text{So that ED} = \frac{\text{Radius of contact point} \times \cos\theta - \text{Radius of pitch cylinder}}{\sin(\phi - \theta)} \times \cot\beta$$

Note that when the point of contact is on the pitch cylinder

$$\text{ED} = \frac{\text{Radius of pitch cylinder} \times (\cos\theta - 1)}{\sin(\text{O} - \theta)} \times \cot\beta$$

$$= \frac{\text{Radius of pitch cylinder} \times (\cos\theta - 1)}{\sin - \theta} \times \cot\beta$$

$$= \text{Radius of pitch cylinder} \times \tan\frac{\theta}{2} \cot\beta$$

as already shown in connection with Figs. 38 and 39.

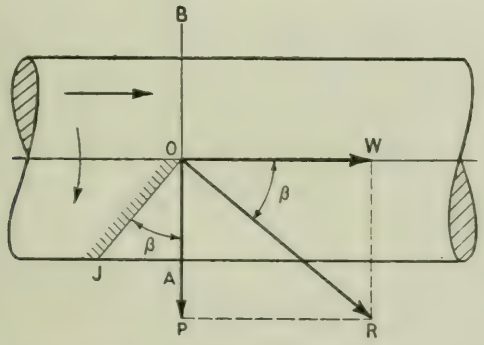


FIG. 42.—The straight line OJ (lying on the thread contour of the F. J. worm) ensures uniform velocity transmission.

STRAIGHT LINE GENERATION.

Let OJ be the surface of the worm thread moving in the direction of the arrow as shown in Fig. 42, and let P, W and R represent the forces acting on it. If the worm thread OJ be moved at a uniform rate longitudinally, parallel to its axis, in the direction of the arrow as shown, then it would drive the worm wheel in exactly the same manner as if it were caused to rotate at a uniform rate, because all linear sections are identical. If, therefore, the worm rotate uniformly in the direction P, then the point O will move uniformly in the direction W; hence the point of contact between the worm and the wheel must move uniformly along the resultant R.

Now the line OR touches the worm cylinder only at the point O. Any other point of contact between the worm and the wheel along the resultant R will therefore be on a portion of the worm thread projecting above the worm cylinder. But R is in the horizontal plane contained by W and P, and as any point of contact along R is an intersection of the worm thread OJ and the resultant OR, it follows that the worm thread must be of such a contour that it lies along a straight line OJ at right angles to R and therefore inclined at an angle β to the plane AB. Clearly the angle β is equal to the lead angle of the worm.

Fig. 43 shows the positions of the straight lines EF and EG (corresponding to OJ) on the threads of a left-handed worm. It is interesting to note that, with this new design of worm gear, there is no thread below the pitch line of the worm, and consequently throat diameters and pitch diameters of worm wheels are identical.

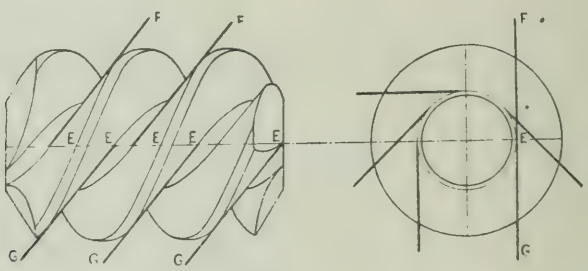


FIG. 43.—Plan of F. J. worm indicating system of straight line generation.

ORDINARY TYPE WORM GEAR.

The threads of the ordinary parallel type of worm gear are made straight sided on a linear section through the centre of the worm at right angles to the wheel axis.

It is well known that the tan. circular pressure angle at any radius = tan. linear pressure angle × cotan. lead angle at the same radius.

The circular pressure angle is, of course, shown in the end view of the worm, tangential to the tooth contour at its point of contact. In order to draw a diagram of the zone of contact of the ordinary type worm gear it is necessary to ascertain the distances of various points of contact at various radii from the central plane. These may be ascertained by drawing a diagram similar to that shown in

Fig. 41. Thus, in Fig. 44 let XEY be the end view of a portion of the worm thread from which it is required to ascertain the distance of the point of contact E (on any radius ME situated at any angle θ) from the central plane. Draw SET at the circular pressure angle ϕ to the radius ME. From E draw EK perpendicular to SET. Then EK represents the normal to the tooth at its point of contact.

Now EK cuts the line AB at K. From E draw EG parallel to AB and GA parallel to EK. Then the angle GAB = the angle EKB = $\phi - \theta$. As before:—

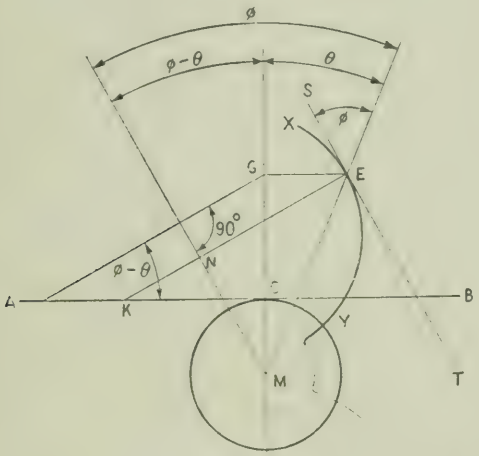


Fig. 44.—Determination of points of contact outside pitch cylinder of ordinary type worm gear.

Distance x = EK × cotan. lead angle on radius MN.

But EK $= GA = \dfrac{GC}{\sin \phi - \theta}$

And GC $= ME \cos \theta - MC$

Therefore $x = \dfrac{ME \cos \theta - MC}{\sin (\phi - \theta)}$ × cotan. lead angle on radius MN.

From this formula the graphs shown in Fig. 45 have been plotted, and from the formulæ already given in connection with Figs. 38 to 41 the graphs shown in Fig. 46 have been plotted. These show the lines of contact along the zone for the F. J. worm gear.

Comparison of the Two Types of Worm Gear.

Figs. 45 and 46 should be carefully compared. In Fig. 45, which shows the lines of contact for the ordinary type of worm

gear, the zone of contact is actually interrupted near the central plane, and almost the whole of the contact takes place on the approaching side of the worm wheel, which means that if the worm be placed in engagement with the wheel, contact between their teeth takes place on the extremities of the worm, when, in many cases, the centre teeth have practically no contact. There is also a duality of the lines of contact, which, during the revo-

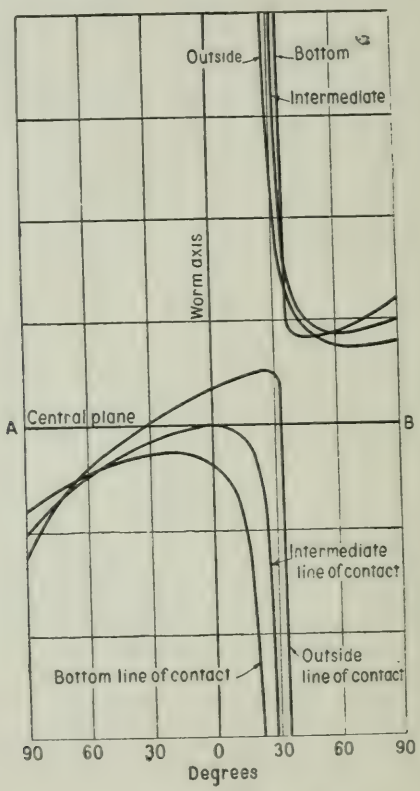


FIG. 45.—Development of the lines of contact between worm and wheel with the ordinary type of worm gear.

lution of the gear, gradually travel towards each other, meeting just beyond the central plane where contact entirely ceases.

In Fig. 46, which shows the zone of contact for the F. J. worm gear, it will be noticed that the lines of contact are regular and uniform, with no duality and no objectionable interruption.

Fig. 45 demonstrates the very imperfect form of tooth contact obtained from the ordinary type of worm gear, which is straight sided on the linear section. In the year 1913 both Mr. F. J. Bostock and the author knew of these defects. The solution and

the remedy were, however, discovered by Mr. Bostock, and it was entirely due to his efforts, in conjunction with the facilities afforded by Messrs. David Brown & Sons, that this new form of F. J. worm gear has been evolved. The author had the privilege, however, of watching the mathematical development of the present invention (which entailed great labour and occupied many months), a brief outline of which has now been made public for the first time.

The difference between the ordinary and the F. J. type of worm gear can be best illustrated—to those who are not keenly interested in mathematics—by taking a concrete example of each case as

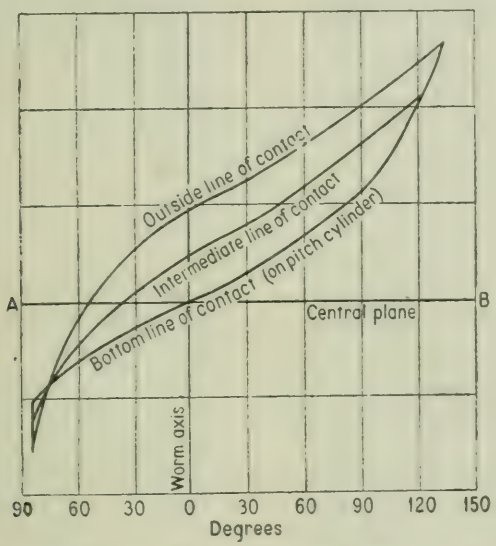


FIG. 46.—Development of the lines of contact between worm and wheel with the F. J. type of worm gear.

shown in Figs. 47 to 50. These illustrations show two plan views of each type of worm, together with a sectional view of each worm in mesh with its worm wheel just above. Figs. 47 and 48 show the nature of the contact between an ordinary type of worm and wheel. Fig. 47 shows the worm in such a position that the drive is taken by the two threads A and B, whilst Fig. 48 shows a different position, the worm having been rotated through an angle of 45 degrees, thus causing the thread A to carry the entire load. The ribbon-like area, outlined on the plan view and stretching across the threads of the worm, marks the "zone of contact" between the gears when the worm is rotated in the direction of the arrow shown in the end view. The intersections

of the worm threads and the zone give the lines of contact between the worm and wheel teeth, and at no point outside this zone is there any contact between the worm and the wheel. In Fig. 47 the actual contact between the worm thread A and the wheel is marked by the line 1—1A, and that between the worm thread B and the wheel by the line 2—2A. After the worm has been rotated to an angle of 45 degrees as shown by Fig. 48, there is only one line of contact between the worm and the wheel as

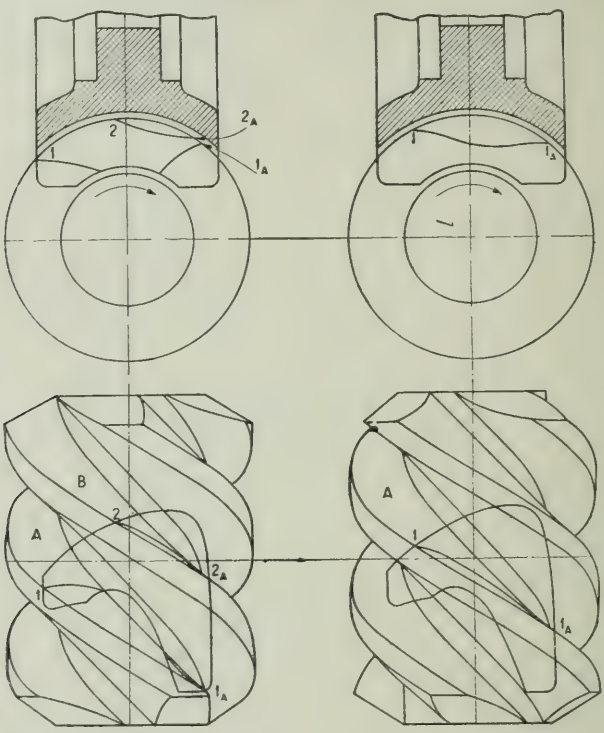


Fig. 47.—Zone of contact between ordinary worm and wheel.

Fig. 48.—New position of worm threads after a rotation of 45 degrees.

indicated by the line marked 1—1A. It will be noted that the lines of contact in both end views in Figs. 47 and 48 are unsymmetrical, and this irregular contact is detrimental to the running of the gears.

Figs. 49 and 50 show the zone of contact with David Brown & Sons' new type of F. J. worm gear. The plan view in Fig. 49 shows the worm in such a position that the drive is taken by the three threads A, B and C, whilst Fig. 50 shows the two threads A and B in simultaneous contact with the worm wheel. The

actual lines of contact between the worm and the wheel in Fig. 49 are marked 1—1A, 2—2A, and 3—3A, whilst in Fig. 50, after the worm has been rotated through an angle of 45 degrees, the new lines of contact are shown by the line 1—1A and 2—2A. With this design of worm gear the regularity of the lines of contact as shown in the end view is in marked contrast to that of the contact lines shown in Figs. 47 and 48.

The worms illustrated in Figs. 47 to 50 are as like as possible,

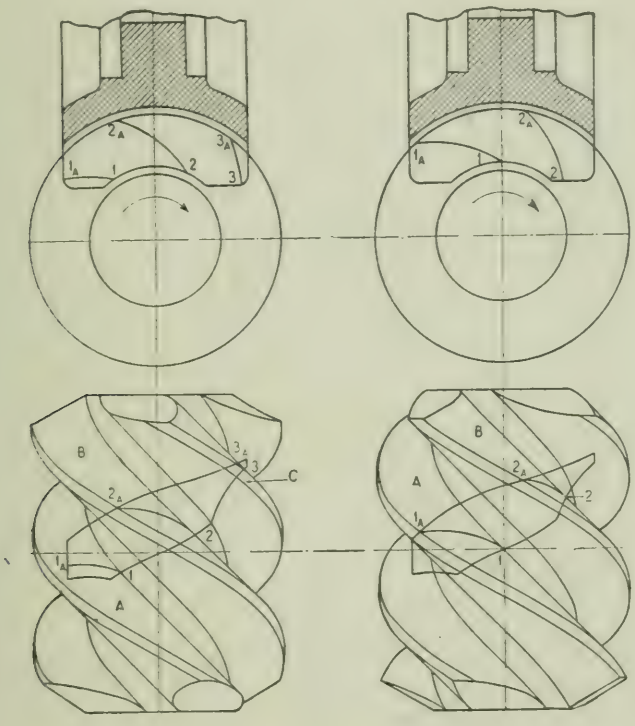


FIG. 49. Zone of contact between F. J. worm and wheel.

FIG. 50. New position of worm threads after a rotation of 45 degrees.

both types having four threads, the only real difference between them being in the shape of the tooth contour. In both cases whenever the worm is revolved through an angle of 90 degrees, the line of contact 1—1A will sweep across the zone of contact until it takes up the position of 2—2A, because when a four-threaded worm moves a quarter of a revolution (or 90 degrees) the thread A will naturally take up the position of the thread B. In the F. J. system it will be seen that the point 1 at the root

of the worm (end view) falls into the position 2 after a quarter of a revolution (see Fig. 50). Thus, the actual contact travels in the same direction as the worm, and this new action between the teeth of the gears reduces the ordinary rubbing velocity to a minimum, which decreases frictional losses and considerably increases the efficiency of the drive.

With the ordinary parallel type of worm gear, however, the point of contact 1A actually moves in the opposite direction to the rotation of the worm to the point 2A (see Fig. 47), thereby increasing the rubbing velocity. One of the peculiar characteristics of the ordinary type is that the extremities of the lines of contact, see plan view of worm in Fig. 47, travel from the position 1 and 1A towards each other until they meet and vanish just beyond the point 2 and 2A.

The end view of the F. J. system (Fig. 49) shows the point A always moving from left to right, in the same direction as the rotation of the worm, whilst with the ordinary type (Fig. 47) the point A moves very slightly from right to left, actually against the rotation of the worm. The high rubbing velocity which occurs on the points 1A, 2A (Fig. 47), together with the converging lines of contact which induce a concentrated load, all tend to produce the worst possible conditions on the leaving side of the worm wheel. This explains the well known fact that when a worm gear of the ordinary type is overloaded, or is imperfectly lubricated, pitting first takes place on the leaving side of the wheel, and on this side only, whilst those portions of the teeth on the entering side remain unmarked. It is believed that this is the first time that this phenomenon of uneven wear on the teeth of the worm wheel of the ordinary parallel system has been fully explained.

The author thanks Messrs. David Brown & Sons for their courtesy in preparing the worm gear diagrams and for providing the lantern slides, and Mr. Bostock for his kind assistance in the preparation of the latter portion of this paper

THE DISCUSSION.

Dr. F. W. Lanchester, in opening the discussion, said: The paper covers superficially a considerable amount of ground and includes a rather miscellaneous collection of problems. I propose in my remarks to deal with a few of the many points that appear to invite criticism.

At the outset the author tilts at the plate or disk clutch, which he refers to as "theoretically incorrect," and expresses a preference for the cone type of clutch. I can find nothing in his arguments of a substantial nature to support his contention, and, in view of the fact that of late years the plate clutch has been steadily gaining in favour at the expense of the cone clutch, it is impossible to accept the statement that it is "theoretically incorrect" on such slender support as that given by the arguments presented. The fact that the cone clutch is rapidly giving way to the plate or disk clutch is too notorious to require statistical proof; as a personal experience, I may say that the two companies with which I am in the closest relationship have (quite independently) both abandoned the cone clutch in favour of the disk, and I could name out of hand some half-dozen or more other manufacturing concerns who have done the same. The author says that the development of the cone clutch has been sadly neglected, while other types have been "nursed" into public favour. I can find no justification for this in actual history; if the "other types," which term naturally includes the plate or disk clutch, had been proprietary articles manufactured and pushed by some particular components company the words quoted might have been justified, but so far as I am acquainted with history (in this country at least) clutches are, in every case, the design and product of the makers of the car, and there has been no "nursing" or pushing of any particular type whatever; as far as it is possible to judge, the plate clutch has come and is coming into its own on sheer merit. Against this positive accumulation of evidence we have a bald statement that:—

> "It is theoretically incorrect, due to the extra spring pressure required to drive by friction between flat plates, as compared with the wedge-shaped cone, and the uneven wear due to different rubbing speeds, owing to the fact that at every different radius of the plate there is a different velocity."

(Dr. F. W. Lanchester.)

The first of these objections is illusory, for the work done in de-clutching is due to the distance the clutch element has to be moved as well as the spring pressure, and to get an adequate clearance in the case of a cone requires a greater axial movement in proportion as the spring pressure is lighter. The second of the objections is also worthless in view of the fact that if, as initially adjusted, the wear is greater in one part of the disk than another a very small amount of initial wear will equalise matters. In brief, I consider the author's statement to be a rash and mistaken assertion unsupported by any evidence.

I will now say a few words more particularly on the clutch designs shown in Figs. 1, 2, 3, etc. These are referred to specifically as "designs," so are presumably open to criticism as such. I do not suppose that clutches of any of these designs have actually been made or tested—if they had been, it is probable that certain necessary modifications would have been made. One grave defect, for example, is the carrying of the driving torque through a sliding element of small diameter, namely, a splined shaft. When this is done the forces on the splines (or keys) are so great that the parts lose their freedom, and the clutch, once in and driving, tends to stay in, and it takes a heavy pressure to start its withdrawal. The torque should be taken by sliding elements of large effective diameter, so that the actual load transmitted through the sliding elements is minimised, and the sliding, therefore, not seriously interfered with by the transmission of power. This point is not raised purely on theoretical considerations. The trouble was met with in 1896 in one of the early Lanchester cars built by me in the experimental period. In this a feathered or splined shaft, about $1\frac{1}{4}$ in. diameter (substantially as shown in Figs. 1, 2, 3, etc.), gave trouble from the cause indicated and had to be replaced by driving dogs representing an effective diameter of about 5 in. or 6 in. A further criticism of these designs, more especially as touching the feature for which novelty is claimed, is that practically the same result can be achieved by a flexible clutch-pedal (or by a flexible connection between the clutch-pedal and the de-clutching lever): in both this and the proposed device there is a series of positions of the clutch-pedal between the position of full engagement and the position of complete de-clutching, each of which positions corresponds to a different degree of pressure between the clutch elements. Most clutch-pedals possess a certain degree of flexibility of this character, but it is admittedly small,

and, generally speaking, it is agreed by designers that increased flexibility possesses no material advantage. On large cars either device, namely, the flexible clutch-pedal or the additional spring device described in the paper, has one great disadvantage and is bad; the work done, that is to say the energy output required to fully de-clutch, is greater than when neither provision is made; consequently, for a given clutch-pedal movement a much lighter clutch (so far as foot pressure is concerned) can be designed by omitting the elastic connection between the foot pedal and the clutch cone. On very small cars the position is different, but, if I may judge in any respect from my own experience, I am very doubtful whether either the flexible clutch-pedal or the intermediate spring as described by the author has any prospect of a useful future.

The double clutch cones described with reference to Figs. 5, 6 and 7 strike me also as an unnecessary complication; at the best I can only see in the device in question a frank confession of the imperfection of the cone clutch, an imperfection that, if I may say so, though not altogether imaginary, is largely attributable to defect of design in the single cone type, such defect, for example, as that already pointed out above.

Referring now to the section of the paper on universal joint design, it does not seem at all clear in what manner it is intended that the proposed new form of universal joint should be employed. The joint depicted in Fig. 8 is evidently designed for very small angular movements, and presumably must be intended for two shafts that are fully housed in bearings of their own, but of which the mountings are not necessarily quite in line angularly or axially. In most cases the universal joint has other functions to perform, and the features of this particular joint would not be advantageous; more often than not a universal joint is used incidentally as a means of locating one end of a shaft which has no journal bearings of its own, and in many cases a coupling shaft with a universal joint at either end relies entirely on its joint articulations for its mounting and is not furnished with any independent journal bearings whatever; for such purposes the joint shown is inapplicable, and in fact it would only seem that it is intended to compete with the Oldham coupling. Now the Oldham coupling is equally capable of taking charge of small angular defects of alinement without in any way violating its geometry, if I may so express it, and it is difficult to see what precise advantage the

(Dr. F. W. Lanchester.)
joint shown is intended to secure. As a matter of fact, in modern motor car practice a double leather or other flexible disk joint has become the more customary method of coupling and appears to meet all the reasonable requirements of the designer. The origin of this newly proposed joint appears to be due to the author of the paper being under the delusion that an Oldham coupling is incapable of taking charge of angular defects of alinement, and it is his lack of acquaintance with the use of the flexible disk couplings which is so prevalent to-day. Parenthetically, it is to be remarked that in respect of the latter composite canvas-and-rubber disks have of late years almost entirely superseded the leather at one time employed.

In looking at the author's proposals for the improvement of the ordinary gear-box, I am wondering whether he has ever tried to carry out practically any of the suggestions he puts forward. I myself made successful use of helical gear on the old 10/12 type of Lanchester engine, and after many experiments succeeded in producing a relatively silent gear for the connection of the two reverse-rotating crankshafts. But these gear wheels were of very large size for the power transmitted and very accurately cut; they were not heat-treated or hardened after cutting, being of a hard grade of cold-blast cast iron. They were of the single-helical type fitted with shrouds to take the end thrust. The spiral angle of the teeth was about 20 degrees, giving almost two teeth of overlap. I have since conducted quite a campaign on the ordinary gear-box in conjunction with the Daimler Company with different types of spiral gearing in order to get perfect quietness in the constant mesh pair of gears, with a negative result. It was found that single-helical gears, in order to be appreciably more silent than ordinary straight tooth gears, require to be many times the tooth face, which necessitated an inordinately long box. Double-helical gears invariably gave as much noise as the straight tooth type. The truth is that with steels that have to be heat-treated and hardened, which is necessary when 50 or more horse-power have to be carried through gears of 3 or 4 in. diameter, it is impossible to obtain the necessary degree of accuracy to ensure silence. The position is, relatively speaking, worse to-day, inasmuch as by the modern methods of tooth grinding after hardening such vastly better straight tooth gears can be made than formerly. To anyone not acquainted with the practical difficulties of producing satisfactory helical gears under the conditions that exist, the

designs illustrated in Figs. 10 and 11 of the paper may look attractive enough, and perhaps some day it may be possible to produce helical gears commercially properly rectified by grinding, such as will give silent running. But when we know the long experience it has taken to enable firms engaged in the rectification work to produce really perfect straight tooth gears we can appreciate that the solution of the problem, if it is possible, is in the manufacture of the gear and not in the designing of the box, and in this vital necessity the paper teaches us nothing. There is, moreover, the fact that the helical gear, like the rectified straight tooth type, is dependent upon its accurate alinement for silent running, and this accurate alinement, however true it may be under no-load conditions, is never what it might be when the gear-box is carrying its full load stresses. The designer is "between the devil and the deep sea"; either his gear-box distorts measurably under load or it is too heavy to be entertained. It would be almost safe to make the assertion that if the gear-box itself and its mountings were rigid the noise of a gear-box would be quite as much due to the ball bearings as to the gears, supposing the latter to be properly rectified after hardening by modern methods.

There is one other point of indirect bearing on this question of gear form. It is too frequently assumed, as in the present paper,* that the thing to be aimed at is to secure that the drive shall take place through the pitch point where contact is purely rolling and not sliding contact, and to this view many existing text-books give support. But gears that have given trouble often display a curious erosion on both driving and driven wheels at this very point, that is to say, on the pitch line, and my view to-day is that if gears are designed to carry extreme loads it is desirable to avoid contact on the line where pure rolling takes place—the pitch line. At this point there is pressure without relative motion between the two surfaces, and this probably determines the squeezing out and exclusion of the oil-film, with the result that metallic contact takes place and the two surfaces of engagement momentarily weld together and pick particles out of each other. Brisk sliding movement seems essential, under heavy load conditions, to provide local lubrication between the lines of contact. Probably the actual erosion does not originate precisely on the pitch line where there

* Page 271, line 4, "Double-helical gears, unlike the ordinary straight spur type, give continuous rolling contact on the pitch line."

(Dr. F. W. Lanchester.)

is pure rolling, for otherwise a ball or roller bearing might be expected to show the same phenomenon, but rather on those points closely adjacent to it at which there is insufficient sliding to ensure lubrication and yet enough to cause the surfaces to bite one another. I regard the matter as one that requires laboratory investigation for its complete elucidation, but I am pointing it out here to prevent it being taken as true that "continuous rolling contact on the pitch line" is a final solution of the gear-box difficulties or even a desideratum.

Passing now to the epicyclic gear-box described with reference to Figs. 18, 26 and 27, and to the method for the calculation of gear-ratios put forward by the author. At the outset I would mention that this method, which is described as "an application of first principles" and as "better than a bookful of formulæ," is not new at all. It appears to be a paraphrase of what was taught at the Royal College of Science when I was a student there some thirty years ago. It will be found in another form in Dr. Low's work, "Applied Mechanics,"* and, I believe, in other text-books treating of the subject of gear trains. The basis in any case is the calculation of an epicyclic gear train as an ordinary fixed train with the super-position of a rotation as a whole (*en bloc*) according to the use to which the gear train is put. From the first experimental Lanchester car built by me in 1895 down to the latest models produced, epicyclic change-gear has been fitted without a single exception, and the method of calculation employed is exemplified as follows:—

The examples are taken from the Lanchester 10/12h.p. gear-box, *low gear* and *compound gear*.

Low Gear. Data.....Sun = 28 teeth.
Ring = 80 teeth.

TABLE III.

—	Revs.	+ 28
Sun Element	+ 80	108
Planet Spider.....	0	28
Ring Element	− 28	0

* "Applied Mechanics," by David Allan Low. Longmans, Green & Co. Second Impression (pp. 391—393).

The low gear ratio (in terms of direct drive) is therefore—

$$\frac{28}{108}=0{\cdot}259.$$

Compound Gear.

Compounding two similar gears as above by cross-coupling the planet and ring elements (Lanchester system), we have for the auxiliary gear:—

TABLE IV.

Aux.	Revs.	+ 80
Sun Element	− 80	0
Planet Spider.....	0	80
Ring Element	+ 28	108

whence

TABLE V.

Low.	Revs.	Low + 108.	Revs.	Aux.
Sun Element	+ 80	* 188	0	S
Planet Spider	0	* 108	108	R
Ring Element	− 28	80	80	P

and gear-ratio is— $\frac{108}{188}=0{\cdot}575.$

When the gears compounded are not identical it is necessary to reduce the one to the same terms as the other before the final operation, thus:—

Auxiliary Gear. Data†.....Sun = 34.
Ring = 86.

† Actual data from 10/12 Lanchester countershaft.

(Dr. F. W. Lanchester.)

TABLE VI.

Aux.	Revs.	$\times \frac{28}{34}$	$+ 70{\cdot}8$
Sun Element	− 86	− 70·8	0
Planet Spider	0	0	70·8
Ring Element	+ 34	+ 28	98·8

TABLE VII.

Low.	Revs.	Low + 98·8.	Revs.	Aux.
Sun Element*	+ 80	178·8 *	0	S
Planet Spider*	0	98·8 *	98·8	R
Ring Element	− 28	70·8	70·8	P

and gear-ratio is— $\frac{98{\cdot}8}{178{\cdot}8} = 0{\cdot}55$.

It may be noted that the numbers given in Tables III., IV. and V. under the heading of "Revs." are merely the inverse of the respective tooth numbers, consequently these figures represent a ratio which may be expressed by any multiple or sub-multiple, whole or fraction of the numbers in question. It is in fact thus that the column 2 is derived from column 1 in Tables V. and VII., the figures being multiplied by $\frac{28}{34}$. In all cases the asterisks denote the two elements forming the driving and driven members; in a single gear train the third element is that to which the brake is applied and its revolutions consequently are zero. In the case of the low gear when driven by the auxiliary gear, as in Tables V. and VII., the third member, namely, the ring member, is rotating at a predetermined ratio owing to its coupling with the planet member of the auxiliary.

In manipulating epicyclic trains of gear in tabular form, as in the tables given, it is useful to remember that multiplying the revolution numbers by any constant is merely defining a different

period or number of turns or cycles through which the gear train is supposed to be operated, whereas adding or subtracting a given constant is equivalent to superposing that particular number of revolutions on the gear-box as a whole.

With regard to the proposed epicyclic gear-boxes themselves, as one who has spent many years in the development of epicyclic gear for motor vehicles, I must confess myself unfavourably impressed There are a few simple rules which, based on experience, I have laid down as desirable in the design of epicyclic change-speed gears, as follows:—

(1) A three-speed epicyclic gear-box is as effective on the road as a four-speed change of the ordinary type, owing to the fact that gear changes can be made with certainty under any conditions changing up or down; consequently the three-speed gear-box is the solution that circumstances demand.

(2) That each individual gear train shall be symmetrically arranged in one transverse plane, that is to say, that the planet gear shall be a simple pinion engaging internally with the sun and externally with the ring member.

(3) That planet elements shall be mounted on roller bearings.

(4) That there shall be no end-ways sliding movements or thrusts and that radial pressures as due to the drum brakes shall be balanced.

(5) That the clutch shall act definitely between the driving and the driven member and not by locking intermediate parts of the gear assemblage.

There are reasons for all these rules, but I suppose that any of them can be broken if proper precautions are taken. However, on looking at the epicyclic gear-boxes propounded by the author, we find pinions mounted on shafts and transmission from one point to another, plain journal bearings not fitted with either balls or rollers, and with no special precautions to ensure adequate lubrication; motions involving end thrusts with apparently no good provision for disposing of them; and clutching between various intermediate members of the gear train instead of between the driving and the driven shafts. We see band-brakes depicted which notoriously give unsymmetrical resultant pressures on the internal bearings, in any case difficult of lubrication, and in addition to all this we find sliding gears introduced under conditions when the small chips of steel that invariably result from a bad change may

(Dr. F. W. Lanchester.)

do untold mischief. It is difficult to believe with all these glaring defects that the designs in question are really going to lead the way to any radical improvement, and I feel that to the unwary designer they might prove rather a snare than otherwise.

I now pass to the worm gear section of the paper. In Figs. 31, 32, 33, 34, 35 and 36 there is a quite unimpeachable appli-tion of the diagram of the resolution of forces which, perfectly correctly, shows, under the varying conditions obtaining along a line AB (tangent to the worm cylinder and forming a generatrix of the wheel cylinder), the required resolution of forces necessary to the stated conditions of the problem. The author then takes a flying leap and gives us the geometrical

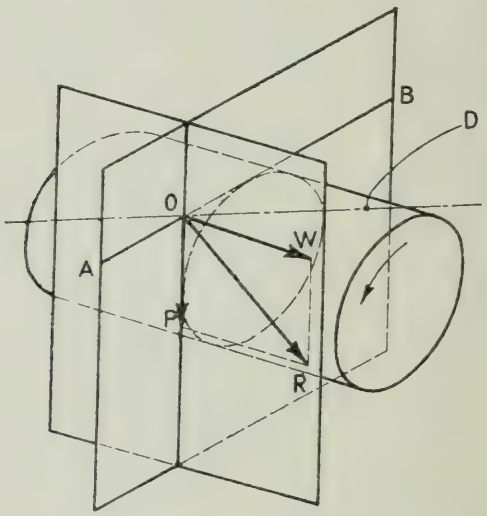


FIG. 51.

basis of the new F.J. worm which, it would appear, is produced from a straight line generatrix tangent to the pitch cylinder (as shown in Fig. 43), such a form as would be cut by an imaginary bowstring tangent to the pitch cylinder of the worm and coinciding with its angle of lead at the root of the teeth. There appears, however, to be no connection, and certainly no proof, that this particular form of worm corresponds to, or gives rise to, the system of forces described in Figs. 33, 34, 35 and 36. Moreover, it seems to me, and I can find no flaw in the argument, that the system of forces in question is utterly inconsistent with the tooth form shown. Taking for example Fig. 34, the worm and wheel are supposed to be in contact at the point O and the line OR

represents the pressure acting between them, more specifically the force acting on the worm from the wheel. Now at the point of contact O the surfaces of the worm and wheel are essentially tangential one to the other and to a plane in common at right angles to the line OR, but from the geometry of the worm itself given in Fig. 43 this plane also contains the generatrix of the worm, which is a line OD, Fig. 51, which is not and cannot be contained by a plane at right angles to the line OR. If this last statement needs proof, in the Figure in question AB is at right angles to OR, therefore AB is contained by a plane at right angles to OR. If, now, this plane also contains line OD it will be a plane tangential to the worm cylinder along its top line and will contain incidentally the line OW which is not at right angles to OR, and, therefore, the plane itself cannot be at right angles to OR. I apologise for this demonstration as the fact appears to be obvious.

I am not suggesting for a moment that there is not some distribution of forces that is consistent with the F.J. worm as defined by Fig. 43 and with a uniform ratio of motion between the worm and the wheel. I am taking exception, firstly, to a demonstration which it appears is no demonstration at all, and, secondly, to the statement in any shape or form that the triangle of forces analysis of Figs. 33—36 has any relation to the F.J. worm as disclosed in Fig. 43. The system of forces disclosed in Figs. 33—36 is only one of an infinite number of possible systems which give uniformity of motion as their result; the best that can be said is that the F.J. may correspond to one of this infinity of possible systems.

There are certain facts in connection with the parallel type of worm gear that it is impossible to evade. If any section be taken at right angles to the axis of the worm wheel then the kinematics of that section are self-contained, for as the wheel rotates the form of the section of the wheel itself is unaltered, and as the worm rotates the form of its section is also unaltered and no juggling with geometry will obviate the fact that that particular section of the worm can and must be considered in theory as a rack, and that particular section of the wheel is definitely a gear wheel in engagement with that rack, and further that all conditions of tooth form that obtain as between "rack and pinion" must obtain for every such section of a worm and wheel. We know that in gear wheel design, within the limits defined by interference, any form of gear tooth arbitrarily chosen may be paired with a correspond-

(Dr. F. W. Lanchester.)

ing gear whether the gear in question be a pair of spur gears or a rack or toothed wheel. In the case of worm gear in which the process of manufacture is to produce some standard shape of worm and hob the corresponding wheel, the conjugate form for every section is self-generating, no matter what arbitrary shape be initially selected for the worm tooth form. Of course, the limiting effect of interference still applies, otherwise one part of the hob may cut away the tooth form correctly generated by another part.* The definite fact remains that as the worm rotates the section parallel to its axis undergoes no change other than a uniform longitudinal movement exactly simulating the movement of a rack,

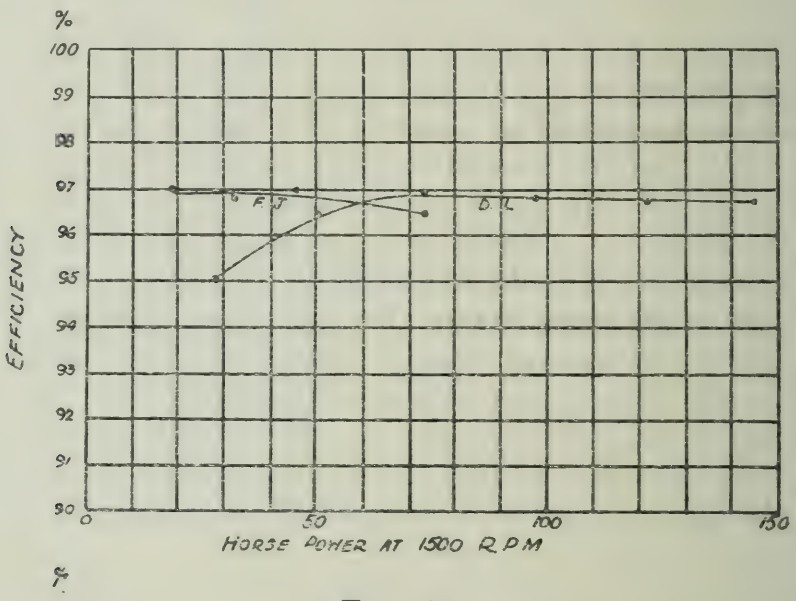


FIG. 52.

and the section of the worm wheel in gear with it has to be related to it according to the ordinary rules of rack and pinion tooth form. I can find nothing in the F.J. method of generating that distinguishes the particular result from any other reasonable and rational basis of worm geometry.

When we compare worm gear generated on the straight worm

* Generally speaking, as between the worm and the wheel, in order to avoid interference, the contained angle of the worm tooth must be made large, especially in a quick pitch worm. In any case the only effect of interference is to render portions of the rack and wheel system unavailable, in other words to make the product worse than it might be.

basis with that generated on the Hindley basis, as was done in my own paper on Worm Gear and Worm Gear Mounting* by cutting actual sections at right angles to the worm wheel axis, and so exploring the whole field of contact, the deficiencies or defects of the parallel type of cutting are at once manifest, and these defects are not actually dependent upon any factor other than the system of cutting, since the clearances between the teeth are due as shown by the sections given to the ordinary requirements of rack and pinion tooth form and represent the differential motion as between a rack and pinion at the various distances from the worm wheel axis. The Hindley system is the only one which actually obliterates the tooth form from consideration. A result of this is

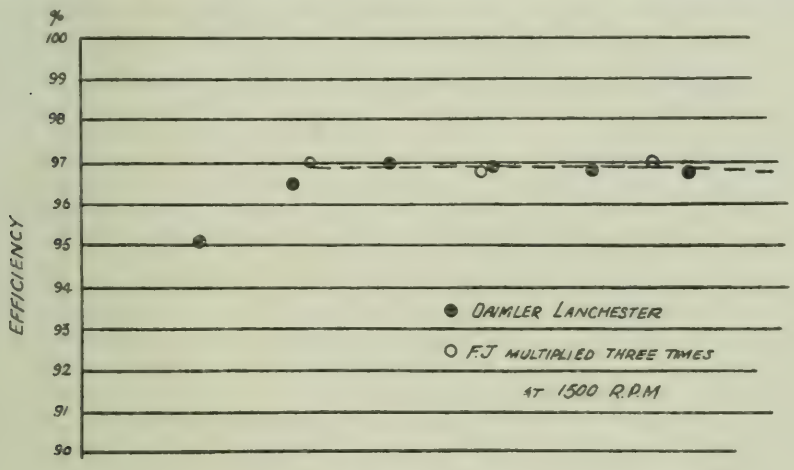


FIG 53.

reflected in the results of tests on the Daimler-Lanchester dynamometer at the National Physical Laboratory. Fig. 52 shows the efficiency curves in accordance with the N.P.L. Report for the F.J. worm as published by Messrs. David Brown & Sons and the corresponding efficiency curve for the Daimler-Lanchester gear cut on the Hindley system; from this it appears that, efficiency for efficiency, the Daimler-Lanchester gear will carry approximately three times the load of the D.B.S. gear. In Fig. 53 the curves have been replotted and compared on this basis. It has been claimed for the F.J. worm that it has given a record efficiency. It is well to note that the margin on which this claim is made is on a margin of 0·2 or, at the most, 0·3 of 1 per cent (Figs. 52 and 53), which actually is less than the N.P.L. will actually guarantee for

* See Proc. I. A. E., Vol. XI., p. 79.

(Dr. F. W. Lanchester.)
the accuracy of the dynamometer. For example, if within the limits of the said efficiency the F.J. had registered a high limit figure and the Daimler-Lanchester a low limit figure the positions would have been reversed. It is unusual to make any very important claim on a margin that is less than the turn of the scales. Moreover, it must be remembered that the efficiency of the worm gear as tested includes the efficiency of the ball journal and thrust bearings on which the worm and wheel are mounted, and in view of the fact that the Daimler-Lanchester gear gave its maximum efficiency at about three times the load of the new F.J. worm, at which point the ball journal and thrust bearings were heavily overloaded, the difference, even if it exists, may not be due to the efficiency of the gear, *quâ* gear, at all. It is clear, in any case, that the sacrifice of two-thirds of the power-transmission-capacity of a gear is a high price to pay for a problematical 0·3 per cent increase of efficiency, an amount scarcely visible in the scale of plotting of Figs. 52 and 53.

Mr. G. Watson: Though some parts of the paper, particularly that relating to epicyclic gears, are of great interest to designers, I am not in agreement with the author in much of what he says, especially with regard to clutches. Much room for improvement in the design of clutches though there is, I do not think that improvement should be sought in an increase in the number of parts, such as the use of double cone clutches, or in any direction which calls for greater skill on the part of the driver. It seems to me, too, that the author rather ignores the existence of the dry plate clutch, or, if he does not ignore it, he touches on it only very lightly, and condemns plate clutches on the score of drag between the plates. If he is referring to the multiple disk type running in oil, I quite agree; the drag was frightful, and gear changing exceedingly difficult. I cannot agree, however, that the plate clutch is theoretically incorrect, simply because it requires greater spring pressure than the cone clutch. It may do so, but it gives a smoother action, and it is possible by the introduction of leverage in the pedal gear to reduce the required foot pressure to quite a reasonable amount. I know of no clutch that is simpler in design, cheaper to produce, or sweeter in operation than the dry plate clutch such as that which is used on the De Dion car, and in improved forms on the Albion vehicles and many others. One great advantage of the disk clutch is that the spinning weight can be reduced to a minimum, whereas with the

cone clutch, even though the cone be made of aluminium and every precaution be taken to reduce the weight, there still remains the spinning weight of the fabric, which is very considerable, and it makes gear changing difficult and requires greater skill on the part of the driver. The clutch shown in Fig. 4, with an oil dashpot, is very ingenious, but I have never succeeded in persuading oil to go exactly where I wanted it, and I should expect some of that oil to find its way out and lubricate the clutch surface rather than go back into the crank shaft passages.

With regard to cardan universal joints, I agree with Dr. Lanchester that we do not want anything of the nature of the Oldham coupling, or a glorified form of it; in fact, I cannot see that we want any mechanical coupling. The fabric disk has given wonderfully satisfactory results; it is cheap, it requires no lubrication, and the only attention it requires is to go over the pins and nuts occasionally with a spanner.

The various gear-boxes shown are interesting as suggestions, but I feel quite sure that the author cannot have made any of them or he would have discovered their weaknesses. I do not wish it to be thought that I am an advocate of the sliding type of gears, but I do think that that type has been a most useful one, and before we discard it we must find something which is better, and, at the same time, cheaper to produce and maintain, and of equal efficiency. I fear that the designs shown in Figs. 10 and 11 will not give us these desirable points, as for a given number of speeds the box must be longer and more costly to produce. The author shows double-helical gears in which both gears are more or less positively located endwise. It has been my experience that where double-helical gears are used, one of the gears must be left free to float endwise in order to take up any position to suit the inequalities of gear cutting, because no matter how carefully they are cut, there will be some inequality, and if one gear is not free to move endwise there will be grinding, inefficiency and noise. The author suggests that there are only six gear wheels in this box. It looks to me as if there were eight, and, I should say, the cost of cutting twelve. My chief point of criticism of the design shown in Figs. 12 to 17 is the absence of any provision for taking the clutch thrust in the first and third speed positions, and the clutch-pedal will require far more skilful manipulation than can be expected from the average driver. Moreover, as the gear-box does not entirely eliminate the need for sliding gears, I do not consider it is a very

(Mr. G. Watson.)
great improvement on the ordinary type of box, except so far as the reduction in length is concerned. There are other disadvantages inherent in this design, for instance, the need for long sleeves which are more or less difficult to lubricate. Dr. Lanchester has dealt fairly fully with the question of epicyclic gears, but I wish to ask the author if he will add an Appendix to his paper giving us the actual bearing loads under various conditions on the planetary pinion pins, as I find that on some arrangements of epicyclic gears these loads are so enormous that there is very little life in the machine.

The author is not quite consistent in his remarks on axles. He talks about reducing unsprung weight, and then suggests putting four changes of speed and a reverse in the back-axle. In Figs. 28 and 29, the author shows us a rather novel and daring type of axle. In all ordinary forms of back-axles failure usually occurs through fracture at the junction of the flange connection between the tubular or conical extensions and the centre casing of the axle. Apparently the author recognises the location of this failure, so he puts a permanent break into the axle in the form of a universal joint. It looks to me as if this would, while reducing the unsprung weight to a considerable extent, introduce the objection of varying angular velocity between the two axle shafts, and as this variation of angular velocity must be taken up by the differential gear, the teeth will be pounded pretty badly.

Mr. Max R. Lawrence: I am afraid that I cannot agree with the opinions of the author on many points, and in others I am afraid I do not follow his line of thought. He starts out by saying that the conical clutch can be made as efficient as a disk clutch, and that if the same amount of work had been put into its improvement it would be so much superior that plate clutches would be completely superseded. He then goes on to describe how this should be done, but from this it is evident that he has overlooked the chief features which are essential to clutches for motor cars with Panhard type gear-boxes.

To readily change speed it is essential that a clutch should have very little inertia; that the power it transmits should be under complete control, and that it should take up the drive without jerk, also that it should stand quite a large amount of slipping without injury. Now the maximum amount of power that can be applied comfortably by the driver through the clutch-pedal is about 100 inch-pounds, and 75 inch-pounds is what suits me

best. I like to rest the weight of my leg on the clutch and apply about five additional lb. by muscular effort to withdraw it. The weight of my leg, sitting in a comfortable driving position, is about 20 lb., so that 25 lb. is my comfortable pressure. I cannot move my toe or the part of my foot on the pedal more than 3 in. comfortably when driving in thick traffic, but 4 in. is not impossible. Lighter or heavier clutch-pedals are not so comfortable. The clutch shown in Figs. 1, 2 and 3, if the print is to scale, will require a movement of about $1\frac{3}{4}$ in., and so the clutch spring

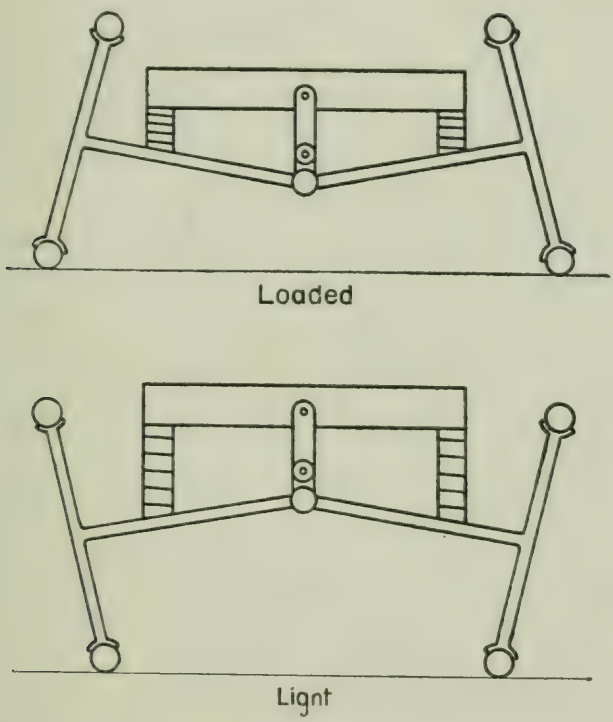


FIG. 54.

will only be able to give 65 lb. depressed, and would only exert about 30 lb. on the clutch, which I believe would be totally inadequate to drive a 16 h.p. car.

The four-speed gear-box shown in Fig. 10 must have nine gear wheels so far as I can see, and not six as claimed by the author. No four-speed gear-box is complete without its reverse, and this will require three wheels. One of the helical gears is also shown split in two, which makes a very difficult piece to make, and the author considers these two parts as only one gear wheel. I should also like to ask the author to state the speed at

(Mr. Max R. Lawrence.)
which the idle pinion A will revolve on high gear. The object of having a motor car gear-box of the Panhard form is to cut the gear wheels out of mesh when they are not in use, but this gear-box precludes that, quite apart from several other difficulties which it introduces. It would be interesting to know what form of mechanism should be employed for the change-speed gear, because the slider R has to be in the same position for the top and second gears, and for the first and third gears, and with another gear wheel in a different position, and it would be rather difficult, I think, to arrange. I think the axle shown in Fig. 30 is absolutely unstable, presuming that the link is solid.

Capt. Bramley-Moore: No, it is pivoted.

Mr. Max Lawrence: That makes a difference, but I cannot see why the axle would not be deflected when loaded and unloaded as indicated in Fig. 54, and I should like to know what device would be employed to make it stable.

Mr. W. H. Hingston: I am very pleased to see that the author has not confined himself to existing types of transmission gear, but has indicated some fresh lines of development, which, even if they do not lead anywhere in themselves, will certainly give us something to think about. In fact, I think the paper as a whole has the great virtue of being constructive, and not merely a description of things as they are, though there are a number of points with which I do not agree. The author refers to disk and plate clutches as extravagant luxuries. My own experience of single plate clutches is that they are not very much more expensive to manufacture than simple cone clutches, but if the cone clutch has to be elaborated as in Fig. 5 in order to get the results given by the single plate clutch, the position will be reversed. I do not understand why the author says that "it (presumably the single plate clutch) is theoretically incorrect due to extra spring pressure." The same pressure is required with a cone clutch or a plate clutch to transmit a given horse-power between a single pair of surfaces, provided the mean effective diameter is the same, and it does not seem to matter whether the pressure is applied by direct springs or whether it is boosted up by the angle of the cone. The thrust is self-contained in the ordinary single plate clutch, and the only time that the thrust is obvious is when de-clutching, and it is then that the advantage of the leverage of the small actuating levers to compress the springs makes itself felt, so that the actual declutching thrust is usually rather less

than that of the ordinary cone clutch. The author refers also to uneven spring pressure. If this occurs to such an extent as to matter, it is sure evidence of bad design or careless manufacture or adjustment of the clutch. In many single plate clutches the springs are short and stiff, so that a very slight variation in the length means a big difference in the pressure, and when this is the case it is almost essential that they should be fitted with a screw adjustment, which, if carelessly used, accentuates the uneven pressure. Now it is perfectly easy, with long, comparatively soft, low-rate springs, to design a clutch in which the springs do not require any adjustment, as they are able to follow up the plate sufficiently to take care of any slight wear. A clutch of this sort has the advantage that there is no possibility of people meddling with the adjustment and upsetting it. Any slight variations in the spring strengths are easily taken up by the rigidity of the presser-plate. I quite agree with the author's remarks about the multiple-disk clutch running in oil; it is certainly very apt to give trouble. His modification of the cone clutch, Figs. 1, 2 and 3, is rather an expensive way of doing what is ordinarily done by the small cushion springs under the fabric. In the author's design the initial engagement is effected by the soft secondary spring, and until that is more or less fully compressed the main spring does not come into operation. With the cushion spring arrangement, the initial engagement is again by these little soft auxiliary springs, and until these are squashed up flat the main spring does not come into operation, so that really it appears to be the same thing carried out in a more expensive way. There is, however, this difference, that the author's clutch gives engagement on the whole of the surface of the cone instead of on a series of projections where the cushion springs are, but I doubt whether it is worth it. In Fig. 5 an attempt has been made to get the advantage of the single plate clutch. Why not go the whole way and have the single plate clutch? It would be cheaper than the design shown, and it gives absolutely satisfactory service. I think the section of the paper dealing with epicyclic gears is one of the most valuable, and I am very much in agreement with the author in his statement that it is best to work on first principles when dealing with epicyclic gears—formulae are very little use. I rather gather that most of these designs have been put up to stimulate discussion, so that I hope the author will not think my remarks have been merely destructive.

(Mr. W. S. Shackleton.)

Mr. W. S. Shackleton: I was very much interested in the author's remarks on the subject of cars with both wheels rigidly fixed to a solid back-axle in which the differential is done away with. I can certainly bear out his statement that the tyre wear does not seem to be increased, at any rate in the case of a very small car, say, up to 12 cwt., because last year I drove a little car, a "Bleriot Whippet," upwards of 10,000 miles and the original tyres are still usable. The wheel track, which is 4 ft. 2 in., would seem to be rather excessive in the case of a car with a solid axle, but this does not appear to be so. The advantages are that there is a reduction in unsprung weight and elimination of wheel spin when a wheel leaves the ground, which happens continually when passing over present-day roads at any speed. This is so serious at really high speeds that many road-racing cars are fitted with solid axles. There is also better road grip obtained, both for driving and braking purposes, and elaborate devices to obtain brake compensation are unnecessary. Very modern, high powered, high speed cars, such as the Hispano Suiza, are, I understand, capable of spinning the road wheels on the second speed by suddenly depressing the accelerator pedal. It would appear, therefore, that a good deal of wheel spin must result at high speeds unless the springing is really extraordinary, and I should like to ask the author if he has had any experience with a differential in which friction is purposely introduced, such as by means of spiral or worm gears in place of the usual bevels or spurs, in order to eliminate wheel spin.

Mr. B. W. Shilson: The author says, "It is certain that modern high powered engines capable of quick acceleration can easily overload all parts of the transmission gear," yet on page 267 he advocates a very low angle for the clutch, which means that when the clutch is fully engaged there is no possibility of slip, whereas the single plate clutch is able to accommodate any sudden overload on the engine. He also states that the single plate clutch has been *nursed* into favour, whereas it should be said that it has *forced itself* into favour. I think I am right in saying that in America leather cone clutches have entirely disappeared, and that the single plate clutch is favoured for all types of vehicle. Another strong point in favour of the single plate clutch is the opportunity which it gives for having a neat universal joint right in the heart of the clutch, instead of having a heavy one overhung by a considerable amount from the engine

rear bearing, as shown in Figs. 1, 2 and 3. In such an arrangement whirling is apt to be set up, which may give trouble in the clutch or a chatter in the gear-box. Incidentally, the author shows a simple screwed collar for locking the withdrawal bearing, but this is liable to come off in practice. Further, it would be interesting to know how the author proposes to dismantle the clutches shown in these Figs. It would appear that either the engine or the gear-box would have to be removed before anything could be done to the clutch. The universal joint which he advocates is, like most of his other drawings, very ingenious, but it is doubtful whether a single joint of this kind is so useful as the ordinary arrangement with a joint at each end of the propeller shaft. The type of clutch shown in Figs. 5 to 7, in which there is a slow-speed action on the one withdrawal collar, must of necessity give a radial thrust on the shaft tending to make the clutch stick, i.e., to increase the pedal load, whereas with the single plate clutch any leverage advantage which may be desired can be obtained and the clutch-pedal may be so light that even the weight of the foot will cause the clutch to slip. The author refers to uneven wear due to the different rubbing speeds of the single plate, and I should like to know if he has any figures in substantiation of this, as my experience has shown that these plates always wear absolutely smooth, and their life is so great that they very considerably outlive the ordinary leather cone which he advocates.

Mr. J. J. Guest: In the design for a two-speed gear shown in Fig. 9, the torque transmitted is determined by the diameter of the crankshaft, which is more than 50 per cent greater than that of the cam shaft, that is, 3/2, though the strength of the crank shaft compared with that of the cam shaft will be that amount cubed, which is 27/8, call it roughly 3; as, however, the cam shaft is geared down from the crank shaft 2 to 1, the crank shaft should carry double the torque. In the illustration the cam shaft is shown smaller in diameter than the crank shaft, and would, therefore, clearly fail at once.

Mr. A. E. Parnacott: My experience has been that the difficulties connected with the cone clutch are generally due to the length of the bearing being insufficient. I would like to ask the author if he would have to have a bearing to support the shafts in the universal joint shown in Fig. 8. I take it from the design that he would have to have a bearing on each side of this universal joint in order to support it, because it is not self-supporting, as

(Mr. A. E. Parnacott.)
is the universal joint on the majority of cars. Fig. 9 shows a most ingenious and fascinating arrangement of two-speed gear, but I suggest that the kick due to the explosion will come always on two teeth in opposite phases on the wheel, and I think one of the safeguards of the modern car is the fact that the clutch takes up different positions, so that the kick is distributed over all the teeth.

Mr. A. A. Remington: In connection with the clutches shown in Figs. 5, 6 and 7, I should like to know if the author has tried the effect of obtaining smooth and progressive engagement by means of limiting and gradually increasing the spring pressure applied to a cone clutch. I have never found friction to be so kind as to obey theoretical laws in this gentle way, because the coefficient is not a definite value, and I think everyone knows a clutch will hold itself engaged when once engaged, and transmit the torque of the engine, until some slight jerk or jar, such as would be given by one of the wheels going over a stone in the road, starts it slipping, and then it will slip indefinitely. Many years ago I tried devices of this sort that have been submitted to me in connection with cone clutches, but have given them up as hopeless because I could never get sufficient regularity of action to make them worth while. The only form with which I have had any success consisted in a cone sliding on a shaft on spiral splines pressed into the flywheel by a spring, and the drive through the splines tended to throw the clutch out of engagement, whilst the spring tended to force it into engagement, so that the torque tended to compress the spring and throw the clutch "out." I feel sure that if the author had tried the various clutches which he advocates he would cease to advocate them.

Dealing with epicyclic gear-boxes, it is useful to have the first principles for calculating epicyclic gears laid down in a clear manner for issue to the drawing office, although I have never heard of anyone using any other way. I notice, however, that the author is quite silent on an equally important, but somewhat more difficult matter, mathematically, i.e., how to calculate the torque reactions between the various members under different conditions. The question of the pressure on the pins mentioned by Mr. Watson is a very important one and requires consideration, and I should be glad if, for guidance of the drawing office, the author would give an Appendix showing the torque reactions between the different members of the various cases on different gears. Take

for example, the gear-box in Fig. 26. There is a band-brake, not shown in the drawing, that surrounds the member marked A3, which is a brake-drum that has to be held stationary to transmit the engine torque on certain gear ratios, but the torque that has to be overcome to hold the drum stationary is not necessarily equal to the engine torque. I have not been through this particular design, but it may be considerably less than the engine torque, or it may be considerably more. The same conditions exist between the member marked B3 and the cone marked B4, and also, of course, between the flywheel and the other cone. Where the members are brought to rest the conditions are simple, but where both move, as in the case of the cone and the flywheel, although in reality quite simply worked out mathematically, they may become somewhat complicated to novices in epicyclic gears. If the author could give us the reactions and the spring pressures required to hold these members in engagement, I think it would add greatly to the value of the paper, as it would sound a strong note of warning against the adoption of epicyclic gears of this class for gear-boxes. Dr. Lanchester gave us the simple element and, of course, in his case, the proposition is simple, but with these designs, where the different members react upon each other, some of the results are likely to be somewhat surprising to one who works them out for the first time.

I should like to support what Dr. Lanchester said with regard to the worm, and to ask the author if he would show us how he works out the sliding of the point of contact across the worm wheel, because I cannot see from the paper how he gets the results given in Figs. 47 and 48 as compared with Figs. 49 and 50, or any reason to assume they are correct.

Mr. L. A. Legros: In Figs. 5, 6 and 7, the author shows a clutch with two conical surfaces engaging in series; a somewhat similar device, but with the surfaces engaging in parallel, was in use by the Germans during the war, and one of the cars so fitted was exhibited recently at Aldershot. Actually, the double cone clutch illustrated does not appear to present any very marked advantage over the dry plate clutch, and it certainly appears to take up more length, which is often an important consideration in design. In the German cars there were several examples of very complex gear-boxes; for various reasons, but often in connection with the winding gear, a second lay-shaft was arranged below the first lay-shaft. When the speeds obtainable were calculated from the

(Mr. L. A. Legros.)
reduction ratios of the gear combinations, it was found generally that if the speeds obtainable on the one were a, b, c, and d miles per hour and those on the other a^1, b^1, c^1, and d^1 miles per hour, some of the latter would, in two or more cases, be found to be nearly the same as those obtained from the first or original lay-shaft. Dr. Lanchester has urged upon us that engineering is an art rather than a science, and bearing this in mind I made further examination of some of the curious features of the German cars, in the hope of finding an underlying trend of design or teaching. I found a certain car had a steering gear in which the lever arm connecting the reduction gear to the radius rod was of abnormal length, about three times the usual proportion, moreover it was not straight or even dog-legged, but changed its plane of bending three times.

This should, I think, be a warning to us to consider any new schemes, such as some of those put forward, by comparative examination on scale drawings; for instance, saving of length in the gear-box is one of the most important points in the design of that detail, and certainly some of the proposals appear to add to the length. The engagement of gears, whether two external gears or one external and one internal, depends on the difference of velocity at the point of engagement, and the weight of evidence is that the difficulty of gear changing is not really great, otherwise some arrangement like that proposed and tried by the late Mr. Henry Lea would have been adopted. Lea's arrangement, it will be remembered, consisted in cutting back the alternate teeth of the gears on the engaging sides to the extent of, say, a tenth of an inch so that the arc of entry was increased from a fraction of the pitch to approximately a whole pitch plus this fraction.

Mr. O. D. North: I should like to support the author, although he has been so severely criticised. I believe the helical gear-box shown in Figs. 10 and 11 is quite all right so far as the end location of the gears goes, if we assume that the pinion A has a little bit of end float on the bush, and that the lay-shaft is not held endwise. It is a question of allowing clearance, but the box, as shown there, could be so constructed that there would not be any end strains, and it would work, although I would not like the job of making it. I believe Mr. Watson quite rightly pointed out that some of the wheels would have enormous velocity on top gear. The jointed back-axle, shown in Fig. 29, would set up side strains on the tyres on the road, which would be very detrimental. In

reference to universal joints, I should be glad if the author could give us some idea as to the conditions under which a simple internal clutch form of universal joint would be satisfactory, by which I mean an internal ring with case-hardened teeth and a pinion, with the teeth rounded off, fitted inside. When used as a short coupling to a gear-box, it generally develops play more quickly than anything I have ever seen, but I do know of one man who put in such a coupling on the rear cardan shaft with 25 teeth of about 5 pitch, which ran fairly well for about 15,000 miles, though such an arrangement gives practically only line contact. The difference in behaviour of this class of joint is remarkable. I have seen cases where they have stood up satisfactorily, but in other cases they have developed knock in a few hundred miles. With regard to the worm gear, I think I must support the author. Dr. Lanchester claims, and, I think, quite rightly, that his gear in transmitting 150 h.p. would stand a load three times as great as that on the F.J. worm and still give a high efficiency, but the point is, does he recommend that gear for transmitting so much power, in other words, is it three times as big as it ought to be for the particular job? I think the Lanchester gear is not much too big, and possibly the F.J. worm might claim that it can give a higher efficiency under normal conditions on top gear when the gear is not loaded to its maximum capacity, or anything like it, and in spite of the admitted advantages of the Lanchester gear, it cannot in practice be loaded to the heavy intensities required to develop a high efficiency to get the very last 1 per cent when lightly loaded; to do so there must be a much higher intensity of loading on the Lanchester type gear than on the F.J.

Dr. Lanchester: Or a smaller gear.

Mr. North: But you would not dare to make them much smaller.

Dr. Lanchester: The ratio is about $1\frac{1}{4}$.

Mr. North: That does rather answer my point, and I am very glad to have brought it out.

Capt. Bramley-Moore, in replying on the discussion, said: I am glad to see that the paper has provoked a good discussion, and I may say that certain portions of the paper were deliberately worded with that end in view. I thank those who have made favourable comments regarding this type of paper, which are all

(Capt. Bramley-Moore.)
the more acceptable seeing that, generally speaking, very little encouragement is given to those who bring forward new suggestions or ideas.

Gear tooth design.—Dr. F. W. Lanchester makes the amazing statement "that if gears are designed to carry extreme loads it is desirable to avoid contact on the . . . pitch line." Furthermore, he states that "brisk sliding movement seems essential under heavy load conditions." Both these conclusions are undoubtedly incorrect, and no designer with experience in high-velocity turbine or heavy rolling-mill gearing would give credence to such views. The more nearly pure rolling contact takes place on the pitch line, then so much the less is the erosion of the gear teeth. With all forms of involute gearing a certain amount of sliding takes place between the teeth at all points except on the pitch line, and the erosion referred to is due to the relatively "brisk sliding movement" which Dr. Lanchester regards as essential. This erosion originates just below the pitch line, at which point the whole of the addendum portion of one tooth is sliding over a small portion of the dedendum of the other. The greater the sliding, the greater the erosion (other things being equal), and modern gearing has to be specially designed to reduce this sliding to a minimum, the aim being to obtain a tooth action approximating, as far as possible, to pure rolling contact. The reason why modern gearing is able to withstand the high velocity and heavy loads which it is now called upon to transmit is entirely due to each pair of gears being individually designed to minimise this sliding action. The whole of this subject has been investigated by gear manufacturers, and the conclusions arrived at unmistakably indicate that "continuous rolling contact on the pitch line" is the desideratum of gear design.

The fact that Dr. Lanchester's "double-helical gears invariably gave as much noise as the straight tooth type" is a tacit admission that either the design, the mounting or the manufacture was at fault. The greater silence which can invariably be obtained with double-helical gears, when correctly designed, is due to continuous pitch line contact in conjunction with the advantages obtained from the use of a finer pitch than would be possible with a straight cut spur.

With reference to Dr. Lanchester's remarks regarding worm gear tooth design, I note that Figs. 31 to 36 are "quite unimpeachable," but would point out that Figs. 37 to 50 are likewise correct.

If Dr Lanchester has jumped direct from Fig. 36 to Fig. 43, then he has certainly taken a "flying leap" from the resolution of forces to the geometrical basis of the F.J. worm gear, but my paper does not do so. Even a superficial examination will clearly show that the various stages of development are both illustrated and explained. Dr. Lanchester, in taking his "flying leap," has obviously overlooked the resolution of forces illustrated by Fig. 38. He states that in Fig. 34 "the worm and wheel are supposed to be in contact at the point O"; but this is his supposition, not mine. There is no statement or even suggestion that the point O in Fig. 34 is a point of contact between worm and wheel. I remark that "Fig. 34 represents a view of the same plane (i.e., the plane PWR: see Fig. 33) when moved through 90 degrees round the worm pitch cylinder, the point O still being on the pitch cylinder of the wheel." (Page 295, lines 7 to 9.) If he will refer to Fig. 37 he will note that there is only one position of the plane PWR in which the point O (which always lies on the line AB) is actually a point of contact, this position being that where the line AB cuts the zone of contact on the circumference of the pitch cylinder. To avoid misunderstanding, this position is furthermore illustrated by Fig. 33. If the point O be at any other position along the line AB, then, clearly, it cannot be a point of contact. Hence his argument against the F.J. worm gear falls to pieces, as nobody except himself ever supposed that the point O in Fig. 34 was a point of contact between the worm and the wheel. If Dr. Lanchester wishes to know the position of the point of contact corresponding to Fig. 34, he has only to refer to Fig. 38, which shows the identical resolution of forces, and wherein I clearly state that D is the point required.

Having misunderstood the elementary geometry of the F.J. worm gear, he next goes on to assert that "the system of forces disclosed in Figs. 33 to 36 is only one of an infinite number of possible systems . . . the best that can be said is that the F.J. may correspond to one of this infinity of possible systems." Now it has already been shown, in my paper, that the F.J. design of worm tooth is the only possible design correctly based upon the application of the principle of the triangle of forces. But there is only one system of triangle of forces, hence it follows that the F.J. system, far from being one of an infinite number, is the one and only solution ever yet put forward.

Dr. Lanchester next goes on to speak about interference which

(Capt. Bramley-Moore.)
certainly does render portions of the gear teeth unavailable, and destroys that valuable characteristic of uniform velocity which I have emphasised in my paper. This interference is detrimental to the gearing, its effect on the lines of contact being illustrated in Fig. 45. Within the limits of lead angles employed for automobile and other power-transmission purposes, there is, however, on the F.J. system, no interference whatever. The uniform path of the F.J. lines of contact, from beginning to end, are illustrated in Figs. 46, 49 and 50.

Little advance can be made by referring to photographs of "dead sections," nor can any mathematical thesis be built up from such data. Examination of worm gear engagement by means of a series of parallel sections has already been dealt with in previous papers and needs no reintroduction.

Dr. Lanchester makes comparative remarks about the clearances between worm gear teeth, but has overlooked the fact that in order to reduce sliding it is necessary for the teeth to have relatively quick engagement and withdrawal. The actual amount of clearance has very little direct bearing on the question, because once the tooth surfaces are outside the zone of contact it does not matter whether they are one-hundredth or one-tenth of an inch apart. Indirectly, however, this matter is important, because the larger clearances provide for a larger supply of lubricant.

Dr. Lanchester claims that "efficiency for efficiency, the Daimler-Lanchester gear will carry approximately three times the load of the D.B.S. gear." If this is the case I cannot understand why the Daimler-Lanchester gear is not made half its present size for a given power and sold at half the price. This, however, is not done, and the claim is unsubstantiated.

I cannot understand his remark regarding the efficiency of the F.J. gear being "at the most 0·3 of 1 per cent" above the Daimler-Lanchester. The National Physical Laboratory have a rule that no extract is to be made from any test unless the results of the whole test are made public. The highest efficiency reading of the Daimler-Lanchester worm gear, according to full published reports of tests up to date, stands at 96·8 per cent. As the F.J. stands at 97·3, the margin is 0·5 per cent. Moreover, in the N.P.L. report on the F.J. gear, dated 20th October, 1919, it is stated that "the accuracy of the efficiency determinations is estimated at ± 0·1 per cent." Hence, Dr. Lanchester's claim that had the efficiency of the F.J. gear "registered a high limit

figure and the Daimler-Lanchester a low limit figure the positions would have been reversed" is not correct.

My paper does not deal with the merits or demerits of the Hindley gear, but I note with interest that "the Hindley system is the only one which actually obliterates the tooth form from consideration." It has, indeed, always been my contention that the theoretical tooth form of the Hindley worm wheel is largely obliterated by interference, so that short worms have to be used in conjunction with narrow faced wheels. We shall, therefore, probably look in vain for the publication of the mathematics of the Hindley worm gear.

I note that Mr. Remington supports what Dr. Lanchester has said regarding the F.J. worm gear, so no doubt my reply to Dr. Lanchester will also fully answer Mr. Remington. Referring to Figs. 47 to 50, I am surprised that Mr. Remington should state that he "cannot see . . . any reason to assume they are correct," because the question in point is capable of such simple verification. These results are obtained direct from the formulæ given in my paper. These formulæ provide all the necessary data: any junior draughtsman can work them out. If plotted for worms similar to those shown in my paper, the results obtained will be identical to those shown in Figs. 47 to 50.

Clutch design.—Mr. Watson refers to "the spinning weight of the fabric, which is very considerable," but in Figs. 5 to 7 there is no fabric on the driven member. This is made from two light steel pressings, spot welded. Spinning weight is also minimised owing to the fact that this type of double cone clutch is smaller in diameter than the ordinary pattern. I thank Mr. Hingston for his remarks, but regarding clutches would point out that experiments made by me indicate that small cushion springs under the fabric do not give the same effect as the double spring design shown in Figs. 1 to 4. The latter enables the clutch cone to locate itself in an absolutely concentric manner before transmitting any load. This is not so with other types. In reply to Mr. Remington, I have tried the effect "of limiting and gradually increasing the spring pressure" of a cone clutch. On some occasions I got wonderfully good results, but at other times, under apparently identical conditions, unsatisfactory results. I put this down to variation in the condition of the leather clutch lining, which I intended to dispense with, but could not proceed further due to lack of time and high cost of experimenting. Major Strickland's

(Capt. Bramley-Moore.)

suggestion that the actual clutch-pedal lever might be made of spring steel, in order to obtain a gradual spring engagement, seems excellent, and has, I believe, already been experimented with.

Universal joints.—I note Dr. Lanchester's remarks regarding universal joints, but because I offer a suggestion whereby one joint can do the work of two, it does not entitle Dr. Lanchester to assert that my design is due to my "lack of acquaintance with the use of the flexible disk couplings which are so prevalent to-day." Now it so happens that the Bradford Corporation Trackless Trams were manufactured by David Brown & Sons (where I was then chief draughtsman) in the year 1913 and were fitted with flexible disk couplings at my own recommendation, so that Dr. Lanchester's statement is unfounded. Incidentally, the joint shown in Fig. 8 is designed to go between the clutch and the gear-

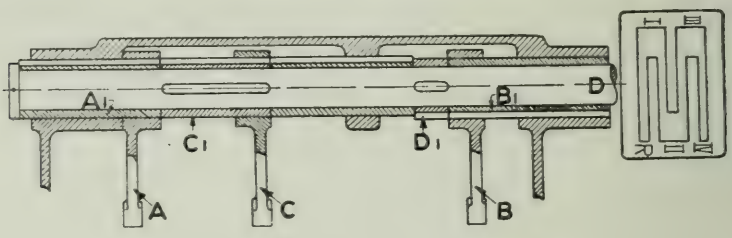


FIG. 55.

box in conjunction with flexible disk couplings between the gear-box and the back-axle.

Gear-box design.—In reply to Mr. Max Lawrence, although the movements of the sliding members in Figs. 10 to 11 are unusual, yet the operating mechanism is of a simple character, as shown in Fig. 55. It will be noticed that the change-speed lever never has to be moved sideways during a change of speed. In the position shown the levers A and B are locked to the gear-box casing by means of the sliding sleeves A1 and B1, and C can be operated from the change-speed shaft D through the keyed sleeve C1. This latter will engage with A if the change-speed lever is moved to the left, and the keyed sleeve D1, shown in neutral, can engage with B if moved towards the right. An identical operating mechanism was made just prior to the war, and was fitted to a similar type of gear-box (i.e., four forward speeds with only six wheels) except that silent chains were used in place of double-

helical gears. As I left for France in August, 1914, the box was not tested on this side, but was sent over to me (with the permission of H.Q. Third Army) in the autumn of 1915. Up to the time of my leaving France excellent results were obtained (it being fitted to a very old type 30 h.p. Daimler chassis which required a new box), but subsequently, during the Retreat in March, 1918, it fell into the hands of the enemy and has never been heard of since.

I am interested to note that my design of two-speed gear shown in Fig. 9 was forestalled by Major Strickland,* and I agree with him that a similar arrangement might well "form the basis for a really cheap runabout." According to Mr. Guest's idea all modern crankshafts are far too big, as he states that "the torque transmitted is determined by the diameter of the crankshaft." This criticism has, however, been effectively dealt with by Major Strickland.

With reference to the location of double-helical gear wheels, it used to be considered advisable to allow one of the members to float sideways, but practice has shown that this arrangement is not always satisfactory. This type of mounting has given rise to extra noise and wear due to the banging to and fro of the loose member. It is now considered preferable to definitely locate both members from side movement whenever possible.

Epicyclic gears.—Dr. Lanchester claims that "a three-speed epicyclic gear-box is as effective on the road as a four-speed change of the ordinary type." I always knew that the ordinary four-speed gear-box was far from ideal, but I did not know that it was as bad as all that. True, in one well-known car where the gear-box and back-axle were made in one unit (I do not refer to the Singer type) gear changing was extremely difficult, but even then I cannot for one moment believe that a three-speed epicyclic gear is as effective as a four-speed ordinary gear.

There is only one firm which has made epicyclic gears on a big scale, and their gear-box, of which I believe hundreds of thousands are on the road, has met with entire success. I note, however, that this American car breaks Nos. 1, 2 and 3 of the rules laid down by Dr. Lanchester, and yet for all that it is one of the soundest commercial propositions ever yet put forward.

Back-axles.—I was amused by Mr. Watson's remark that as

* See written contribution, p. 342.

(Capt. Bramley-Moore.)

any failure in back-axles generally occurs near the centre, I have put a "permanent break" into mine "in the form of a universal joint." But no one has remarked on one of the peculiar advantages of my back-axle that I omitted to draw attention to, namely, that there is no bending moment in the centre of the axle. The bending moment, from being zero at the point of central suspension, increases to a maximum under the rear road springs. For this reason the axle is of strong design and the central casing can be made much lighter than usual. It is quite true that the design permits of varying angular velocity between the two axle shafts, but this varying velocity is a daily occurrence with axles of all types (except those without a differential), when passing over rough roads, as emphasised by Mr. Shackleton. I admit Mr. Watson's impeachment that Fig. 29 shows "a rather novel and daring type of axle," but those who take the trouble to work out the forces acting on this axle will find that it is a perfectly stable and practical proposition.

COMMUNICATIONS.

Lt.-Col. D. J. Smith wrote: I wish to congratulate the author on his paper, which will undoubtedly give rise to much thought on transmission problems. While I am not in agreement with all the suggestions which he puts forward, many will undoubtedly prove valuable in providing new points of vision. Some of the devices illustrated would, I feel sure, prove unsatisfactory in practice, notably that of the jointed back-axle, but certain modifications suggested by the author's ideas would, I think, prove valuable. Indeed, I have no doubt that modifications in transmission which will be seen shortly in practice will have had their inception in the paper.

I was, however, hoping that the author was going to save us from all the worry and expense involved by transmission gear as we now know it. I am convinced that very shortly, for pleasure vehicles at any rate, electric transmission will largely replace the present system. I remember many years ago driving a car which was fitted with electric transmission, and the driving was extremely pleasant. Both automobile and electrical engineering have progressed far since that date, and I am sure that at present, there would be no difficulty in constructing a vehicle with electrical transmission which would be a very great advance in comfort and ease of driving on the gear-driven vehicle. Such a transmission would abolish the clutch, the universal joints and the gear-box, and would offer many other advantages which I need not now go into. Any slight increase in weight would be more than negatived by the advantages gained.

I can corroborate the author's statement that no harm results to tyres through the absence of the differential gear. During last winter I drove a car which had no differential gear over a thousand miles in and about London. The freedom from skidding was most remarkable, and no undue wear was experienced. Many years ago, I also had experience with a heavy vehicle which had been run many thousands of miles with no differential without detriment. If the differential was eliminated and electrical transmission adopted, there need only be one gear on the whole transmission—the final back-axle drive which could be of any type desired.

(Major F. Strickland.)

Major F. Strickland wrote: In the first place, I should like to thank the author for taking quite a new line in the matter of papers, and one which appears to me to be a very useful one. We have had a large number of valuable papers dealing with actual results of motor construction, but I do not remember that we have had one before which has dealt with sugestions for improvement which have never been carried out. It appears to me that this form of paper is of great interest, and that it might be developed. Suggestions are always valuable as, even though the form in which they are made may not be perfect, they may lead to the necessary detail improvements to make them satisfactory in working being carried out by some other person. Many automobile engineers have ideas in their heads which for various reasons they have no opportunity of carrying out, and I think it would be of great advantage if these could be discussed more fully than they are now, as they might be of use to someone else. It seems to me possible that an interesting evening might be spent if members were invited to contribute such ideas on any given subject, which could be printed and circulated in the same manner as a paper and then discussed. I make the suggestion for what it is worth.

With reference to the paper, about 1900 I built a car to the design of a gentleman of the name of Carew Gibson which had a change-speed gear practically identical with that shown in Fig. 7, except in minor details, and this gear is illustrated in my book on Petrol Motors published by Messrs. Griffin & Co., Ltd. The camshaft was used as a layshaft in exactly the same way, but all the gear wheels were inside the crank case. The arrangement of cones was very slightly different, though the principle was exactly the same. In place of being engaged by a spring and pedal, however, the clutch was moved by a lever on the steering column, which was pushed one way for high speed and the other way for the low speed. The gear in principle was quite satisfactory, and I have always thought that it might form the basis for a really cheap runabout, as the number of parts in addition to the engine is very small.

Two difficulties arise, however, especially in the form shown in Fig. 9. In the first place oil works out along the sleeve and gets on the cone, and some provision should be made to prevent this unless the clutch is arranged with a view to running in oil. In the second place I think that with the arrangement shown the

pedal pressure required to hold the low speed in gear would be found to be too great for comfort unless the engine was of very small size. The torque on the low speed is, of course, raised in proportion to the reduction of gear, and as the ratio of a two-speed gear should be at least $2\frac{1}{2}$ to 1, the pressure required to keep the low-speed clutch engaged would be about $2\frac{1}{2}$ times that of the top-speed. When the low-speed clutch is held in the spring has to be compressed as well, so that the pedal pressure will be something over $3\frac{1}{2}$ times that required to compress the clutch spring.

In the actual car built, the engine had four cylinders, 3 in. by 3 in., and the pressure required to hold the low speed in was found to be pretty heavy. This was not a very serious matter, however, as the lever had notches to hold it in position and the pressure did not have to be maintained all the time.

I noticed that Mr. Guest, in criticising this design, pointed out that the camshaft was smaller than the crank shaft although the torque on it was twice as great. This criticism appears to me unfounded. In the first place I presume that the author has only made a suggestion and expects that anyone carrying out the design would be acquainted with the first four rules of arithmetic and would, therefore, be able (with the assistance of a text-book) to calculate out the size of the shafts to suit any particular machine he was designing. In the second place, crank shafts have to carry other stresses besides torque stresses. If Mr. Guest's ideas of motor construction were correct, the splined shaft of an ordinary gear-box and the propeller shaft ought to be stronger than the crank shaft in the proportion of the reduction of gear in the box, which is often about 4 to 1. As some cars have $2\frac{1}{2}$ in. crank shafts, his idea appears to be that the splined shaft and propeller shaft should in this case be about $3\frac{1}{2}$ in. dia. In this he appears at all events to differ from the large majority of experienced motor constructors.

I cannot agree with the author's remarks as to the disk clutch. In the first place, he ignores the dry disk clutch, which is now, I believe, the usual form. In the second place, it is quite possible to make a disk clutch to run in oil quite satisfactorily. A very moderate clutch stop should prevent a well-designed clutch of this type from spinning, and in this case the gears will not grate on engagement nor will the car creep in traffic. It is, of course, possible to design a very bad disk clutch, and many such have

(Major F. Strickland.)
been designed, but I know cars over ten years old in which the disk clutches have been quite satisfactory, and if one clutch is made right there is no excuse for others being made wrong. The low inertia of the disk clutch makes it very much the easiest to change gear with, and as I drove a car with a disk clutch for some time before war broke out, I found when I joined the M.T. that I had forgotten how to change gear!

With regard to the author's double-spring clutch, Figs. 1 to 3, it appears to me that his statement to the effect that "the moment the surfaces of the clutch surfaces come into contact the full pressure of the clutch spring comes into immediate operation" is hardly correct. The pressure on the clutch surfaces can only be the pressure due to the spring minus the pressure on the clutch-pedal. No doubt it is difficult to control the pressure on the pedal as accurately as might be desired when the movement for different pressures is very small, and the double spring will make this easier. The same effect would, however, be produced by a spring in any part of the connecting gear between the pedal and the clutch, and this would be simpler to arrange; in fact the simplest would be to make the actual pedal lever a piece of spring steel. I do not think that the author's remarks as to the ordinary gear-box are quite justified. If I remember rightly, the term "clash gear" which he uses was coined in America, and used as a term of contempt by the numerous makers who made other types, but these makers have now generally adopted the sliding gear themselves. I have no doubt that when perfectly ignorant people were set to learn to drive the ordinary Army lorry, which had a cone clutch of very large diameter, there was trouble, for many of these lorries are not at all easy to change gear on. I have driven about 5,000 of them personally, and it seems to me that many of them could hardly be worse. If machines were to be designed solely to suit the learner, however, every ordinary machine from a type-writer to a battleship would be made quite differently and would be far less efficient than it is. Ordinary drivers are not "trained" at the rate of 2,000 a week in one school, and with a good clutch gear-changing is a completely different thing from changing on many of the Army lorries.

The great merit of the ordinary change-speed gear from the driver's point of view is that whatever gear is in use the same thing is done to control the car. That is to say, that whatever gear is in use the clutch-pedal is depressed to slow or stop, and I

believe that any form of gear which takes its place must be equally simple in this respect if it is to come into general use.

In conclusion, I have again to express my thanks to the author for having struck out a new line in papers and given us some original ideas to think about.

Mr. GEO. STEINHEIL wrote: I quite agree with the author that the ordinary cone clutch is the most practical for all-round purposes. Probably one of the reasons for its partial unpopularity lies in the fact that in the early days of its adoption it was not properly designed, and was in very many cases under-powered. By under-powering, I mean that, although theoretically the clutch was supposed to be able to transmit the power required, in actual

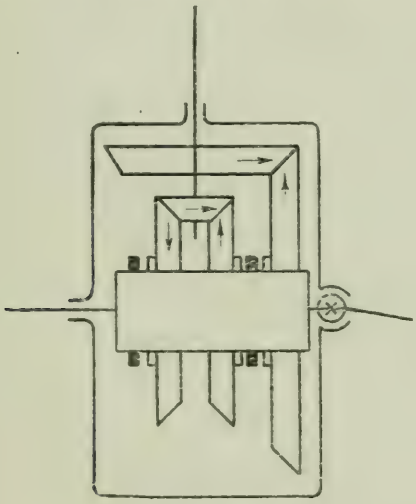


FIG. 56.

Two direct forward.
Two reverses, one of them direct.

practice it often failed to do so, especially when the car got into deep sand or boggy ground. In this case the clutch would slip and the leather would burn out. If the friction surface is made of ample dimensions to overcome the above mentioned difficulties this form of clutch is the simplest and cheapest available.

Many people object to a large diameter cone owing to its tendency to rotate too long after the clutch is freed. If this cone is made of a light alloy or a light steel stamping, I see no objection to using a large diameter clutch provided that due consideration is given to ground clearance.

With reference to Fig. 9, I think a fairly simple transmission

(Mr. Geo. Steinheil.)
system could be evolved for motor cars by using a gear-box in the differential, this gear-box consisting of three bevel gears loosely mounted on the differential casing, and being locked when necessary by suitable jaw clutches as indicated in Fig. 56. The propeller shaft would have two bevel gears fixed solid on it which would mesh with the bevel gears on the differential casing. Two of the latter are for forward drive and the third for reverse. With this arrangement four speeds forward are obtainable, and two speeds reverse. Two of the forward speeds will be direct drive from the engine and one of the reverse speeds will also be direct. Some people might object that this arrangement will give a heavy back-axle, and therefore unsprung weight, but this drawback can be overcome by using a jointed back-axle suspended from the frame, similar to that shown in Figs. 28 and 29 of the paper.

With regard to the differential, this mechanism could be easily dispensed with on light cars for all roads, and on heavy touring cars and lorries to be used on bad roads. If, however, a universal service car were aimed at, probably some sort of differential locking gear would be necessary, which would be operated from the driver's seat.

Mr. G. H. Lanchester wrote: In the portion of the paper dealing with epicyclic gears the author makes the sweeping assertion that in the past epicyclic gears "have suffered, when designed to transmit three or four speeds, from . . . weakness of construction and general complication of parts." This statement is an unfortunate one, for if there is one quality in which epicyclic gears show marked superiority over other types of gear it is in strength of construction—as evidenced by its immunity from breakdown and its durability. It is a matter of common knowledge that three-speed epicyclic gears have been fitted to every car that has been turned out from the Lanchester works during the last twenty-one years and that they have proved in every respect equal to the demands made upon them. In many cases gears have been running for upwards of ten years without repair or replacement, other than the renewal of the friction blocks by which the drums are held.

The charge of general complication of parts is largely a matter of definition. If the definition of complication consists simply and solely in the use of a greater number of parts, then it may be admitted that the epicyclic gear is complicated; but, in respect of complication, the designs suggested by the author and referred to

as Figs. 18, 26 and 27, are in no way simpler than forms of epicyclic gear existing to-day, and which have been in constant use for the past twenty years. Far from being any improvement on existing designs, his proposals are open to criticism in many points of detail. They not only involve considerable complication, but, what is worse, complication of an undesirable kind. The planet pinions in all three of his designs are carried on overhung bearings, i.e., the pinion bearings are not in the plane of the load. The bearings, moreover, are shown as plain journals without any visible means of lubricating them. Many of the elements forming portions of the gear are designed in a manner that would present difficulty in machining, and would not lend themselves to economic production.

In no case do the illustrations (which, from a designer's point

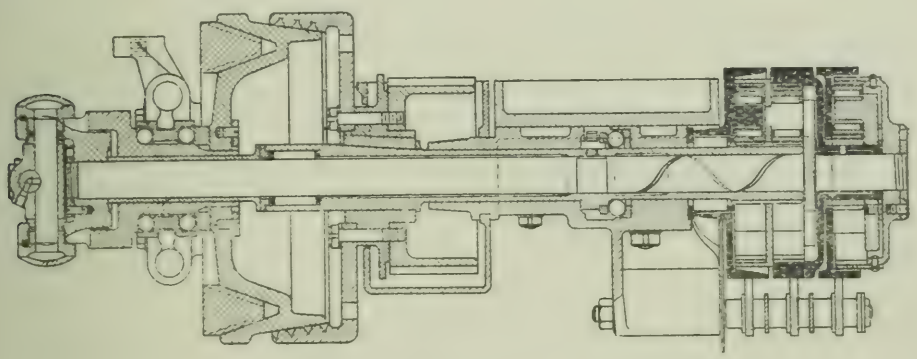


Fig. 57.

of view, are no more than diagrams) represent a type of epicyclic gear more helpful to future development of this type of gear than the epicyclic gears used in the Ford car or that designed by Dr. F. W. Lanchester in 1900, to which I have already referred.

In this gear, as will be seen from the assemblage drawing, Fig. 57, practically the whole of the gear system is on roller bearings, the bearings being in direct line with the load on the pinions. All bearings other than the roller bearings are pressure-lubricated, the oil to the roller bearings of the sun gears and to the pinions being supplied by centrifugal force through radial holes in the centre shaft and concentric elements.

Fig. 58, Plate VI, illustrates the assemblage of the present-day Lanchester three-speed epicyclic gear with direct drive clutch disk and the gear-box brake-drum.

(Mr. G. H. Lanchester.)

The employment of sliding gears, as advocated in Figs. 18 and 26, is open to grave objection:—Firstly, sliding gear requires the use of coarser tooth pitch than is desirable for epicyclic gears, since coarse pitch-teeth limit the selection of gear-ratios for given overall dimensions. Secondly, the use of sliding gear in the bowels of an epicyclic train cannot be too strongly condemned on account of the risk of tooth chipping and mortality of the gears consequent upon the mastication of the chips. It is well known that on account of the fine metallic particles and chips disengaged by sliding change-gears, plain bearings do not stand up well in the ordinary type of gear-box. If this is so in the case of a gear-box where metallic particles or chips can fall to the bottom and remain undisturbed, the conditions which exist in epicyclic gear, in which everything is rotating, will be bad indeed, and such designs as those given in Figs. 18, 26 and 27 stand condemned. Thirdly, the introduction of sliding gears complicates the design of the gear actuation, the gear-change operation mechanism having to do duty for engaging clutches for both sets of epicyclic gear trains and simultaneously to operate the sliding gear when changing over from one set of epicyclics to the other. This complication, which is not easy of solution in a sound and practical manner, has been completely ignored.

Mr. Bernard Shirley wrote*: The discussion that has taken place regarding the relative merits of the cone and disk type of clutch has been a good one, and I should like to support Dr. Lanchester in what he has said regarding the advantages of the disk type. I was also pleased to see that he called attention to the statement on page 271 that "Double-helical gears, unlike the ordinary straight spur type, give continuous rolling contact on the pitch line," as a good number of people believe this statement to be correct. Single- and double-helical gears are just as dependent on the correct tooth form to transmit uniform motion as straight teeth.

To justify this statement, a characteristic line of contact for a helical gear is *a b c* as shown in Fig. 59, *b* being the point of contact on the pitch line. Now if it is assumed that the tooth formation is in error to the extent that the width of tooth at the

* This communication has been considerably abbreviated by the Publication Committee.

top is greater than the theoretically correct width, Fig. 59a, it follows that contact will take place earlier at the point *a*, which will result in the actual contact line only extending a short distance along the face, and thus pitch line contact will be prevented.

The reason why helical gears give better results is not that continuous pitch line rolling action takes place, but that the load on each tooth is taken up gradually.

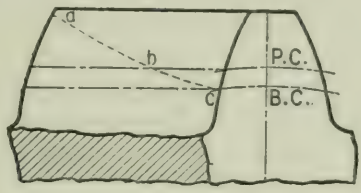


FIG. 59.

Gear Tooth Design.—Dr. Lanchester's remarks regarding the erosion of gear teeth at or near the pitch line have especially interested me. Further, this subject has not, up to the present moment, been given the consideration it deserves. My own experience has led me to a somewhat similar conclusion to that which Dr. Lanchester has come to. To give my own experience

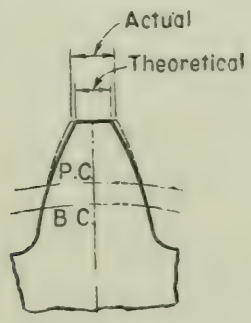


FIG. 59a.

of this important question, I was, in December, 1919, conducting some severe tests with bevel and spur gears, the loads carried were well in excess of normal loads, and with gears made in toughened steel the peculiar erosion about the pitch lines as mentioned by Dr. Lanchester was quite apparent after fairly short runs, the rapidity with which this effect was produced striking me rather forcibly. Parenthetically, I ought to state that the teeth were

of good formation, and showed a good bearing for the full working portion of the tooth. Case-hardened gears were also tested in the case of the bevel gears, and it was found that with the same severe loads, this same erosion, or breaking down of the casing, could be produced about the pitch lines, though only after a very much greater time. These remarks suggest that there is a limit to the load combined with speed that can be carried on a tooth surface for a given surface-hardness of the material employed. The curious thing is that the erosion is only apparent in the

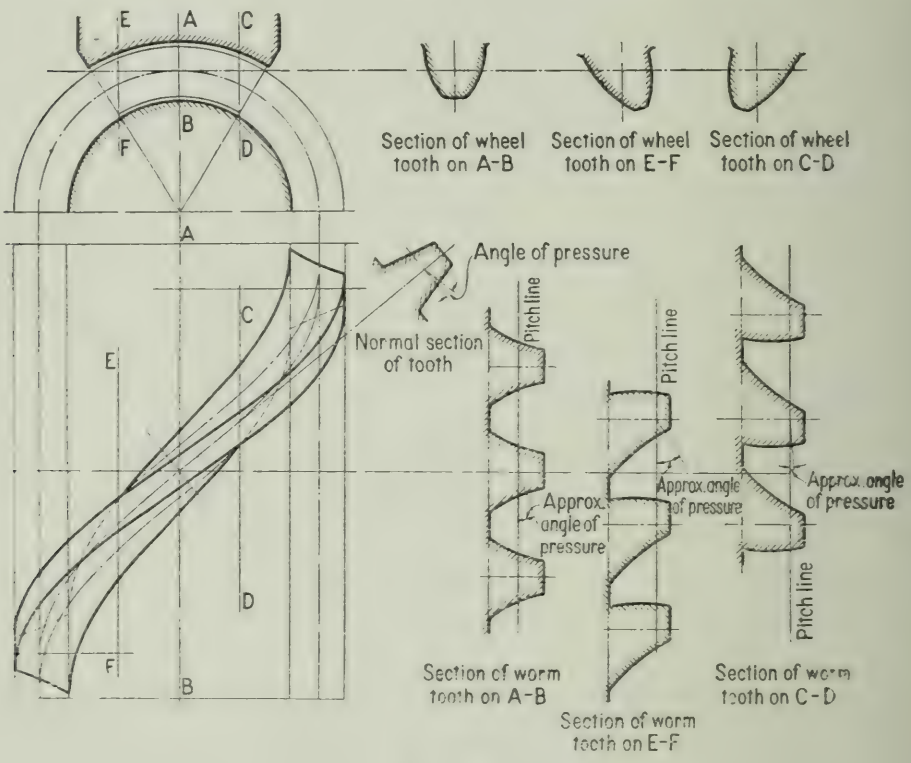


FIG. 60.

vicinity of the pitch lines, and not at the points of the teeth where the speed on the tooth surface is highest, thus tending to the conclusion that speeding up of the movement of the load on the tooth surface is quite possibly a desideratum. I may say that after conducting these tests I was at a loss to explain this phenomenon, but finally I definitely came to the conclusion that the desideratum would be so to form the tooth surfaces nominally near the pitch line as to cause movement of the load to be accelerated. Following on these conclusions I have the pleasure

of hearing Dr. Lanchester's views on this subject. I should therefore, in company with Dr. Lanchester, suggest that laboratory investigation would be desirable.

To carry this point further, I will here make reference to worm gears of dimensions suitable for automobile back-axle drives. Taking sections of the worm at right angles to the axis of the wheel, there is a variation in angle of pressure of the side of the worm thread, see Fig. 60, which is fundamental from the geometry of the worm and cannot be prevented. This angle of pressure

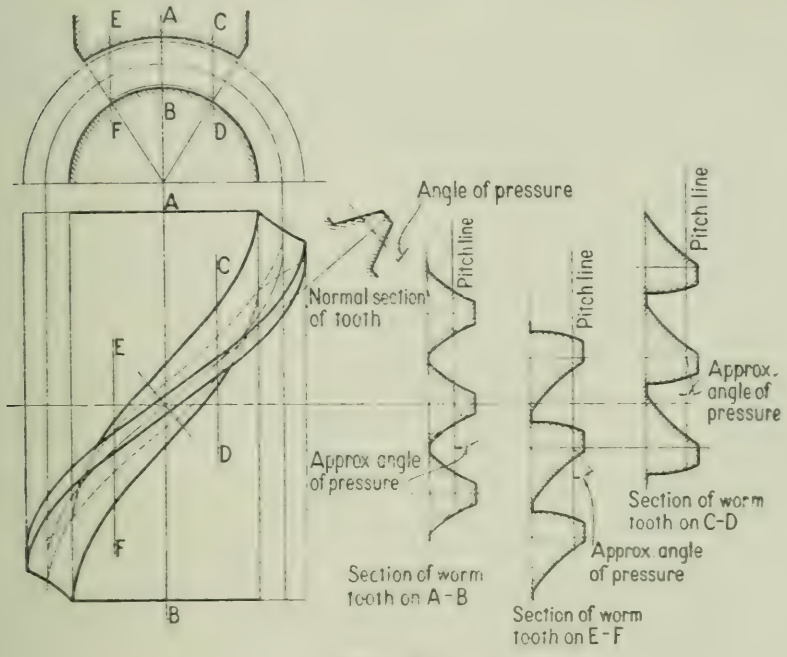


FIG. 61.

(sometimes curved) change cannot be overcome, no matter what form of tooth may be adopted. The angle of pressure at section CD will be smaller, and at section EF will be larger, than the angle of pressure adopted at section AB. The result is that movement down the wheel tooth is considerably slower, or, conversely, of longer duration on section CD than on section EF.* This angle of pressure change can be overcome to a certain extent

* This will readily be apparent when reference is made to the contact lines on the face of the wheel tooth shown in Fig. 60. The contact lines correspond to a fixed increment of rotation of the worm wheel, so that the movement at any section of the tooth surface in the given increment of time or rotation, will be the distance between the contact lines at that section of the tooth surface.

(Mr. Bernard Shirley.)

by increasing the angle of pressure at section AB, but if this is done, the angle of pressure at section EF may become too great to be of practical value, see Fig. 61.

Now here comes the comparison; the steep side of wheel tooth with relative slow movement of the load, due to small angle of pressure, produces erosion (the worm thread, which is moving across the face of the wheel tooth, does not show this same defect) while on the portion of the tooth surface in the vicinity of section EF (Fig. 60), where the angle of pressure is larger, a good bearing surface is produced. Further, the critic will probably say that any alteration that tends to increase the sliding action is detrimental to the carrying of heavy loads, but here, in the case of a worm gear, we have heavy loads carried by a drive which is essentially a sliding one, or in other words, *if it is proved that sliding across the tooth is not detrimental, why should sliding up the tooth be detrimental?*

The explanation that I put forward is as follows, and this seems to me to be a theory which is not far from the actual truth:—

1. All tooth surface action is sliding.

2. The length of an involute curve increases as the square of what may be termed the "opening out" angle expressed in radians; this means that for a given increment of angle, the length of face near the base circle is very small compared with the length of face for a corresponding increment of angle at the outside diameter of the gear, and since the outside edges of the teeth have to engage with the portion of the teeth near the base circle of the engaging gear, it will be seen that for a given relative angular motion it is possible for the extremes of tooth length increments to meet with a possible maximum of sliding action. This will be clearly understood by reference to Fig. 62, when, if two gears of even number of teeth are in mesh together, the portion of tooth surface marked 1 will have to mesh with the portion 1a on the mating gear, also portion 4 will have to mesh with portion 4a of the mating gear. The amount of sliding that actually takes place will be the difference between the two corresponding increment lengths.

3. All sliding produces friction. All friction produces heat.

4. A portion of the heat engendered is carried away by the lubricant, the remainder being absorbed by the gears.

5. The heat that is given up to the gears is produced on, and is transmitted via, the surface in contact.

6. It is reasonable to assume that the heat engendered and given up to the gears at any particular instant is absorbed, half by the one gear and half by the other gear, or perhaps more correctly in proportion to the specific heat of the material that each gear is made of (very often the same).

7. Further, it will be seen by reference to Fig. 62, that for a given increment of rotation, a given amount of sliding action, and consequently a given amount of heat, the two surfaces (for instance, comparing portion 4 with 4a) are much different in length, which means that the amount of heat given to portion 4 will be distributed over a greater length, and that portion will consequently be relatively cool as compared with portion 4a, which has all the heat concentrated almost at a point.

8. The foregoing suggests that excessive heating below the pitch

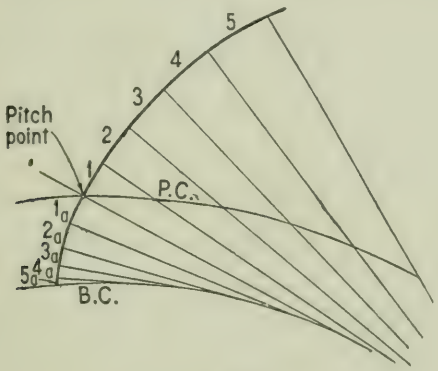


FIG. 62.

line will take place, which will possibly result in the local breaking down of the oil film and lead to the crumbling of this portion of the tooth surface when heavy loads have to be carried.

9. The conclusion to be drawn from the foregoing is that if the erosion of the tooth form below the pitch line is to be eliminated, speeding up of the movement of the load is quite possibly a desideratum even at the expense of introducing a slight increase in the amount of heat which would be more evenly distributed.

10. This theory therefore suggests that an increase in angle of pressure is more to be desired than correcting a small angle of pressure form of tooth.

Epicyclic Gear-Boxes.—Regarding the use of an epicyclic gear-box in place of the ordinary sliding gear-box, I think that Mr. G. Watson sounded the right note when he deprecated the elimi-

(Mr. Bernard Shirley.)

nation of the sliding type gear-box before something better and cheaper was discovered. That is just the problem that the designer is confronted with, and whilst I do not wish in any way to deter any one who is desirous of making use of the epicyclic gear, it is as well to take note of Dr. Lanchester's unfavourable impressions as to the type put forward by the author. It is, however, very interesting to note the rules Dr. Lanchester puts forward as being desirable, which will materially assist those who contemplate making changes in favour of epicyclic gears.

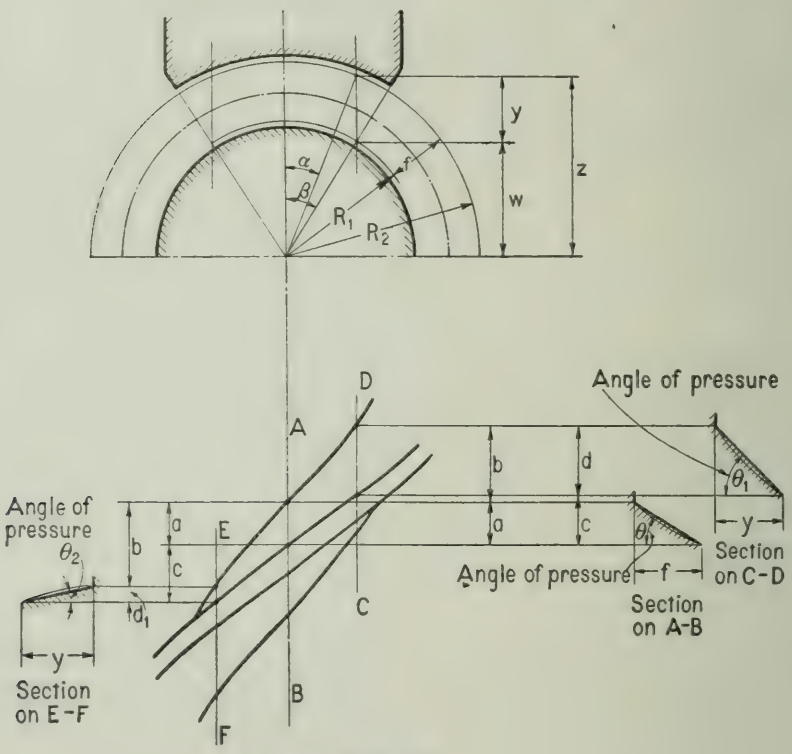


FIG. 63.

Worm Gears.—After referring to the customary practice of some manufacturers to make the worm with a rack or "involute" form of tooth on its linear section, the author asks the question (on page 291), "But what is taking place on adjacent parallel planes?" He then goes on to answer this question by stating, quite rightly, that, "The shape of the teeth of a rack-shaped worm, on one of these corresponding parallel sections, is peculiarly curved." This shape is due to the geometry of the worm itself and cannot be prevented; the alteration in the form is brought about by the

fact that the angle of the thread is continually changing in magnitude from top to bottom of the worm thread, which is occasioned by the diameter of the worm varying, whereas the lead of the worm is a constant quantity. In other words, if a worm is made with a straight side, and with a given angle of pressure on its linear section, containing the axis of the worm, it is fundamentally impossible for it to be straight sided and of the same angle of pressure on any other of the corresponding parallel sections, as referred to by the author. Further than this, no matter what form of tooth be arbitrarily chosen for the central linear section (such as the form produced by the F. J. process), a corresponding change in formation will take place.

The choice as to which plane should contain the straight line (or any other curve that may be chosen to accommodate the process of manufacture) can be made with perfect freedom, but thereafter all other sections are geometrically derived from this originating line or curve. Yet later on (page 301) the author makes the statement that uniform motion will be transmitted from the worm to the wheel "because all linear sections are identical." It is, however, as explained above, fundamentally impossible for all linear sections to be identical.

For the benefit of those who desire to see the proof of this, I introduce here Fig. 63, also the derivation of the formula for obtaining the change in angle of pressure (curvature being ignored since its amount is small, unless curvature is specifically introduced into the form).

R_2 = Outside radius of worm.

f = Working depth of thread.

θ = Angle of pressure on linear section containing axis of worm.

θ_1, θ_2, etc. = Angle of pressure on adjacent parallel linear sections.

L = Lead of worm = Linear pitch × number of starts.

X = Distance away from central plane at which angle of pressure is desired.

Then $R_1 = R_2 - f$.

$\sin \alpha = X/R_2$ from which α is determined.

$\sin \beta = X/R_1$ from which β is determined

$W = \cot \beta \times X$.

$Z = \cot \alpha \times X$.

$Y = X \times (\cot \alpha - \cot \beta)$.

(Mr. Bernard Shirley.)

$$A = \tan\theta \times f.$$

$$B = \frac{L \times \beta \text{ degrees}}{360 \text{ degrees.}}$$

$$C = \frac{L \times \alpha \text{ degrees}}{360 \text{ degrees.}}$$

$$D = A + B - C.$$

$$\therefore D = A + \frac{L \times (\beta \text{ degrees} - \alpha \text{ degrees})}{360 \text{ degrees.}}$$

and $\tan\theta_1 = D/Y$, thus θ_1 is determined.

Recapitulating:

$$\tan\theta_1 = \frac{\tan\theta \times f + \dfrac{L \times (\beta \text{ degrees} - \alpha \text{ degrees})}{360 \text{ degrees.}}}{X \times (\cot\alpha - \cot\beta)}$$

When on opposite side of central plane $D_1 = A + C - B.$

$$\therefore D_1 = A + \frac{L \times (\alpha \text{ degrees} - \beta \text{ degrees})}{360 \text{ degrees}} \text{ and } \tan\theta_2 = D_1/Y.$$

Therefore $$\tan\theta_2 = \frac{\tan\theta \times f + \dfrac{L \times (\alpha \text{ degrees} - \beta \text{ degrees})}{360 \text{ degrees}}}{X \times (\cot\alpha - \cot\beta)}$$

Proceeding further, the author goes on to state on page 292 that "the wheel teeth, on these planes, should be conjugate to suit, but this is impossible owing to interference." This is incorrect, and here it might be as well to state that the tooth shapes can be classified as follows:—

1. Wholly conjugate.
2. Partly conjugate.
3. None conjugate.

A worm wheel can be looked upon, as regards its driving side, primarily as a helical gear engaging with a "helical" rack (or a rack the teeth of which are inclined at an angle corresponding to the angle of helix of the mating helical gear), but which has its angle of pressure changing throughout its face width and at the same time having its addendum increased as both extremes of the face are approached. This will be readily apparent by reference to Fig. 60.

Further, in the same manner that a helical gear can be reduced, as regards tooth form and contact, to an ordinary spur gear, we are able to reduce a worm wheel to an ordinary spur gear, thus leaving only a gear which has a change in angle of pressure and having its addendum and depth of tooth below the pitch line modified.

Again, increasing the addendum and reducing the depth of tooth below the pitch line, that is, displacing the tooth form about the pitch line, virtually has the tendency of increasing the angle of pressure of the wheel tooth, as regards formation of tooth, but not modifying the inclination of the line of action (after the fashion of a Gleason corrected tooth). As this is of a minor character, which will be seen by reference to the diagrams already given, the result will be to either accentuate or deter the variation in angle of pressure already mentioned: thus this factor can be ignored. This, therefore, allows of the worm wheel being considered as a spur gear of which the angle of pressure is variable from one end of its face to the other: thus the whole tooth shapes can primarily be reduced to a question of effects produced by changes in angle of pressure.

In the light of the foregoing it is fairly easy to classify the tooth shapes of the various sections, bearing in mind that interference in the case of a spur gear engaging with a rack is dependent upon three factors—the number of teeth, the angle of pressure, and the height of tooth. In the case in question, the number of teeth is a constant quantity, and also the height of tooth is approximately so: the only variable in determining interference (apart from correction as explained above) is therefore the angle of pressure.

As to the extent to which these tooth shapes can effectively be made use of, it should be remembered that the whole object of making the worm wheel teeth envelope the worm is to produce the effect obtained by the use of an ordinary helical gear as distinct from a spiral gear.

Considering, then, the worm gear from the helical gear point of view, what is expected is that the load shall be taken up by each tooth gradually by overlapping of the teeth, and that the length of the contact line shall be sufficient to ensure (on any particular section) that the next tooth be in contact before the leading tooth is out of contact. All tooth sections that do not give this result, either by reason of too large an angle of pressure or by interference (both conditions have the same tendency, namely, to shorten line of action), should, if possible, be rejected. To fulfil these conditions it will readily be seen that to produce the happy medium it is necessary to carefully select the angle of pressure and height of tooth for the linear section of worm containing the axis of the worm. Another point worth noting is that no advantage in this direction is gained by having a very

large throat angle. It is also interesting to note that the F. J. type of worm gear introduces interference earlier than the ordinary type of worm gear of equivalent angle of pressure.

A few lines further on, the author says, "Hence it follows that the transmission of true uniform velocity, which is a characteristic of the involute gear, only applies to a section on the axial plane where the worm section is that of a rack." This, again, is a faulty statement, as will be readily apparent from the foregoing remarks. Further, F. J. worm gears are just as dependent upon the conjugate surface to transmit uniform motion.

The author then deals with the question of worm gear contact upon the principle of the triangle of forces. Now forces have nothing whatever to do with the contact that takes place between two gears. The subject that deals with contact is kinematics. To quote Grant, "The action of gear teeth upon each other is purely kinematic, but we cannot consider whether the material is wood, steel or wax; whether the gears are lifting one pound or one ton; or whether they are running at one revolution per second or one per day." Hence a considerable amount of this section of the paper is quite unnecessary and of no account. Also, when a worm is in mesh with a wheel the actual contact *is* contact, and is absolutely independent of the plane from which it is viewed.

Comparison of the two types of Worm Gears.—Under this heading, on page 303, the author draws attention to Figs. 45 and 46, which are apparently laid out to given formulæ, and supposed to represent the development of the lines of contact respectively for the ordinary worm gear and the F. J. worm gear. The appearance of Fig. 46 suggests that these are intended to be developed plans of the zone of contact shown on the worms in Figs. 47—48 and 49—50, but comparing Fig. 46 with Figs. 49—50 this does not appear to be correct, because the zone shown in Fig. 46 extends to 120 degrees on the right hand side of the line representing the worm axis, which is not consistent with Figs. 49—50, nor is it possible. However, it will be assumed that Figs. 45 and 46 are intended to be developed plans of the zones of contact. In this case these Figs. should resemble respectively the zones of contact shown in Figs. 47—48 and 49—50, but would be drawn out or extended in a direction at right angles to the worm axis. With the above mentioned proviso, Fig. 46 reasonably agrees with Figs. 49—50, but Fig. 45 is utterly

inconsistent with Figs. 47—48. It is, therefore, unfair to make any sort of comparison between Figs. 45 and 46.

The author states on page 307 that the worms compared are as like as possible; an objection can be cited against this, as the angle of pressure used in the preparation of the illustration of the ordinary worm gear given in Figs. 47—48 is only 15 degrees (assuming the Fig. is, as it appears to be, the same as that used

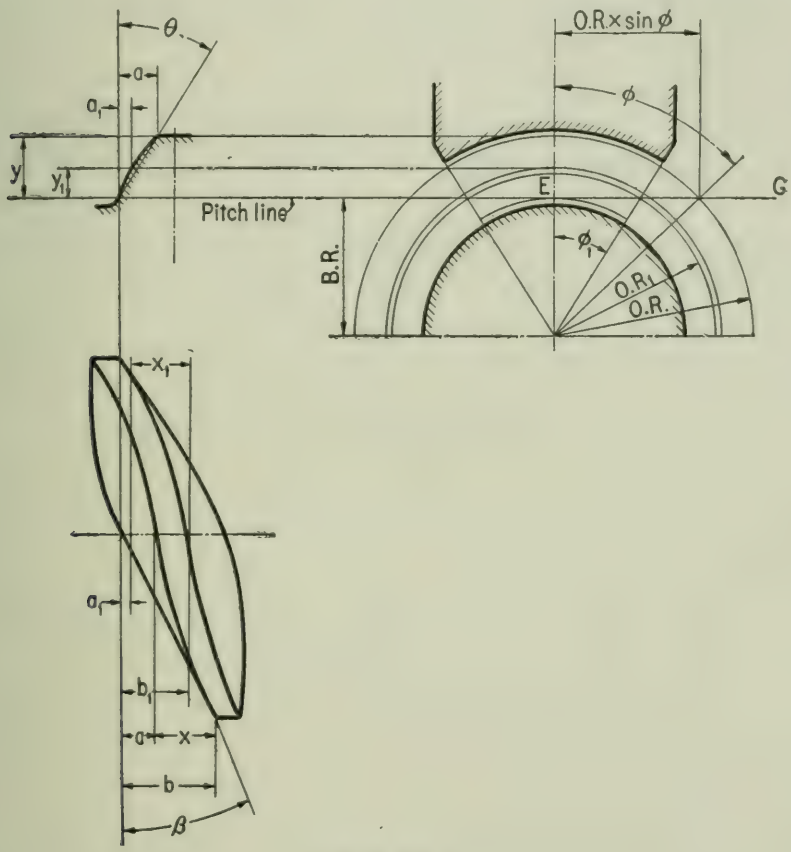


FIG. 64.

by Mr. Bostock in his paper read before the Manchester Association of Engineers in 1917). I should like to ask the author how many worm gears for automobile back-axle drives are made with an angle of pressure of 15 degrees. A common linear angle of pressure for this type of drive is 30 degrees, and it is to be noted that this angle of pressure was Messrs. D. Brown & Sons' standard prior to the introduction of the F. J. system.

In the light of this, it is interesting to investigate what angle

(Mr. Bernard Shirley.)

of pressure is obtained with the F. J. system. From the information contained in Fig. 43, the formula for obtaining the angle of pressure on the central linear section can readily be obtained. For the purpose of illustration, I will select the D. B. S. gear designation reference F. J. 45 degrees 6 inches 6/23, which answers to the following proportions:—

Circular pitch = 1·300 in.
Number of starts = 6.
Pitch dia. of worm = 2·483 in.
Outside dia. of worm = 3·724 in.
Lead angle = 45 degrees.
Lead angle (considered as an ordinary worm) = 38 deg. 40 min.
Working depth of tooth = 75 per cent of standard B. & S. = 0·6205 in.
Clearance = 80 per cent of standard B. & S. = 0·0650 in.

Fig 64 illustrates the conditions. The line EG is the generating line as in Fig. 43 of the paper.

From this diagram the following formula will be readily understood:—

$$BR = \frac{\text{D.B.S. pitch diameter of worm}}{2}$$

$$OR = \frac{\text{Outside diameter of worm}}{2}$$

$$\cos \phi = BR/OR$$

$$X = \frac{\text{Circular pitch} \times \text{starts} \times \phi \text{ degrees}}{360 \text{ degrees}}$$

$$B = OR \times \sin \phi \times \tan \beta$$

$$A = B - X$$

$$\text{and } \tan \theta = A/Y$$

where θ = Linear angle of pressure.

If it is desired to plot the whole of the tooth form, this can be done by taking different values for Y_1, and obtaining the value for A_1 from the following formula:—

$$OR_1 = BR + Y_1$$

$$\cos \phi_1 = BR/OR_1$$

$$X_1 = \frac{\text{Circular pitch} \times \text{starts} \times \phi_1 \text{ degrees}}{360 \text{ degrees}}$$

$$B_1 = OR_1 \times \sin \phi_1 \times \tan \beta$$

$$A_1 = B_1 - X_1$$

The result that will be obtained is shown in Fig. 65.

It is interesting to note the angles of pressure detailed in

Fig. 65, especially bearing in mind that the angles of pressure usually made use of for back-axle drives range about 30 degrees on the linear section. It is not to be expected that this small variation in tooth form will produce any material difference in the tooth action, apart from the fact that the working tooth form of the worm on the central linear section is all addendum, which, as already intimated, tends to introduce interference earlier. Further, if a small lead angle were to be used on this system, practically a square thread would be produced on the central linear section.

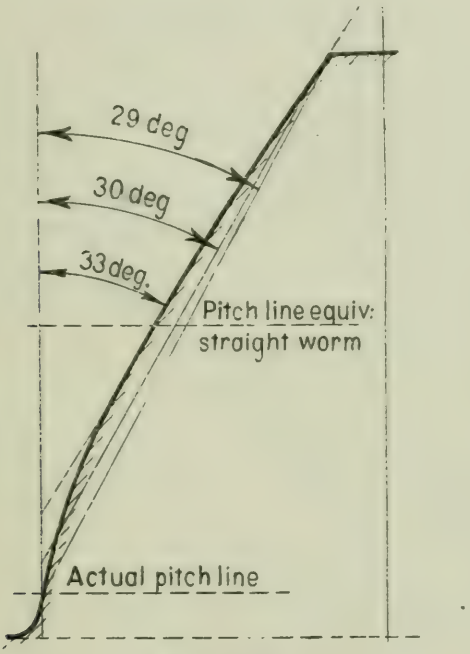


Fig. 65.

Finally, the author goes on to explain that the actual contact of the F. J. worm gear moves across the face of the wheel, that is, contact 1a—1, Fig. 49, rotates round the worm axis; this, again, is incorrect. The movement of the contact is from the top of the wheel tooth to the bottom, that is, contact would commence at 1, Fig. 49, and move down the wheel tooth to 2a; this does not in any way differ from the ordinary worm gear. Regarding the point of contact that is supposed to correspond with Fig. 34, in the first place I contend that Dr. Lanchester was entitled to interpret that the point O was the point of contact sought, as, in addition to the section of the paper in which reference is made to this

diagram being headed in a plural sense, this portion contains statements that lead to this conclusion, especially with the knowledge that it is impossible for contact to take place at point D. However, Dr. Lanchester proved that point O is not a point of contact with the F. J. worm, and reference to Fig. 66 will also prove that point D is not a point of contact. This Fig. is a linear section taken tangential to the pitch cylinder of the worm and corresponds to the plane shown in Fig. 34 containing "O," "W," "P" and "R."

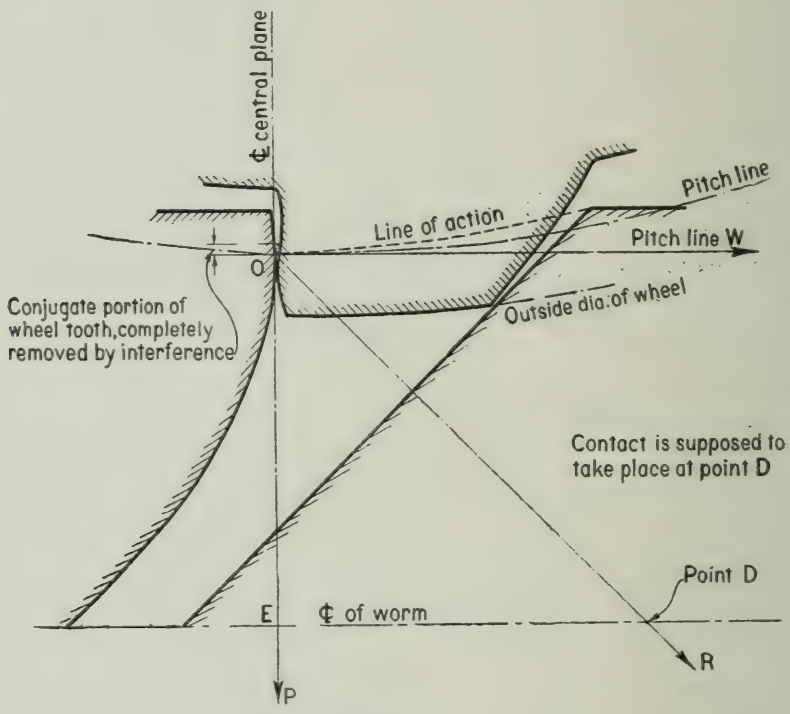


FIG. 66.

I fully indorse the remarks of Dr. Lanchester to the effect that the F. J. system is *not* designed on the principle of the triangle of forces, and that there is nothing in the F. J. method of generating that distinguishes the particular result from any other reasonable and rational basis of worm geometry.

Efficiency for Efficiency.—There is certainly truth in what Dr. Lanchester says that, efficiency for efficiency, the Lanchester gear will carry more load than the D. B. S. gear, whereas the *highest recordea* figure has been (according to published figures) obtained with the D. B. S. gear. The difference, which, however, is very

small, I attribute to the difference in lead angle of the Lanchester gear compared with the D. B. S. gear. That is to say, in other words, Dr. Lanchester's gear was primarily designed as a load-carrying gear, making this all-important, whereas the D. B. S. has been designed with the intention of obtaining the highest possible efficiency. I am of opinion that if a Lanchester gear were made of equivalent lead angle to the D. B. S. gear used in the tests, not only would the efficiency be at least equivalent to that obtained by the D. B. S. gear, but the Lanchester gear would distress; or possibly an equivalent efficiency could be obtained by also be capable of carrying more load without equal signs of reducing the width of face of the Lanchester gear in proportion to its load-carrying capacity.

Further, I am of opinion that if the D. B. S. gear can show these high efficiencies, as they have done, then any maker of the parallel type of worm gear that takes reasonable care in manufacture can do likewise.

There is, however, no doubt in my mind (and I have held this view since I made certain investigations in 1913 and 1914) that the Lanchester worm gear will carry more load, centre for centre, than any form of parallel worm gear (which statement will, of course, include the latest F. J. type) without showing detrimental effects. The relative advantages of the two types can only be settled by tests under loads in excess of those for which the gears were primarily designed.

In conclusion, I should like to state that the only difference between the ordinary parallel worm gear and the F. J. worm gear as manufactured by David Brown & Sons is that that Company have found that they can more conveniently manufacture this type of drive for automobile gears and the like, utilising a straight line in the plane already explained, and have built up their processes of manufacture accordingly. The actual working difference, if any exists, is infinitesimal. The only other point is that they make use of a worm which has all its working depth above the pitch line on its linear section, which condition, if it can be claimed as an advantage, can be obtained with any worm gear of which the centre distance is contracted the necessary amount. The only advantage is that it has practically no arc of approach: this advantage, however, hardly applies in the case of worm gears.

(Capt. Bramley-Moore.

In reply to the written communications, Capt. Bramley-Moore wrote: I have been especially interested by Mr. Shirley's written communication, and I note with particular interest his strong support of Dr. Lanchester. To start with, he insinuates that double-helical gears do not give continuous rolling contact on the pitch line, and he tries to justify his remarks by referring to a helical gear with an *incorrect tooth formation*. Does he seriously suggest that anybody would expect continuous rolling contact on the pitch line "if it is assumed that the tooth formation is in error"? Naturally, my remarks refer to gears cut with *correct tooth profiles*, and under these conditions double-helical gears, with a properly proportioned face width, most certainly do give continuous rolling contact on the pitch line. His admission that helical gears give better results than straight spurs because "the load on each tooth is taken up gradually" is only part of a larger truth. They are better, not merely on account of gradual engagement, but also because the load to be transmitted is divided up into a number of portions, each portion being sustained on separate teeth. Hence, their load-carrying capacity is greater than that of ordinary spurs, especially seeing that all phases of tooth contact are taking place at any one moment. Mr. Shirley ignores the fact that these various advantages, combined together, enable finer pitches to be employed than would otherwise be possible, which, in turn, increase the number of teeth in contact, thereby reducing vibration and rendering high circumferential speeds commercially possible. For these reasons double-helical gears have superseded straight cut spur gears in nearly all modern applications of high speed gearing.

Gear Tooth Design.—Regarding an endeavour to overcome the erosion of gear teeth in the vicinity of the pitch lines, Mr. Shirley states that he "definitely came to the conclusion that the desideratum would be so to form the tooth surfaces nominally near the pitch line as to cause the movement of the load to be accelerated." This argument, in support of Dr. Lanchester, is to the effect that a sliding action between gear teeth is advantageous rather than detrimental. In the building up of his theory, he admits that sliding produces friction, and that friction produces heat, and that excessive heat causes erosion. If, therefore, erosion is caused by heat, and heat by friction and friction by sliding, it is obvious that the erosion is due to the sliding action between the gear teeth. With this latter conclusion I entirely agree, as

distinctly pointed out in my reply to Dr. Lanchester, which Mr. Shirley appears to have overlooked. Until the introduction of modern high-speed gearing the important part played by the sliding action of one tooth over a localised portion of its mate was not realised. The strenuous conditions, however, which modern gearing is called upon to meet, in conjunction with the high circumferential speeds now required, have made it necessary for the whole subject to be thoroughly investigated, and I have dealt with this matter in detail in my work entitled "Gears and Gearing" now in the printer's hands. I know of cases of marine turbine gears which, when the teeth have been designed on standard lines, have given serious trouble through erosion. The trouble was overcome, to a large extent, by shortening the height of the teeth so as to reduce the sliding action between the gears. Similar conditions arise with heavy mill gearing required to transmit high horse-powers at slow speed, for, in these cases, the actual velocity of the sliding of one tooth over its mate is considerable owing to a large pitch being employed. I clearly indicated in my reply to Dr. Lanchester the only safe line upon which such gears can be designed, namely, care must be taken to reduce the sliding action to a minimum, which must on no account be increased as advocated by him.

Mr. Shirley brings forward an interesting example of pitting in worm wheels, and he considers that the pitting is brought about by the low rubbing velocity taking place on the leaving side of the worm wheel tooth. Fig. 45 of my paper shows a typical example of the lines of contact between an ordinary type worm and wheel, and a careful examination of Fig. 60 will show that although this worm thread has been made straight sided on the normal section, a similar formation of contact will take place, that is to say, near the central plane the lines of contact will become reversed in direction as shown in Fig. 45, where they run practically parallel and close together. In Fig. 47 I show the line of contact for a portion of this zone. It is marked on that diagram 1A, and it will be quite obvious that if the diameter of the worm wheel is large the contact 1A will commence a considerable distance away from the central plane, then travelling towards the centre. Considering the plan view in Fig. 47, it will be seen that the worm travels a quarter of a revolution while the point 1A moves to 2A, hence the actual length of worm thread passing 1A-2A is one-quarter of the length of its helix for one

(Capt. Bramley-Moore)
revolution. But during this movement the location of the point of contact on the tooth of the worm wheel has hardly moved at all, as will be seen on reference to the end elevation of this figure. Hence on the leaving side of the worm gear it will be seen that a long portion of the worm thread sweeps across a localised point of contact on the worm wheel tooth causing an excessive amount of slide.

The above clearly demonstrates that, when both members are taken into consideration, Mr. Shirley's contention that on "the steep side of the wheel tooth" (i.e., the leaving side) there is "relative slow movement of the load" is certainly not correct." We both agree that the erosion takes place on the leaving side of the gear, but this is the side where "brisk sliding action" takes place, and the erosion is due to this high rubbing action. Hence the argument that sliding is desirable for the sustenance of load again falls to the ground.

Worm Gears.—When reading one portion of my paper Mr. Shirley accepts the general interpretation that "a linear section of the worm" is a section taken through its axis, but in another place, apparently in order to introduce some irrelevant mathematical calculations, he suddenly assumes a different interpretation from that usually employed. When I have need to refer to sections other than that passing through the axis, I speak of them as "adjacent parallel planes," and I feel quite sure that the ordinary reader would not misunderstand such a simple general expression and one so commonly used.

Mr. Shirley is correct when he says that with the ordinary design of worm gear it is necessary to carefully select the angle of pressure and height of tooth for the linear section containing the axis of the worm. He goes on to say that no advantage in this direction is gained by having a large throat angle. Fig. 45 substantiates his remarks, for it will be seen that the limit for the throat angle (for the typical gears illustrated) is just over 60 degrees on the entering side and approximately 30 degrees on the leaving side. His supplementary statement that "the F.J. type of worm gear introduces interference earlier than the ordinary type of equivalent pressure angle" is, however, quite incorrect. Had he compared Figs. 45 and 46 he would not have made such a statement. Fig. 46 shows that the limit for the F.J. is over 75 degrees on the entering side, and no less than 130 degrees on the leaving side. The permissible throat

angle thus extends, on the leaving side, beyond the range of commercial practice. Mr. Shirley is therefore quite incorrect when he states that the F.J. introduces interference earlier than the ordinary type.

Mr. Shirley takes exception to my remarks regarding contact between worm and wheel teeth on adjacent parallel planes. It is quite true that if truly conjugate teeth, on adjacent parallel planes, are in contact, then uniform velocity will be transmitted. Theoretically, this condition obtains with the ordinary type of parallel worm gear. Nevertheless, my statement on lines 3 and 4 of page 292 that the worm and wheel teeth are not conjugate on adjacent parallel planes, owing to interference, is correct. Experience with the cutting of a large range of worm gear has shown that, during the hobbing process, the hob cuts away from the sides of the wheel teeth certain portions which are necessary for the transmission of uniform velocity. It is sometimes found that this interference is so great as to almost entirely remove contact from the central portion of the worm. This can be readily shown by taking a multi-threaded worm and removing all but one of its threads. If this worm be then set up in a machine, so as to engage with its mating wheel, it will be found that the contact between this single thread and the wheel teeth is not such as will provide uniform velocity transmission. This trouble is more pronounced in the case of worm wheels having a large number of teeth.

It is possible, however, to modify the ordinary type of worm gear by re-designing the wheel so that its pitch line coincides with its throat diameter. This modification entails either a reduction of the centre distance, or alternatively the addition of two extra teeth in the wheel. In either case this alteration to the design entirely alters the tooth contact, and not only enables the central portion of the worm to carry the load, but also removes objectionable contact at the extreme end.

The mathematical investigation of the contact between a worm and a wheel can be dealt with either on the lines of a system of forces or a system of velocities. They are, in a sense, interchangeable, and similar reasoning and argument takes place whichever system be adopted. I have clearly shown in my paper that the F.J. is built up on the principle of the triangle of forces, and neither Mr. Shirley nor Dr. Lanchester have been able to prove anything to the contrary.

(Capt. Bramley Moore.)

Comparison of the Two Types of Worm Gear.—Under this heading Mr. Shirley makes the sweeping statement, *without any substantiation,* that "Fig. 45 is utterly inconsistent with Figs. 47—48," and after stating that Fig. 46 "does not appear to be correct" and "is not consistent with Figs. 49—50" he subsequently states that "Fig. 46 reasonably agrees with Figs. 49—50." As a matter of fact there is, throughout the whole of the six figures

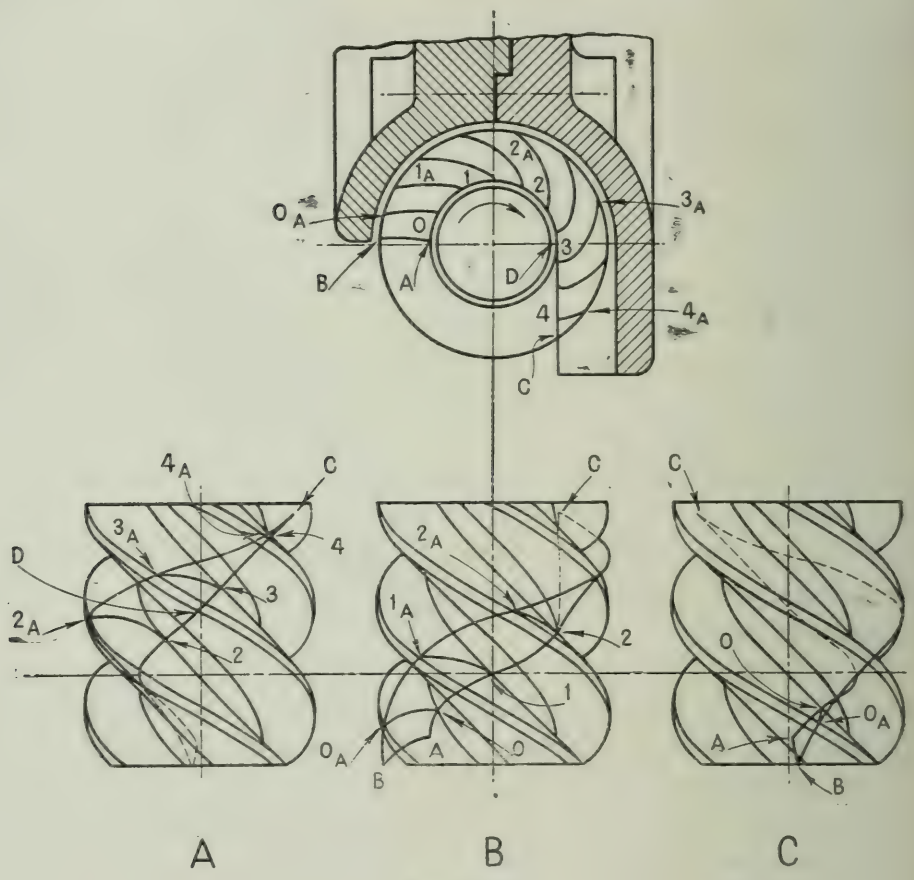


Fig. 67.

referred to, no inconsistency whatever. Figs. 45 and 46 show the full developments of the lines of contact, irrespective of the diameter or face width of worm wheels. The actual extent of the lines of contact is, however, naturally limited by the diameter and face width of the worm wheels employed. These limitations are shown in Figs. 47 to 50 inclusive, which illustrate actual concrete examples.

An examination of Fig. 67 will show the manner in which the

lines of contact extend, on the leaving side of the worm wheel, to approximately 130 degrees, as shown in Fig. 46. The top diagram shows an end elevation of a four-threaded F.J. worm with its corresponding wheel, whilst B represents a plan of the worm. These two views correspond to the two views shown in Fig. 50, but in Fig. 67 the zone of contact has been extended to correspond to the development shown in Fig. 46. In addition to the above, two side views of the worm are shown by views A and C respectively.

To consider the tooth action over the full extent of the zone of contact it is necessary to assume a wheel of unlimited face width and diameter. If such a wheel engage with the four-threaded worm in the positions shown in Fig. 67, then the driving sides of the various worm threads will make contact along the lines 0—0A, 1—1A, 2—2A, 3—3A and 4—4A respectively. If the worm be revolved in the direction of the arrow, then the line of contact will start at AB on the thread W and will travel along this thread passing successively through the various positions 0—0A, 1—1A, 2—2A, 3—3A, 4—4A, finishing at the point C on its outside diameter. It will be observed that contact, on the bottom diameter of the worm thread, ceases at the point D. After passing this point the line of contact shortens, and continues to do so until it ceases at the point C. It will be seen, in Fig. 46, that after the bottom line of contact has passed 90 degrees, it curves upwards until it intersects the outside line of contact at an angle of approximately 130 degrees. This cutting off of the zone of contact exactly corresponds to the cutting off of the tangential line DC in Fig. 67. This explanation will, no doubt, show Mr. Shirley that his remarks regarding the contact over 120 degrees are without foundation.

Mr. Shirley's remarks regarding the actual movement of the line of contact across the face of the wheel tooth are, to some extent, correct in relation to the ordinary parallel type of worm gear. Reference to Figs. 47 and 48 shows that the line of contact does move from the top to the bottom of the wheel tooth as shown in Fig 47 where the line 1—1A, near the top of the wheel tooth, takes up the position, after one-quarter of a revolution, of the line 2—2A near the bottom of the wheel tooth. In Fig. 49 the movement of the line of contact 1—1A is, however, altogether different. This line, in taking up its subsequent positions 2—2A and 3—3A, clearly moves *across* the face of the wheel tooth, following the

(Capt. Bramley-Moore.)

direction of the rotation of the worm, and *not* from top to bottom as imagined by Mr. Shirley. His contention that the point of contact marked 1 in Fig. 49 moves to the point 2A shows that he has entirely misunderstood the nature of this contact, for it is obvious that the point 1 moves to 2 and the point 1A to 2A. There is no need to enlarge upon this, as it has already been clearly explained in connection with Fig. 67.

Mr. Shirley further contends that Dr. Lanchester, when referring to Fig. 38, "was entitled to interpret that the point O was the point of contact sought for," and he further endeavours to support this mistake by going on to say that it is impossible for contact to take place at the point D, and tries to prove this in a vague sort of way by reference to Fig. 66. But this figure is no

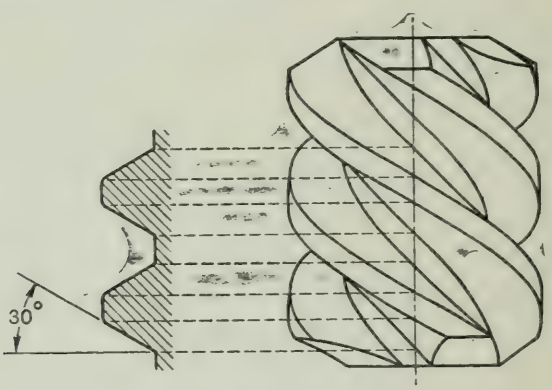


FIG. 68.

proof that the point of contact is not at the point D. It only proves that it is not where it should not be, that is to say, at the point O in Fig. 66. The point D in Fig. 67 corresponds to the point D in Fig. 38, which Mr. Shirley contends cannot be a point of contact. But I have already shown that the line of contact begins at AB on any particular thread (see Fig. 67) and terminates at the point C, *thus passing through the point D.*

Mr. Shirley objects to my statement that the worms shown in Figs. 47 to 50 are as like as possible. He assumes that the pressure angle of the worm shown in Figs. 47 and 48 is only 15 degrees, and asks how many back-axle drives are made with such a pressure angle. Very few, if any, for such a low pressure angle would cause excessive interference. Thirty degrees is, of course, the usual linear pressure angle, and for this reason I have naturally enough drawn the ordinary parallel worm to correspond

with standard practice. The worm shown in Figs. 47 and 48 has certainly not got a 15 degrees linear pressure angle, but one of 30 degrees, and to prove this beyond any possibility of doubt I have now drawn a linear section of this worm, as shown in Fig. 68, which is a projection of the worm shown in Fig. 47.

With reference to Mr. Shirley's remarks regarding Fig. 65, anyone who is accustomed to design gear teeth knows perfectly well that a variation in tooth form, however small, causes a corresponding variation in the line of action. The profile of the

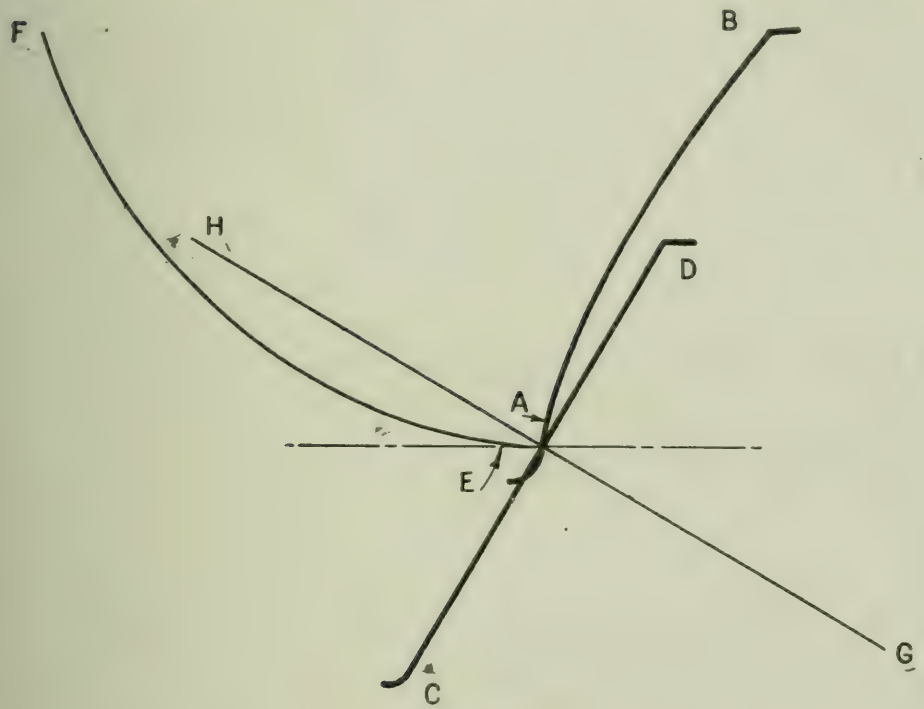


FIG. 69.

F.J. worm thread, on a linear section, is curved, as shown in Fig. 65, whilst ordinary worms are straight sided, and moreover the position of the working depth of the thread of the F.J. worm in relation to the pitch line is quite different from that of the ordinary type. The amount of alteration is shown in Fig. 69, where AB represents the profile and position of an F.J. worm thread, and CD the same for an ordinary type of worm thread. Surely, even a superficial glance will show that there must be a great difference between the tooth actions of such dissimilar threads. That this is so is clearly emphasised by the lines of action EF and GH for F.J. and ordinary types respectively.

(Capt. Bramley Moore.)

This alteration in the profile and position of the F.J. worm thread actually causes an immense difference in tooth action, and this is clearly illustrated by the developments of the lines of contact in Figs. 45 to 50 inclusive. It is difficult to comprehend how anyone who professes to understand the art of designing and generating gear teeth could make the amazing statement that between these two distinct types of worm gear "the actual working difference, if any exists, is infinitesimal." The immense difference between these two types is plainly and unmistakably shown throughout the latter portion of my paper.

Efficiency for Efficiency.—I do not propose to deal with the relative merits of the Lanchester and F.J. types of worm gear. Both have been officially tested, at varying speeds and loads, by means of the same apparatus, by the National Physical Laboratory. The official reports for both types have been published, and those interested can refer to these documents, make their own comparisons and draw their own conclusions.

The last paragraph of Mr. Shirley's communication contains a rash and misleading assertion. He speaks without any knowledge of the circumstances which led to the design and manufacture of the F.J. worm gear. There are only two people who are competent to express an opinion regarding this matter, and those two are Mr. Bostock and myself. Many years ago we were both aware of the limitations of the ordinary type worm gear, and I am able to state definitely that this new type was originated with the sole idea of providing uniform velocity transmission, and not for the purpose of providing a gear which could be more conveniently manufactured. Mr. Shirley is, indeed, very wide of the mark, for it so happens that the F.J. gear was originated by Mr. Bostock from mathematical considerations pure and simple. *The method of manufacture was a subsequent development*, and I am able to speak with authority as I witnessed its growth from its theoretical conception on paper to its final stage of machine shop manufacture. I thank Mr. Shirley for his information that the F.J. worm gear "has been designed with the intention of obtaining the highest possible efficiency." In one sense he has no right to make such an assertion, seeing that he knew nothing whatever about its inception or development, but in another sense he is certainly justified, seeing that his words are fully confirmed by the results which have been obtained at the N.P.L. and elsewhere.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

ANNUAL DINNER.

JANUARY 26TH, 1921.

The first Annual Dinner of the Institution, which was postponed from October 27th on account of a threatened railway strike, was held at the Royal Automobile Club on January 26th, and must be put down as a conspicuous success to which many things contributed. In the first place, the list of guests indicated a recognition on the part of Government Departments and the other motoring organisations of the position to which the Institution has attained. Among these guests may be mentioned:—

Lt.-Gen. Sir TRAVERS CLARKE, Quartermaster-General.
Maj.-Gen. Sir EVAN CARTER, K.C.M.G., C.B., M.V.O., Director of S. and T., War Office.
Sir J. A. F. ASPINALL, Consulting Engineer, Ministry of Transport.
Hon. Sir A. STANLEY, G.B.E., C.B., M.V.O., Chairman, R.A.C.
Sir G. E. P. MURRAY, K.C.B., Secretary, G.P.O.
Mr. W. CLARK, J.P., Master Cutler.
Mr A. S. MAYS-SMITH, President, S.M.M.T.
Capt. RIALL SANKEY, President, Inst. Mech. E.
Sir FREDERICK BLACK, President, Inst. Petroleum Technologists.
Col. L. SIMPSON, C.B.E., D.S.O., Ministry of Transport.
Mr. L. PENDRED, Editor, "The Engineer."
Sir J. ORDE, Secretary, R.A.C.
Mr. H. WYATT, Secretary, A.B.M.M.
Mr W. M. LETTS, C.B.E.
Mr. W. L. FISHER, Editor, "Automobile Engineer."

Secondly, the speeches were few in number and commendably brief, but at the same time they were of a very high order of excellence. Mr. Mays-Smith, the President of the S.M.M.T., laid great stress on the necessity for co-operation between the various motoring bodies, especially between the commercial side, as represented by his Society, and the technical and designing side, as represented by the Institution, the importance of which he

emphasised by announcing that the Society were prepared to make a grant of £2,000 per annum for a term of years to the Institution for the purpose of enabling that body to undertake more work in the direction of investigating and discussing the many problems with which the Industry is faced to-day. He also put forward an urgent plea that all the men on the technical side of the manufacturing firms should join the Institution, and not only that, but that they should make every effort to attend the meetings and take an active part in the discussions at those meetings. He instanced the medical profession, pointing out the great value which had resulted to the public by the co-operation which was found among the members of that profession. A very practical little point he made was that the automobile engineer is the man to decide whether a part should be designed to give 100 per cent efficiency and cost 30*s*. or whether it would be a better commercial proposition if the part were designed to be only 80 per cent efficient but to cost only 10*s*., bearing in mind that such a reduction in cost would make all the difference between commercial success or failure when dealing with a large output, while not of such great importance in the case of a small turn-over. Mr. Mays-Smith stated that it was just such little points that were the real core of the problem of mass production.

Sir Henry Fowler's speech in reply also dealt with the need for co-operation, and he gave a very interesting sketch of his experience in the early days of automobile history, concluding his remarks in a tone of optimism as to the ability of the automobile engineer to overcome the difficulties which appear to lie before him to-day.

Brigadier-General Bagnall-Wild, in proposing the toast of "The Guests," gave a very happy series of lightning sketches of the many distinguished men present, to which Sir Arthur Stanley replied in his best form in a speech which was accompanied by a continuous chorus of laughter and applause.

A short but very excellent musical programme was interspersed between the speeches, and the "Dismiss" was sounded at an early hour.

In conclusion, it may not be out of place to quote the following extracts from the Press: "To complete one of the most successful functions of the kind," and "As a social function the I.A.E. Dinner was a success. As a business meeting it left little to be desired, and it is hoped that there will be many happy returns of the event."

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

EXTRAORDINARY GENERAL MEETING.

An Extraordinary General Meeting of the Institution was held at the Institution of Mechanical Engineers on Wednesday, February 9th, in order to consider the proposed alterations in the Articles of Association, so that the conditions attached to the grant from the S.M.M.T. could be complied with.

The following alteration in the Articles of Association was than proposed by Mr. CLARKSON and seconded by Mr. WATSON:—

"(1) That, in addition to the class of Members already provided for by the Articles of Association of the Institution, an additional class be added, to be known as 'Affiliates': consisting of persons nominated by affiliated firms, as provided by the proposed new Article 13a.

"(2) That the Articles of Association of the Institution be accordingly altered by deleting from Articles Nos. 4, 5, 6, 7, 23, 26, 27, 30, 31, 32, 40, 55, 56, 57, 67, and 69 the word 'or' or the word 'and' where either of such words occur immediately before the word 'Associate' or the word 'Associates'; and by adding after the word 'Associate' or 'Associates' in each such Article the words 'or Affiliate,' 'or Affiliates,' 'and Affiliate' or 'and Affiliates' as the context shall require.

"(3) That the following Articles be added to the Articles of Association of the Institution, viz.:—

"(13a) An Affiliated Firm shall be a firm, company or individual engaged in the manufacture or sale of self-propelled vehicles or of parts or materials to be incorporated therein, or who shall be otherwise interested in the objects of the Institution.

"An Affiliated Firm shall be entitled to nominate one

member of their staff as an Affiliate for each payment of four guineas per annum to the funds of the Institution, provided always that such Affiliate shall be approved by the Council of the Institution.

"An Affiliate shall be entitled to attend all Meetings in London and the provinces, the Summer Visit and all visits to works, to receive the 'Proceedings' and all other publications, to use the Library and to all other privileges of membership except that of voting and the use after his name of the authorised abbreviations of the Institution as set out in Article 8.

"An Affiliate shall not be required to pay an entrance fee.

"(33a) The Council of the Institution shall have power from time to time, if, in their opinion, it is desirable to do so, to co-opt as additional members of the Council not more than three persons, who shall be nominated by any other Society, Corporation or Body which shall have kindred interests with or further or assist the objects of the Institution. Such additional members shall hold office for such period and on such terms as may be agreed upon by the Council and the nominating body."

The PRESIDENT moved the omission of the words "need not be members of the Institution and," as the solicitors advised that the Board of Trade would insist upon all members of Council being members of the Institution. The amendment was seconded by Col. SMITH and accepted by the proposer and seconder of the resolution.

A very full discussion followed in which Col. A. E. Davidson, Lt.-Col. D. S. Kennedy, Mr. H. G. Burford, Mr. C. H. Bown, Mr. A. E. Berriman, Mr. R. W. A. Brewer, Mr. Max Lawrence, Dr. W. R. Ormandy, Lt.-Col. T. B. Browne, and Lt.-Col. W. A. Bristow took part.

The resolution as amended was carried with only three dissentients.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

FEBRUARY, 1921.

The Seventh Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1, on Wednesday, February 9th, 1921, at 8 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following list of Members who had completed their election since the previous Meeting was posted up:—

MEMBERS.

THOMAS MARTIN CONROY (Dudley).
CHARLES JULIAN ERADE (London).
CHARLES BROADBENT PERKINS (Sheffield).

ASSOCIATE MEMBERS.

FULLERTON GEORGE GORDON ARMSTRONG (Beverley).
JAMES ALBERT ROBERT BARKER (Lincoln).
FREDERICK ARNOLD BEST (Sheffield).
FREDERICK JOSEPH WARR HEDGCOCK (London).
JAMES SKINNER INGRAM (London).
HUGH PATRICK MCCONNELL (London).
GEORGE PRESCOTT (Croydon).
WILLIAM JOHN REID (Gloucester).

GRADUATES.

CLIFFORD HENRY KEMPSTER (Bristol).
GODFREY YATE PITTS (Liverpool).
GEORGE TAYLOR (Leeds).
EUGENE NICOLOVIUS VARIAN (Walthamstow).
THOMAS YULE, JR. (Liverpool).

PROBATIONARY GRADUATES.

ERNEST DUNCAN BROWN (Birmingham).
LANCELOT WILLIAM BROWN (Margate).
SYDNEY EDWARD EKERS (Birmingham).
DENNIS WILLIAM CLULIE JONES (Birmingham).
GEORGE MEADS (Birmingham).
JOHN HENRY TYMONS (Birmingham).

TRANSFERS.

WILLIAM FERRIER BROWN (Coventry), Associate Member to Member.

HERBERT BERGER BENNY (London), Graduate to Associate Member.

CYRIL MAURICE SIMPSON (Coventry), Graduate to Associate Member.

JOHN LIONEL HANMAN (London), Probationary Graduate to Graduate.

ARTHUR JAMES LIDSEY (Derby), Probationary Graduate to Graduate.

The following paper was then read and discussed:—

"The Design and Manufacture of Pneumatic Tyres," by Colin Macbeth.

There were present 120 Members and visitors.

DESIGN AND MANUFACTURE OF PNEUMATIC MOTOR TYRES.

By COLIN MACBETH.

Functions of a Pneumatic Tyre.

In discussing the design of pneumatic motor tyres, it is well to consider broadly their functions. This can be done under six main headings, which, in the order of their relative importance, are as follows:—

1. *Comfort.*—To allow of high speeds on poor surfaces such as can only be obtained when using wheels having a yielding periphery.

2. *Safety.*—To provide a sure grip on all conditions of road surface when driving, but particularly when accelerating, braking and cornering.

3. *Reliability.*—To provide means of protecting the air container from penetration by road obstacles and consequent loss of pressure.

4. *Durability.*—To give long mileage and only to fail from fair wear and tear.

5. *Handiness.*—To be reasonably easy to fit and take off when required.

6. *Efficiency.*—To absorb the minimum amount of engine power.

Tyres must fulfil the above requirements in all climates and on all conditions of surface, and when subject to overloading and under-inflation.

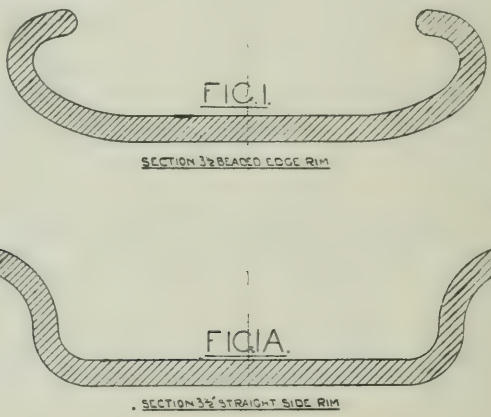
The conditions under which tyres are used vary so widely, and the abuse to which they are subject is so very great, that a high factor of safety must be provided in order to ensure that they can withstand very rough usage, and it is not possible to treat the question of tyre design on the same basis as metal parts can

be dealt with in machine design, namely, by merely specifying the "strength" to be incorporated in the various parts of the tyre.

Features limiting Scope of Designer.

The tyre designer has not a free hand to-day when instructed to design a certain size of tyre or line of tyres, for while he is unhampered in quite a number of directions, the limiting features can be well summarised as follows:—

(a) Type and size of rims on to which the tyre is to fit.
(b) Competition to be met.
(c) Loads to be carried.
(d) Clearance to be maintained.



Figs. 1, 1a.

(a) *Type and size of rims on to which the tyre is to fit.*

To-day the shapes of common rim sections have been reduced to two—the beaded edge, or B.E., type, Fig. 1, and the wired-on, or W.O., now generally called straight sided, or S.S., type, Fig. 1a. The tyre designer must be sure that his tyres fit standard rims without trouble.

(b) *Competition to be met.*

Car manufacturers and users all wish to get the best tyre at the lowest price, and for this reason a given size of tyre made up from first-class materials to be manufactured on a competitive basis must conform closely to the general dimensions adopted

by tyre manufacturers to-day. This fact in itself means that the tyre designer must work closely to established sizes.

(c) *Loads to be carried.*

These are well established for the various sizes of tyres, and a certain size of tyre therefore is sold to carry a given load; the necessity for meeting competition prevents oversizing of tyres. In this respect the tyre manufacturer is largely in the hands of the chassis makers, namely, he sells a tyre to suit the particular size of rim standardised by the chassis maker for his car.

(d) *Clearance to be maintained.*

The design of a given size of tyre cannot vary materially from common practice as regards outside dimensions without trouble arising due to the reduction of clearance or over-gearing.

As an example showing how the designer is hampered, take the case of one who wishes to make a tyre which would compete with other makers as regards:—

Quantity of materials in tyre;
Rim to be fitted;

but wishing to provide for:—

Larger sectional width than usual;
Reduced number of plies;
Reduced air pressure;
Better cushioning effect.

Such a tyre would require unusual clearances and reduced back-axle gear-ratios.

Details allowing Scope to the Designer.

While it is necessary for the tyre designer to conform to a specification within fairly narrow limits, yet there are certain details in reference to which he has a free hand. These can be summarised as follows:—

A.—*Contour and construction of bead portions within the limitations of rim on which tyre will fit.*

With a given type of rim the actual shape of the tyre at the point of attachment can vary considerably according to the ideas of the tyre designer and still ensure a good fit.

B.—*Constructional design of casing walls.*

These may be composed of any suitable material, although cotton is now almost universally used.

The usual construction is to make up the body of the casing of a number of superimposed plies of rubbered cotton material, each ply being suitably insulated from adjacent plies, and the outer part of the casing being suitably protected from the weather.

Fig. 2 shows a $3\frac{1}{2}$ in. tyre with a casing having four plies of square woven canvas, the thickness of each ply in the finished

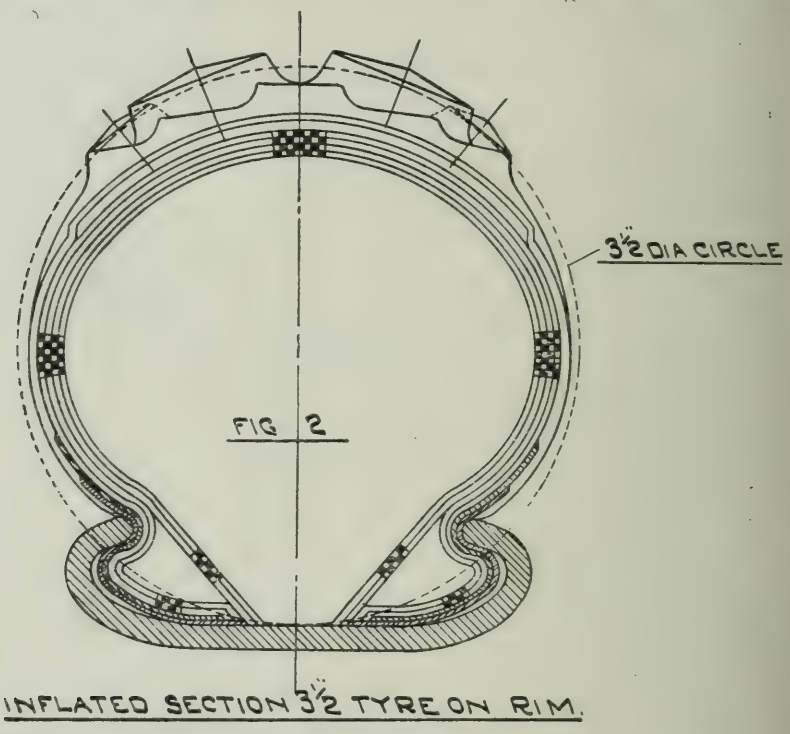


Fig. 2.

casing being approximately 1 mm. The load coming on this casing, dealing with it as a pressure-containing vessel, with an inflation pressure of 50 lb. per sq. in., equals 64 lb. per sq. in. of section measured along the bead circumference. This gives a load on each ply of 16 lb. per in. width. It is actually possible for such a casing to withstand an internal pressure of 400 lb. per sq. in., so that we have a pressure-containing vessel with a factor of safety of eight to one, reckoning on tensile strength of the plies as the basis on which strength against bursting is calculated.

An essential point necessary to ensure a good casing is, not that it shall be very strong merely as a pressure-containing vessel, but that it shall remain a sound casing, without separation of the plies or fracture of the threads in the plies due to failure of the insulation between the plies or between the threads, after many thousands of miles usage. It is also essential that the casing shall be flexible and thus able to efficiently absorb blows from obstacles on the road and to withstand the continual flexing action due to ordinary running conditions. These requirements can only be fulfilled with a properly designed canvas which has been property treated and assembled in proper condition as regards equality of stretch and disposition of threads in conjunction with suitably proportioned mixings.

C.—*Pattern and general proportions of tread surface and its foundation on casing.*

There are, perhaps, wider variations between the practice of different manufacturers at the tread portion than at any other point, both as regards the design of the tread surface and the type of foundation for the tread in order to ensure proper adhesion to the casing. On these two points the tyre designer has a pretty free hand so long as the tyre gives good results on the road and has a pleasing appearance.

D.—*Textiles used—their quality, disposition and type of weave.*

Tyre manufacturers have an absolutely free hand as to the quality of cotton used, the method of incorporating it in the casing, and the method of uniting the threads, whether by weaving in the ordinary way into square fabric or the use of what is commonly known as "cord fabric," or any other method of ply formation which may be found suitable.

E.—*Rubber mixings used—their characteristics.*

The manufacturer is not hampered in any way as to the mixings he chooses to use in his tyre providing they give satisfactory results: the main lines on which variations occur are summarised as follows:—

E1. Hardness.
E2. Degree of vulcanisation.
E3. Disposition.
E4. Volumetric and area costs.

These points can be dealt with separately as follows:—

E1.—*Degree of hardness.*

This is varied according to the particular part of the tyre in which the mixings are incorporated—thus, bead cores, which require to be of a more or less inflexible nature according to the type of tyre used, are generally of a vulcanite or semi-vulcanite type, being fairly hard and unyielding, so that friction or movement between the core and adjacent parts of the tyre cannot take place.

Casing rubbers must be very elastic and yielding, and are required to insulate one ply from another and to insulate even one thread from adjacent threads. They must also protect the casing from damp or dirt, whether from the outside inwards or from the inside outwards.

The mixings used in the casing for insulation purposes are, perhaps, the most vitally important, as they affect very materially the life of the tyre as a pressure-containing vessel.

Tread rubber.—These rubbers must:—

(a) Be yielding and still tough.
(b) Resist cuts.
(c) Resist abrasion.
(d) Resist the action of high temperature from exposure to direct rays of the sun and of low temperature from frost.
(e) Resist decomposition by oil and other injurious substances.

Cushion rubbers.—Another important place for which special mixings are required is at the point of attachment of the tread to the casing. On the efficiency of the mixings at this point depends largely the question of maintaining a proper junction between the tread and the casing. If once the junction of tread and cushion rubbers, or of cushion rubber to the casing, fails, there is a probability of early failure due to an extension of the separated portions eventually causing a failure of the casing itself.

E2.—*Vulcanisation.*

The life of the tyre depends on correct vulcanisation at all points. Thus, if any portions are over-vulcanised they are liable to deterioration due to cracking if exposed to the atmosphere, and to internal breakdown due to excessive hardness. If under-

vulcanised, the portions affected are liable to excessive distortion and early breakdown due to separation of adjacent parts.

E3.—*Disposition.*

An insufficiency of rubber at one point, such, for instance, as poor insulation due to too little rubber between adjacent plies, will lead to breakdown due to lack of adhesion, causing loose plies and internal friction and heating. Failures due to this cause should not be confused with separation troubles due to tyres run for long distances under-inflated, for such a practice causes ply separation at the tyre shoulders although the tyre itself may be sound in every way. On the other hand, too great a quantity of rubber at one point, for instance, an excessive thickness of rubber at the side walls of a tyre, will tend to cracking of the surface, and will also reduce resiliency.

If cushion rubbers and the tread portions are wrongly proportioned, they will be liable to cause separation due to too great or too little thickness, as the case may be. The best proportions of tyres at this point depend very largely on the design of tread adopted and the load carried, and no fixed proportions can be laid down to suit all cases.

E4.—*Volumetric and area costing.*

In taking out the cost of a tyre, it is necessary to reckon on the cost of materials to fill a given space and to cover a given area.

Purchasers of tyres often think that a certain tyre must be better because they are getting more weight for their money, whereas in actual practice the best qualities of mixings generally weigh less per cu. in. than poorer qualities, which make use of possibly cheap but heavy mineral matters.

On a competitive basis, therefore, it is advisable to reckon on the basis of cost per cu. in. for mixings, though as regards textiles cost should be reckoned per superficial foot of materials used.

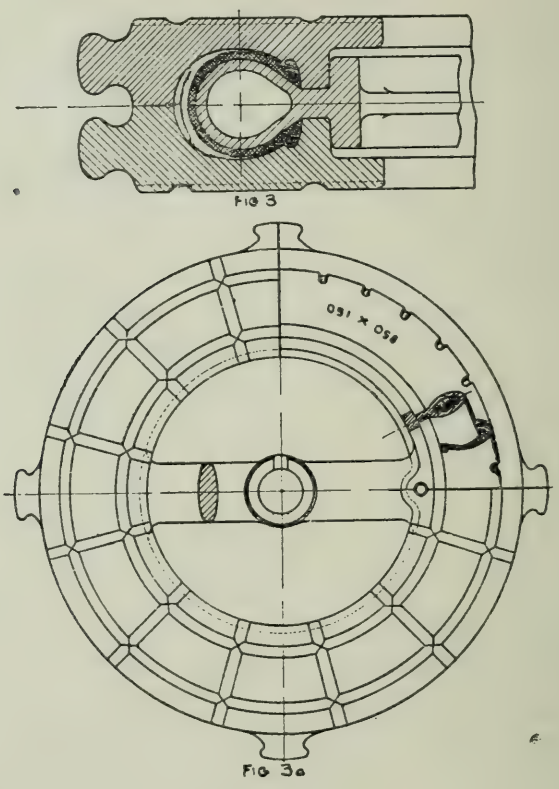
F.—*Metal parts—their quality and disposition.*

In the case of tyres making use of metal parts, such as steel studs or wire insertion in the beads, it is very necessary to use materials of suitable nature. For instance, steel studs in non-skid tyres require to be very hard on their surfaces but soft on the portions which are riveted to the washers, while the wire used

in beads of S.S. tyres requires to be very ductile, and yet to have sufficient temper to retain its shape without permanent set when subjected to rough usage.

Detail Design is dependent on Manufacturing Systems.

There are many different ways in which a tyre can be made while still having the same general construction in the finished



FIGS. 3, 3a.

article, and while still having the same shape of tread, casing, bead, etc.

Although motor pneumatic tyres have been made for upwards of a quarter of a century, there are, perhaps, more good ways of making tyres to-day than at any previous date in the history of tyre manufacture, and no one way can be stated to be the best or to have become accepted as the best.

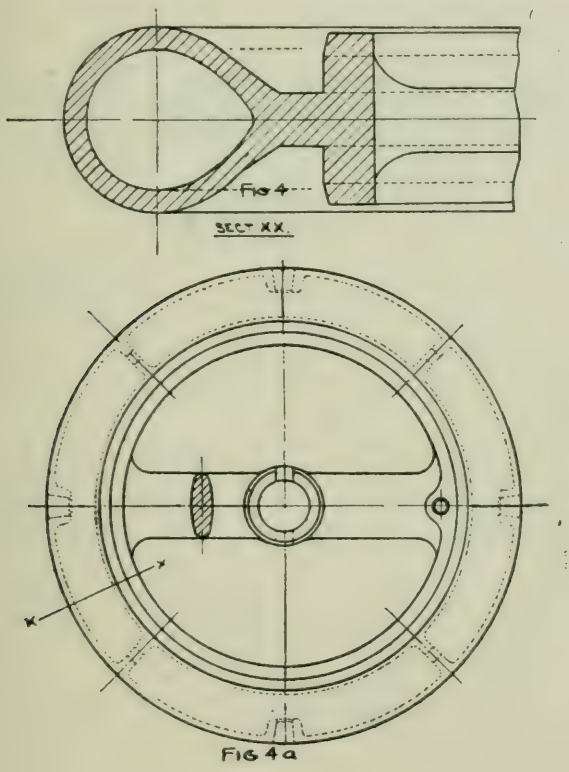
Before dealing with the design of a new tyre or line of tyres, it is necessary that the designer should know the broad methods

of manufacture to be employed, particularly those appertaining to:—

(1) The moulding system to be used.

(2) The curing system to be used.

Although a tyre is a combination of assembled units, such as bead, casing and tread, which do not vary materially in the raw state, yet methods of uniting them into a complete tyre are many and varied; the broad divisions of the more usual methods of moulding and vulcanising may therefore be outlined as follows:—



Figs. 4, 4a.

Moulding Systems.

1. External pressure system, using shaped casings.
2. Internal pressure system, ditto.
3. Drum system using flat casings.

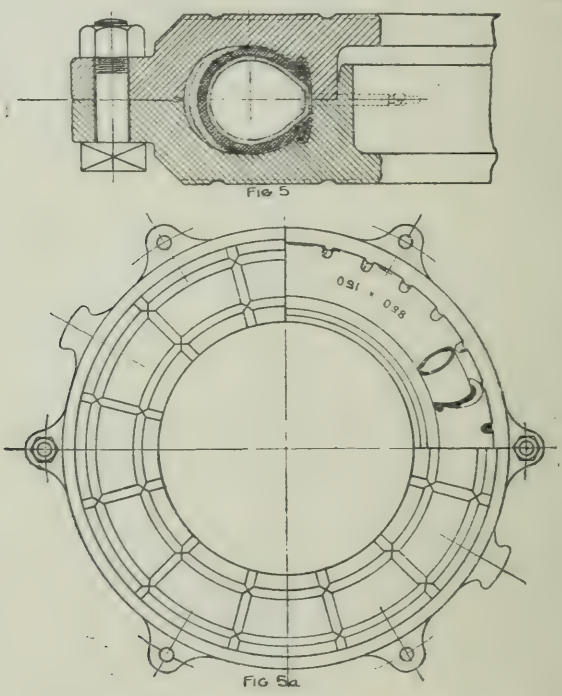
1.—*External pressure system.*

The casing is vulcanised approximately to its final shape on a metal core, and moulds are clamped on to the casing by external

pressure during vulcanisation. Figs. 3 and 3a show an assembly of the core with the tyre on it enclosed in the two halves of a mould. Figs. 4 and 4a show details of the core. This system requires great accuracy in tyre building and moulds, but provided a proper system is adopted the method is very good.

2.—*Internal pressure system.*

Here the shaped casing is vulcanised approximately to its final shape and is pressed outwardly against a mould by means of an



Figs. 5, 5a.

expanding tube or other pressure-containing means within the casing. This does not entail a close working to gauge and volumes, but care is required to prevent overstretch of the casings and to prevent undue cost of the air-containers used, see Figs. 5 and 5a.

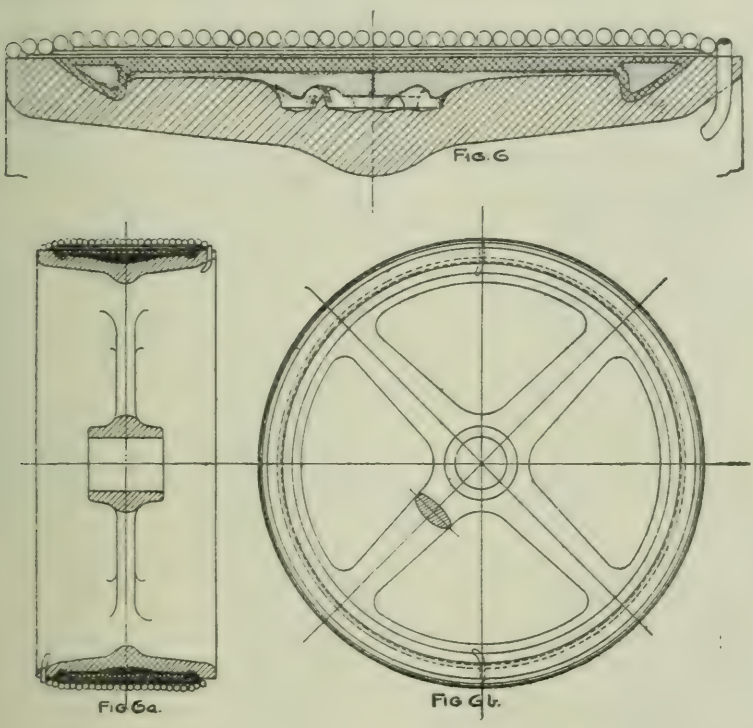
3.—*Drum system.*

In this system the casing is moulded flat-wise on a drum by means of internal or external mechanical pressure, its final shape

being obtained at a later stage. This system is not regularly used except for very small sizes of tyres. Figs. 6, 6a, and 6b show the well known wire and canvas "lapping" system, and Figs. 7, 7a, and 7b show a special expanding flat sectional press.

Vulcanising Systems.

The vulcanising system employed may vary considerably, irrespective of the method of moulding adopted. Broadly, the vulcanisation systems are those which:—



Figs. 6, 6a, 6b.

Either vulcanise the casing unit and tread unit apart one from the other and later vulcanise them together, or

Vulcanise these units at one time as a whole.

These can be summarised as follows:—

1. Two-cure two-piece system.
2. Two-cure one-piece system.
3. One-cure one-piece system.

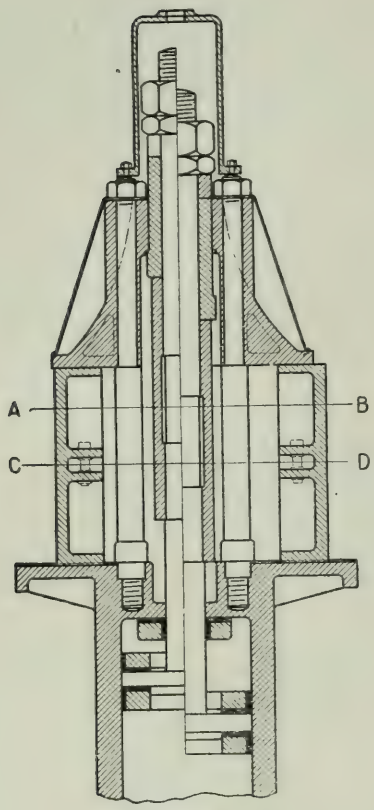


FIG 7a

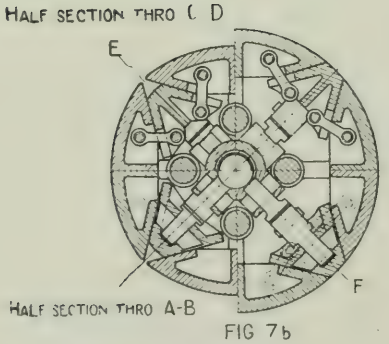


FIG 7b

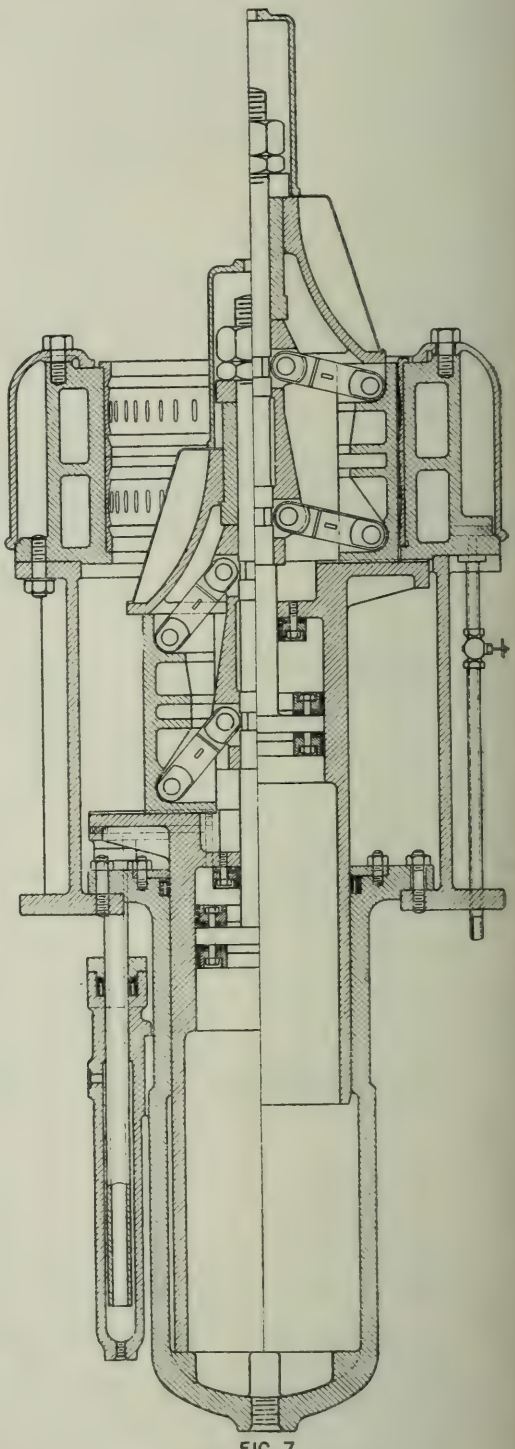


FIG 7

SECTION THRO. E-F

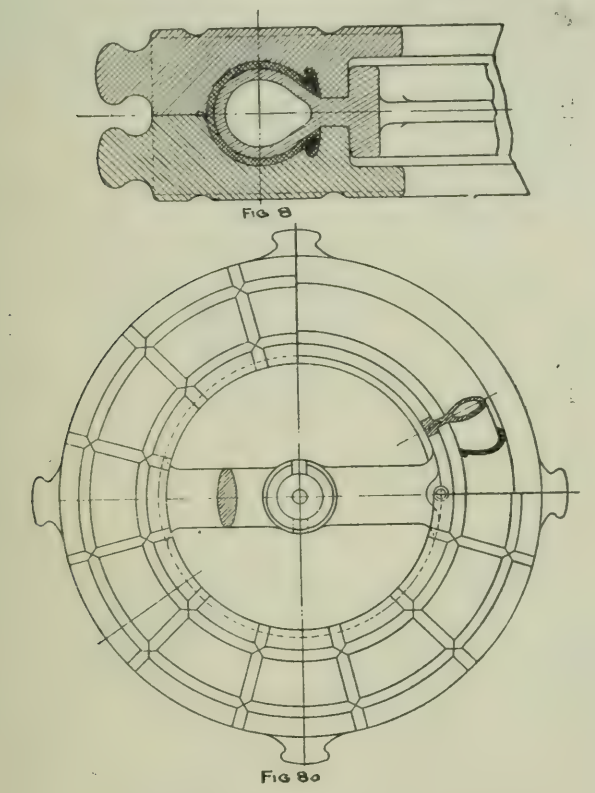
WITH HEAD COLLAPSED AND EXTENDED

FIGS. 7, 7a, 7b.

1.—*Two-cure two-piece system.*

A. Giving a tyre which is not vulcanised completely at one moulding operation but may be—

A1. *Moulded without the tread to the final shape* by external pressure, buffed and solutioned, raw or semi-cured tread then fitted

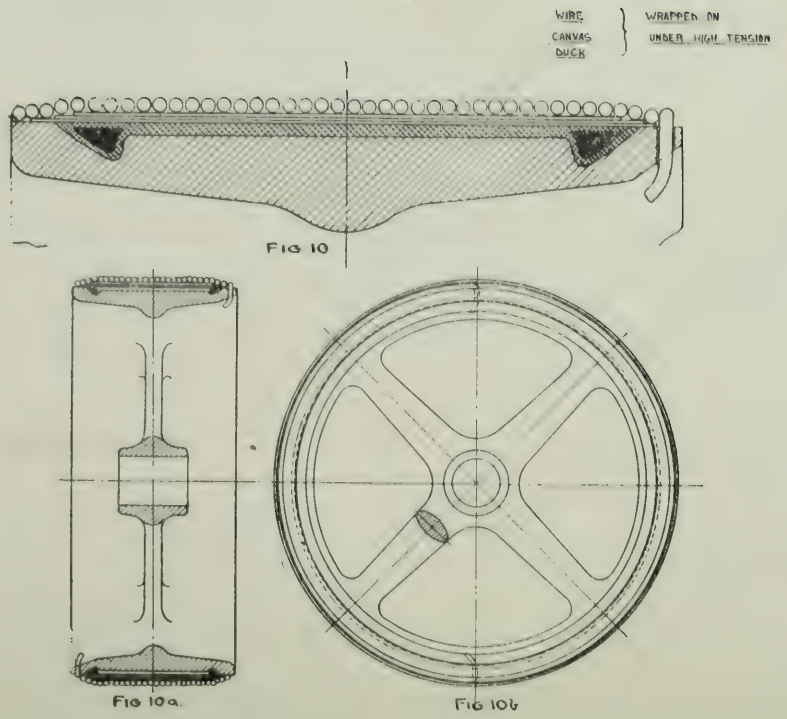


Figs. 8, 8a.

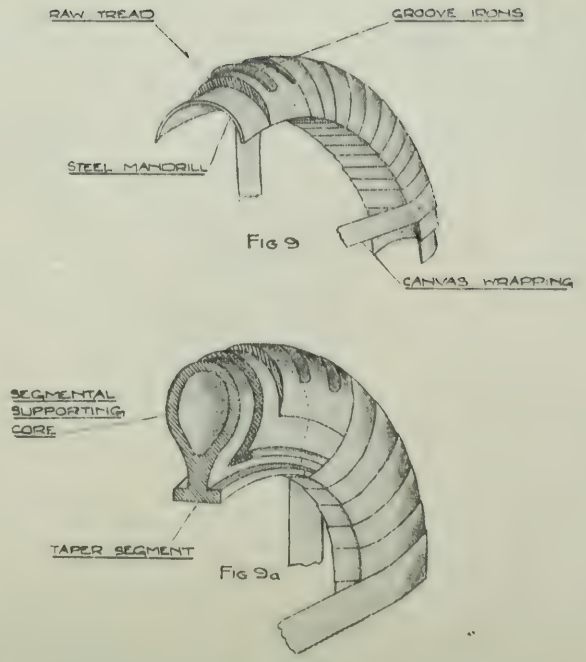
and finally the whole tyre cured with canvas wrapping round it, see Figs. 8, 8a, 9, and 9a.

A2. Moulded without the tread to the final shape but by internal pressure, see Figs. 11 and 11a.

A3. *Moulded without the tread in flat shape* by external pressure, to be later finished in final shape with added tread, see Figs. 10, 10a, and 10b. This method is commonly used for small tyres having rubber and steel studded treads—usually called "combination" treads.



FIGS. 10, 10a, 10b.



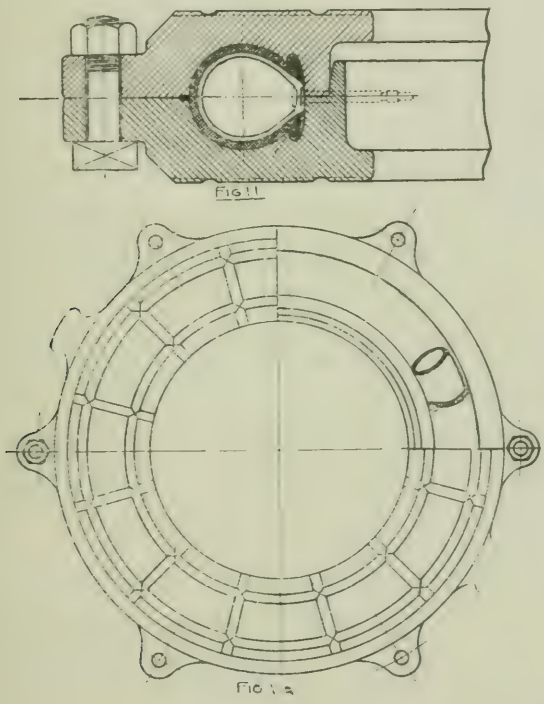
FIGS. 9, 9a.

2.—*Two-cure one-piece system.*

B. Giving a two-piece moulded tyre with a one-piece appearance.

B1. Casing moulded without the tread to final shape by external pressure, buffed and solutioned, raw tread then fitted, the whole finally cured in a pattern mould, see Figs. 8 and 8a, 3 and 3a.

B2. Where the case is semi-cured on a flat drum by external or internal means with the tread, and then shaped for final cure—only used for small size tyres. Figs. 6, 6a, and 6b, or Figs. 7, 7a, and 7b.



Figs. 11, 11a.

3.—*One-cure one-piece system.*

Where the complete uncured tyre is moulded and cured in a "patterned" mould.

C1. To final shape by external pressure, see Figs. 3, 3a, 4, and 4a.

C2. To final shape by internal pressure, Figs. 5 and 5a.

Types of Rim.

These having now been standardised under two broad types, namely, the Beaded Edge (B.E.) and the Straight Side (S.S.) types, it is as well to outline the broad features of each and their relative advantages, as the design of tyres and the results obtained from each depend to a very considerable extent on the type of rim to which they are attached.

Beaded Edge Rims.

The *one-piece type*, Fig. 12, Plate VII,

Is the best known;
Has been longest in use;
Is the simplest rim known;
Is the lightest rim known;
Requires an extensible bead tyre to fit on it;
Gives very good protection against wet getting into tyre;
Is likely to survive for small car use to the exclusion of other types.

The *two-piece loose flange type*, Fig. 12a, Plate VII,

Reduces muscular effort when fitting large tyres;
Is not excessively heavy;
Makes it possible to use inextensible bead tyres which do not require stretching over the flange on to the rim seat;
Is liable to rusting effect on beads;
Gives trouble due to rusting in place, and is liable to blow off in use.

This rim is seldom used on European cars and on very few American modern cars.

The *collapsible type*, Figs. 13 and 13a,

Requires to be detachable from the wheel felloe;
Entails considerable weight of the combination;
Requires little muscular effort in fitting;
Involves considerable complication due to having to provide detachable and collapsible features.

Generally, B.E. rims are most satisfactory when of the one-piece type.

Straight Side Rims.

Two-piece type, Fig. 14, Plate VIII.

This type, though comparatively unknown in Europe, is almost universal in U.S.A. for cars larger than Fords.

It is a development of the standard English wired-on cycle tyre and rim on a larger scale.

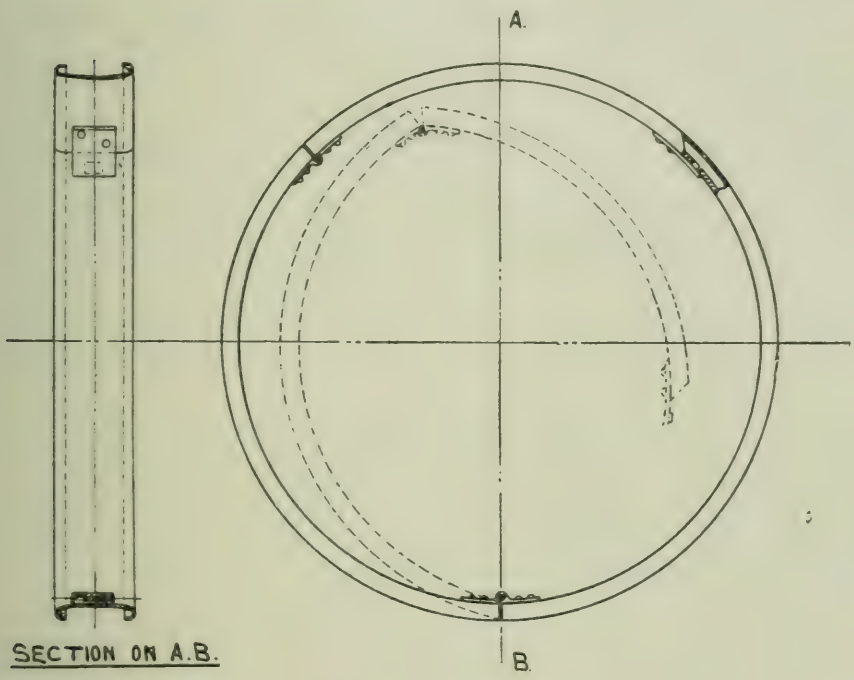


FIG. 13a. FIG. 13.

BEADED EDGE COLLAPSIBLE RIM.

FIGS. 13, 13a.

The merits of this combination are very great, and can be summarised as follows:—

Little effort needed to fit and take off tyres;

Good support given to tyres at point of attachment;

Wide base given to section helps to prevent rolling;

Simplified construction of tyre bead facilitates manufacture.

One-piece detachable, Figs. 15 and 15a, Plate VIII.

The smaller sections of S.S. rims are sufficiently shallow and flexible to enable them to be contracted, thus allowing of a one-piece section of rim having great lateral strength.

The following are its main features:—

Detachability necessary;

Considerable weight entailed;

Easy fitting, though several tools are required;

Great lateral strength and accuracy of rim section;

Freedom from rust damage.

S.S. rims bid fair to be generally adopted for all but light cars and Fords, as the care and effort needed in handling by the user is less than with large B.E. tyres. Also S.S. tyres are more durable than B.E. tyres in the larger sections, due to the better support given to the tyres against rolling action, and such tyres suffer much less than B.E. tyres from rim cutting caused by running deflated.

Tyres on B.E. rims are subject to great local pressure at the rim clinch, and many tyre makers have found great difficulty in obtaining freedom from failure at this point. This difficulty does not arise with well designed and constructed tyres, provided that the rims are well finished at this vital point.

Design.

Explanatory Note.

It must be understood that the methods of design dealt with in this paper do not deal with tyre design as it might be if the necessity for suiting existing standard sizes and for meeting keen competition were not taken into account. The author will not deal with the theory of the action of a pneumatic tyre, but will endeavour to show clearly how a tyre of well known type is designed and manufactured.

Having summarised the well known systems of moulding and vulcanising and the two common methods of attachment to the wheels, we will assume that the tyre designer is given a specification of requirements for a line of tyres. We will then follow out the preparation of a design complying with this specification, and then describe manufacturing methods to suit such tyres.

Specification of tyre under consideration as regards design and manufacturing methods.

$30 \times 3\frac{1}{2}$ *Rubber Non-skid.*

(a) Rims. B.E. type "British standard" section.

(b) Casing. Canvas, 4 plies.

(c) Tread. All rubber non-skid type.

(d) Moulding. By external pressure system.

(e) Curing. By one-cure, one-piece system.

Fig 2 shows the assembled cross section of a rim, cover and tube in inflated condition complying with the above specification.

We will now deal with the various points which arise in working out the required design, taking them successively from the point of application of the tyre on the rim to the actual pattern of tread adopted.

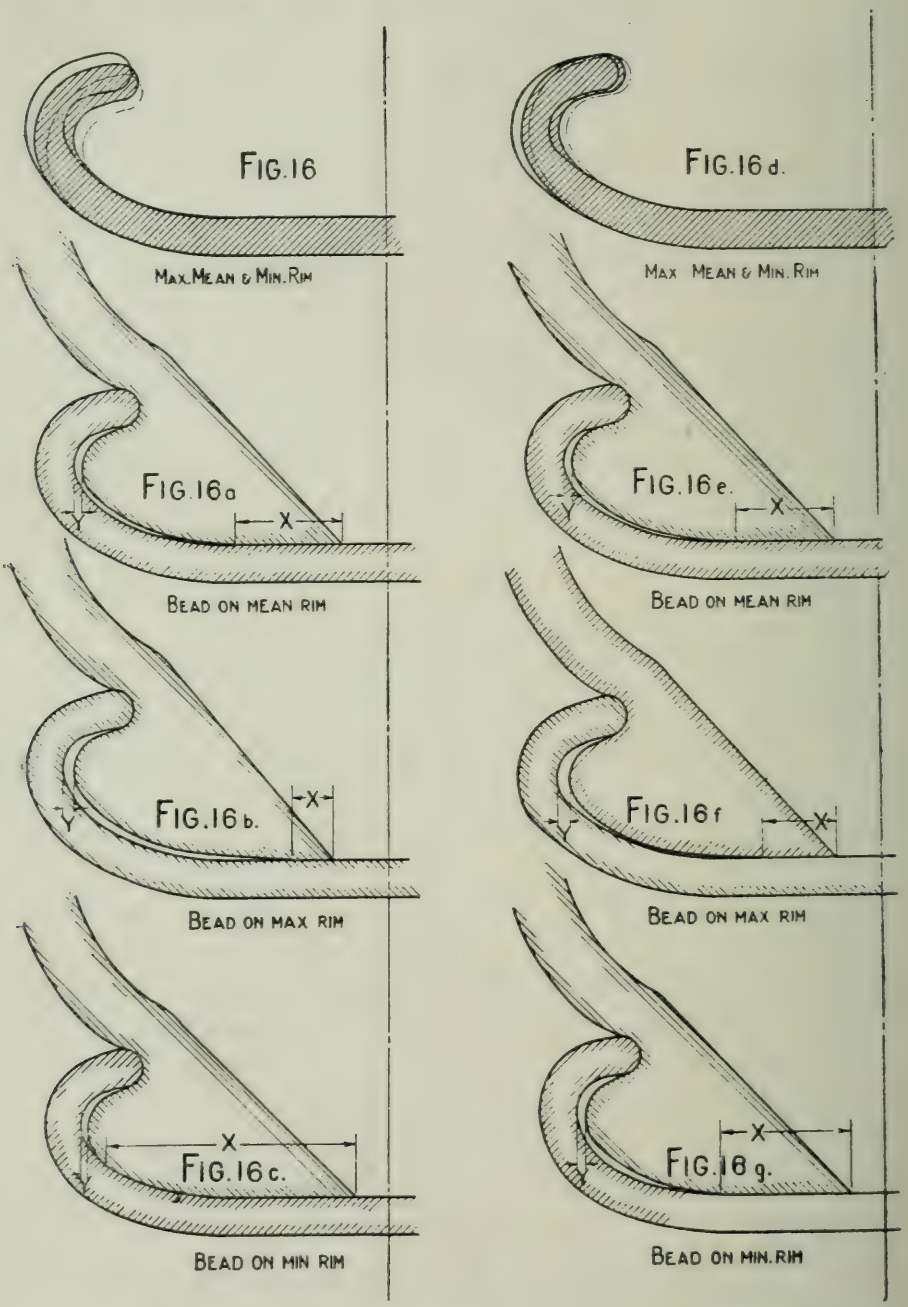
Beads, and how Tolerances allowed in Rims affect the Design of Beads.

Fig. 2 shows a $30 \times 3\frac{1}{2}$ "British standard" rim with a tyre fitted. It will be noted that the tyre beads bed at the clinch or rounded lip of the rim and for a slight distance underneath this clinch towards the corner of the rim. This portion is known to designers as "point A." The beads also bed on the base of the rim from the toe and along a portion of the under surface of the bead. The design of tyres would be much simplified at the bead portion if rims were made more accurately to size. The limits of variation allowed in the sectional shape of the rims, which are very considerable, are illustrated as follows:—

Fig. 16 shows a $3\frac{1}{2}$ in. "British standard" rim with the maximum, mean and minimum sections which will pass the gauges, and demonstrates the wide tolerances allowed.

Figs. 16a, 16b and 16c show respectively $3\frac{1}{2}$ in. rim sections of mean, maximum and minimum dimensions, each with a correctly shaped bead of the same design in place. The tolerances allowed by the American Tyre and Rim Association are considerably closer than British tolerances, as is evident from Figs. 16d, 16e. 16f and 16g, showing the S.A.E. $3\frac{1}{2}$ in. rim.

The consequences of the more serious defects due to incorrect rims can be summarised as follows:—

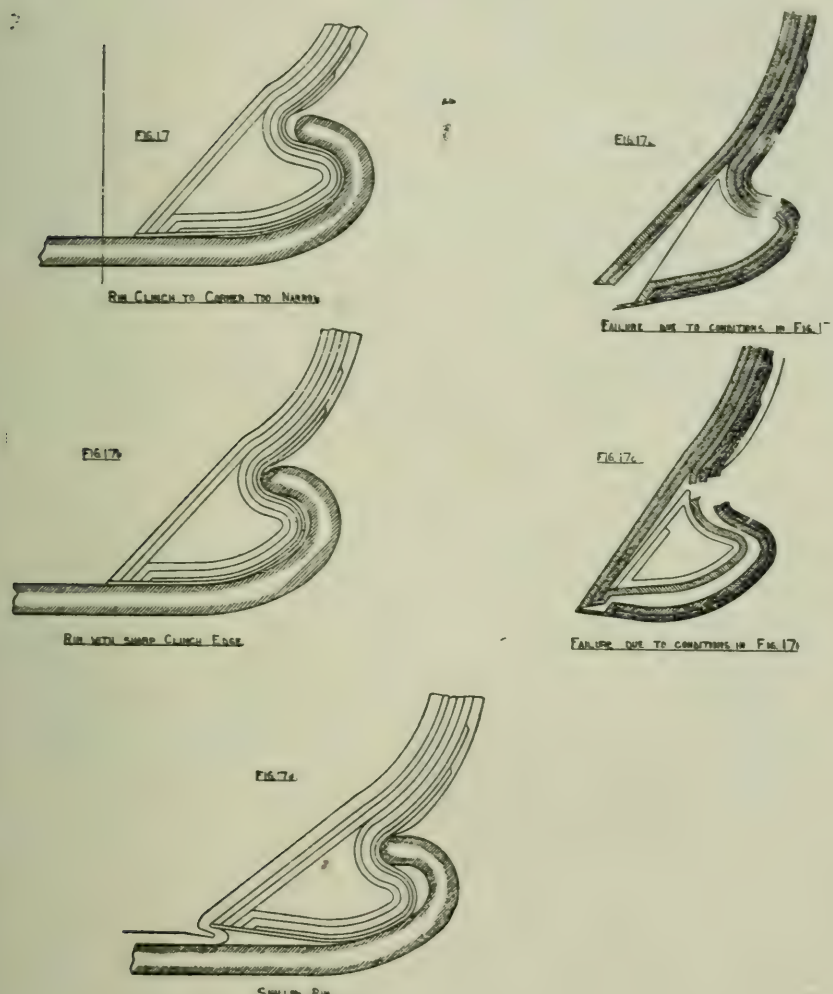


FIGS. 16, 16a, 16b, 16c, 16d, 16e, 16f, 16g.

Narrow rims.—Fig. 17 shows a rim which is too narrow from the clinch edge to the corner of the rim. This prevents the tyre

from bedding into the clinch point properly, and puts an undue pull on the junction of the case plies to the bead core, causing an eventual failure as shown in Fig. 17a.

Sharp edges of rim at clinch.—This is shown in Fig. 17b, and



Figs. 17 to 17d.

the effect in Fig. 17c. The consequence of this defect is that the outer plies are cut right through and the bead core is peeled out of the tyre, causing early failure.

Shallow rims.—The effect of these is that the toe of the bead

is not easily able to lie down on the base of the rim, as shown in Fig. 17d, and tends to pinching of the tube due to the air pressure forcing it below the bead toes.

Reinforcement of Bead portion of Tyre by Chafing Strips.

Fig. 18 shows how chafing strips are added to prevent damage to the vital plies of the casing by sharp or rusty rims or any undue

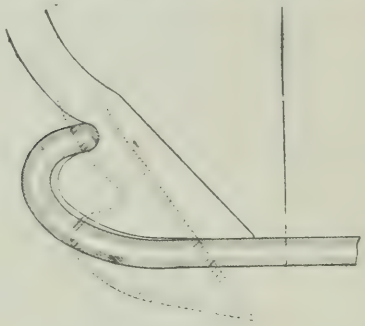


FIG 18 c.
STRETCH OF BEAD ON RIM.

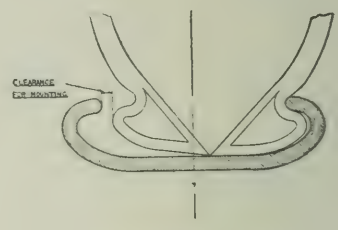


FIG. 18 b.
LIMITATION OF BEAD TOE LENGTH
CLEARANCE OF MOUNTING

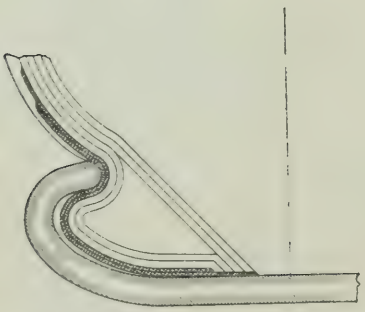


FIG. 18
USE OF CHAFING STRIPS.

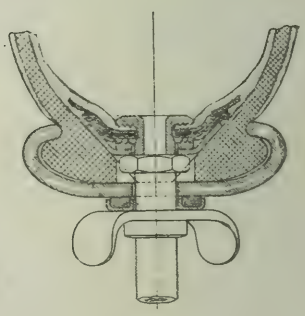


FIG. 18 a.
LIMITATION OF BEAD TOE LENGTH

L 13.

FIGS. 18 to 18c.

frictional action which takes place when the tyre is overloaded or under-inflated.

These chafing strips are continued down to the toe of the bead and take care of any rusting effect on the tyre surface caused by water getting into the rims again protecting the vital casing

plies. They are also useful in the case of tyres running at any time in a punctured condition, as they then prevent an immediate failure due to the great load coming on the casing plies, which would otherwise be pressed directly on to the upper surfaces of the rim edges.

Chafing strips provide protection at the point of great compression, and are very essential on the large sections of tyres: for instance, the pressure per inch of bead length at this point is 217 lb. for 150 mm. tyres, and may reach a pressure of 723 lb. per sq. in. of contact surface at the clinch, whereas on a smaller tyre such as the $3\frac{1}{2}$ in. the pressure may reach $81\frac{3}{4}$ lb. per inch of bead length or 355 lb. per sq. in. of contact surface. These figures are based on rims of correct thickness at the clinch point; thin rims increase the intensity of pressure on the clinch surfaces.

It should be noted that a tyre to fit a given size of rim need not vary as regards the shape of its bead, although the body of the casing and the tread dimensions might vary considerably; thus a bead for a rubber-studded tyre would be the same as that for a steel-studded tyre of similar dimensions.

Bead Core.

It is obvious that on a B.E. tyre the core of the bead is necessary to ensure that the outer plies retain their proper shape to give a permanently good grip on the rim. The tendency when in operation is to separate the bead core from the inner plies, so that the adhesion of the core to the inner plies and also to the outer plies must be ensured by proper mixings and vulcanisation. On this point it is well to emphasise the necessity for the proper consolidation of the casing to do away with any sponginess in the plies between the bead core and the edge of the rim. If this is not allowed for, a failure of the tyre will take place at this vital point.

It is very necessary that the bead core shall (while being sufficiently hard to prevent distortion or movement when in use) be capable of being stretched without undue effort over the outer diameter of the rim and sprung into place on the bed of the rim. The stretching quality must be combined with the necessary elasticity, so that repeated fitting and demounting can be carried out without the tyre becoming loose on the rim. The moulded bead for B.E. tyres is usually composed of a semi-vulcanite centre with a wrapping of rubbered canvas round it, the combination being

moulded and semi-vulcanised before incorporation in the casing. The correct design of the bead core in this type of tyre is of vital importance to insure freedom from defects in making the box-moulded type of tyre.

Limitation of toe length.

This is necessary to allow easy fitting and to enable the valves and "security" or "spreader" plates to be properly fitted, see Fig. 18a. It is also necessary to limit the toe lengths so that when fitting or taking out the second side of the bead it shall miss the first side, as shown in Fig. 18b.

Percentage of stretch on rims.

Fig. 18c shows in dotted lines the position in which the beads of a tyre are moulded, and the amount of radial stretch to which they are subject when actually fitted on the rim. The stretch is generally 1 to 4 per cent when fitted on the rim, according to the ideas of the manufacturer, and according to the amount of bead stiffness and the size of the tyre concerned. The correct determination of stretch has an important bearing on ease of fitting and freedom from blowing off and "nipping" troubles.

Casing dimensions.

Assuming that the "square footage" and quality of canvas incorporated in the tyre is such as to enable the manufacturer to compete with other makers, it is necessary to decide upon the number of plies incorporated in the casing. Having done this, the "square footage" allowed limits within very small variations the main dimensions of the tyre itself, namely, the diameter of the tyre and its outside sectional width when inflated on the rim.

"Toe to toe" size—or sectional periphery.

This dimension gives a very good basis of comparison between one maker's tyre and another, provided an equal number of plies and an equal quality of canvas are used. Given a certain "toe to toe" dimension, we can then very closely estimate the dimensions of the tyre mould to give the required section.

All well known tyres are built up of textile material having bias running threads incorporated in the body of the casing, and it is obvious that any appreciable variation in the angles of the

threads forming the casing itself will have a bearing on the width and diameter of the finished tyre produced from a given size of mould. The reason for this is, of course, that on inflation there is more or less displacement of the cotton fibres (which are subject to load due to the contained air pressure) in their efforts to arrange themselves throughout their length under equal loading conditions. There is, therefore, a certain amount of "cut and try" method in getting out the first sample moulds of a new type of tyre, though with experience sizes can be very closely worked to.

The tyre should be moulded and vulcanised as nearly to its inflated shape as possible, so that in use it will approximate to the shape and condition in which it was when it was vulcanised. If much distortion in the inflated condition does take place it makes the tyre liable to disintegration at the distorted points.

For the $30 \times 3\frac{1}{2}$ tyre under review, we will adopt 4-ply casing, which has proved well equal to the average conditions under which this size of tyre works, and since it conforms to the practice of other manufacturers, puts the design on a competitive basis.

The method of treatment of the canvas and of incorporating it in the casing will be dealt with under the heading of "Manufacture."

Non-skid tread design.

The tread adopted for the tyre dealt with and shown in Fig. 19, Plate IX, was designed to give high efficiency as regards the six main points as set forth in the first paragraph of the paper.

For comfort.—A sufficient depth of pattern has been provided to give a proper cushion between the casing and the road.

It will be observed that the studs are well radiused at their base, so that each one can be likened to a supporting stool, distributing the load over the maximum possible area on the casing.

For safety.—A tread must have a good "fore and aft" and "lateral" grip on all conditions of road surfaces. The design in question is a bold pattern, which is equally effective whether "driving" or "braking," and it should be pointed out that so long as "fore and aft" grip is maintained, a car is under brake control.

Just as soon as the "fore and aft" grip fails, the wheels become locked and the car depends for safety entirely on the "lateral gripping" powers of the pattern.

Fig. 20, which illustrates a section of the tyre on the road when the car is under the tendency to slide into the curb, shows that not only do we have a scraping edge E directly exposed to the surface of the road, but we have a second scraping edge E2 between that and the centre of the tread, and finally a bold centre rib E3, and that all these three points are presented like saw teeth to resist the lateral sliding action. In addition, the centre studs under lateral sliding conditions bring into effect their well buttressed sides E4 which are provided with square edges E5.

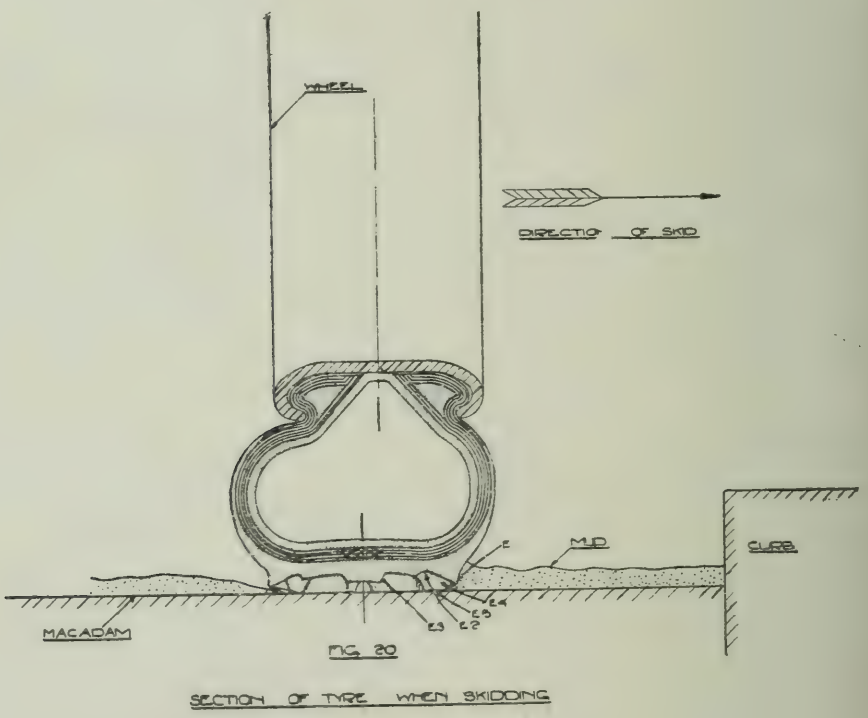


FIG. 20.

Fig. 21, Plate IX. shows that though the tread is in a half-worn condition, the scraping edges are still sharp and the large studs still have their buttress edges intact. Thus, although this tyre has lost a considerable amount of "fore and aft" grip, yet the "lateral" gripping points still retain about two-thirds of their original efficiency.

Reliability.—For reliability, the tread should be impervious to blows from all road obstacles, and should protect the casing from

damage due to blows or bruises. Given a good design of tread, the most important thing is to have a thoroughly good quality of mixing, able to resist cuts though still providing a good cushion. This point does not particularly come under the control of the tyre designer, though he must know that, given certain properties in tread mixings, he has to provide sufficient thickness to give the tread a chance of acting as an efficient protector.

In the case of a large tyre it is not sufficient merely to put a tread on to a case, but on account of the very high load which comes upon the tread surfaces in contact with the road, it is necessary to provide a specially soft high-quality "cushion" between the tread and the case in order to prevent separation under the loading conditions or when running over rough stony roads.

In the case of the larger section tyres, it is necessary in some cases to fit what is termed a "breaker strip," and sometimes two or more are used. This practice, however, is to a considerable extent dependent on the design of tread adopted and the quality of mixings used and on the ability of the tread to protect the casing without requiring further reinforcement. In the case under consideration, a soft rubber pad has been used between the tread and the casing.

Tread proportions.—It cannot be laid down as a law that a heavy and bold pattern tread will in itself be an effective non-skid and protector for a definite mileage, but the quality of mixings and the thickness of tread used will also affect the results.

The design under consideration is an example of an endeavour to so balance the pattern that the great bulk of the tread is disposed where the greatest wear comes, while yet providing sufficient material at the borders of the tread to ensure proper stability and good protection from glancing blows from road obstacles.

The result of such bulk distribution is to extend the mileage for which the non-skid qualities are retained, and to provide at all times a "lateral" emergency grip.

The rolling factor of a tyre section and its effect on the stability of pneumatic tyres.

All motorists know that when tyres are under-inflated a "roll" takes place, and this is undoubtedly due to the lateral displacement of the tread contact point, as compared with the true running plane of the rim.

The "stability" of a given size of section is demonstrated on Fig. 24, where we have the base P of an inverted loaded triangle as compared with the height R of such triangle. The apex angle X will be reduced when an oversize tyre is fitted to the rim, and the rolling factor will vary according to the angle X.

TABLE I.

Rolling Factors for Canvas Beaded Edge Tyres.

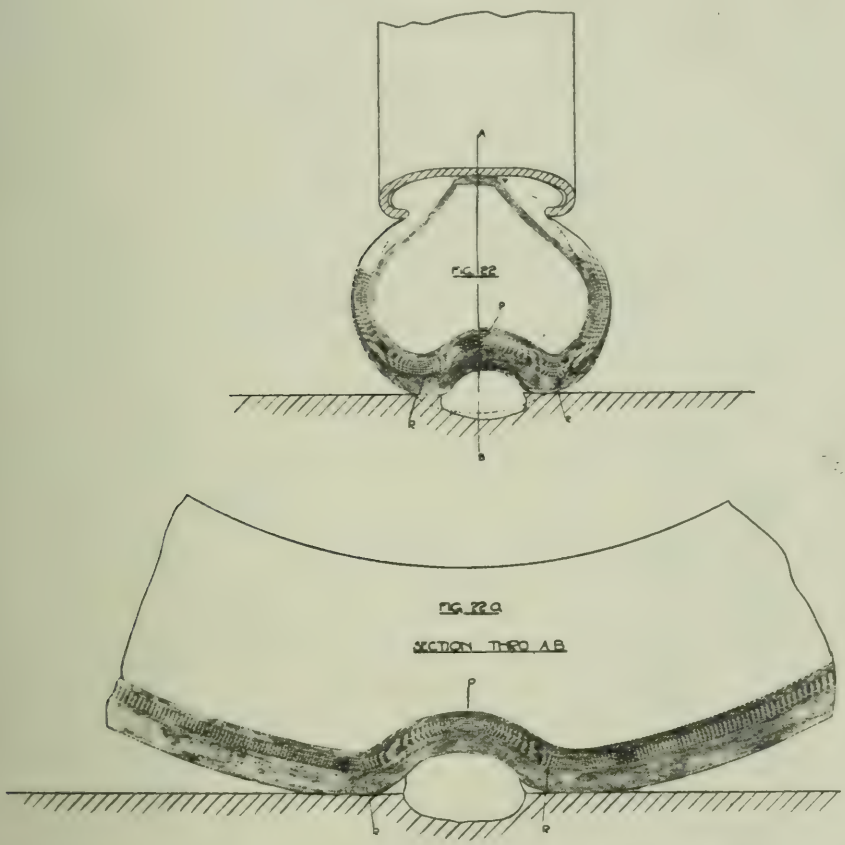
	Size. 30 by $3\frac{1}{2}$.	Oversize. 31 by $4/3\frac{1}{2}$.	Size. 815 by 105.	Oversize. 815 by 120/105.	Size. 820 by 120.	Oversize. 820 by 135/120.
Load, lb.	570	570	815	815	1100	1100
Standard inflation pressure for Size and inflation pressure for oversize, giving the same vertical compression under load.	55	45	65	60	75	60
P. mm. (See Fig. 24)	52		62·5		67·5	
R. mm. (See Fig. 24)	64·4	72·0	67·8	84·7	80·9	89·6
Rolling Factor, P/R	0·808	0·723	0·922	0·738	0·834	0·753
Angle X in degrees and minutes. (See Fig. 24)	44 0	39 40	49 30	40 30	45 20	41 10

Table I. shows average figures of the angle X and the rolling factor P/R, and is interesting as showing samples of current practice as regards these properties adopted to-day for B.E. canvas tyres; thus, on standard sizes the angle X varies between 44 and 49 degrees, and in oversizes on the same rims, between 39 and 41 degrees, the oversize figures being taken with tyres run at a proportionately reduced pressure compared with that of the standard size tyres.

It is obvious that with a well inflated tyre the rolling factor will depend very largely on the "grip" of the tyre on the road; for instance, a plain tyre would not "roll" but would skid, whereas a non-skid pattern would "roll" somewhat but would not skid. It is obvious also that non-skid patterns will tend to roll more than plain patterns on dry surfaces, due to their better gripping qualities.

Resistance of tread and casing to road obstacles.

Figs. 22 and 22a show what takes place in the tyre section when going over a considerable road obstacle, such as a stone of conical shape. It is obvious that a reverse bending action takes place at the centre of the casing right above the obstacle, and



FLEXING OF CASING PLIES WHEN PASSING OVER A LARGE STONE

Figs. 22, 22a.

that the inner plies at the point P are very considerably stretched, as are also the outer plies at R on each side of the obstacle. The load which comes on the fibres of the casing under such conditions depends on the contained air pressure, and on the amount of distortion of the casing from its natural curvature when under load. It is obvious also that if a large section is adopted, the

resistance to absorbing or swallowing the road obstacle would be reduced, as local displacement of the casing would be less.

Effect of inflation on the surface in contact with the road.

Fig. 23 is an impression from a tyre fully inflated, and indicates the surface in contact with the road, whereas Fig. 23a shows the

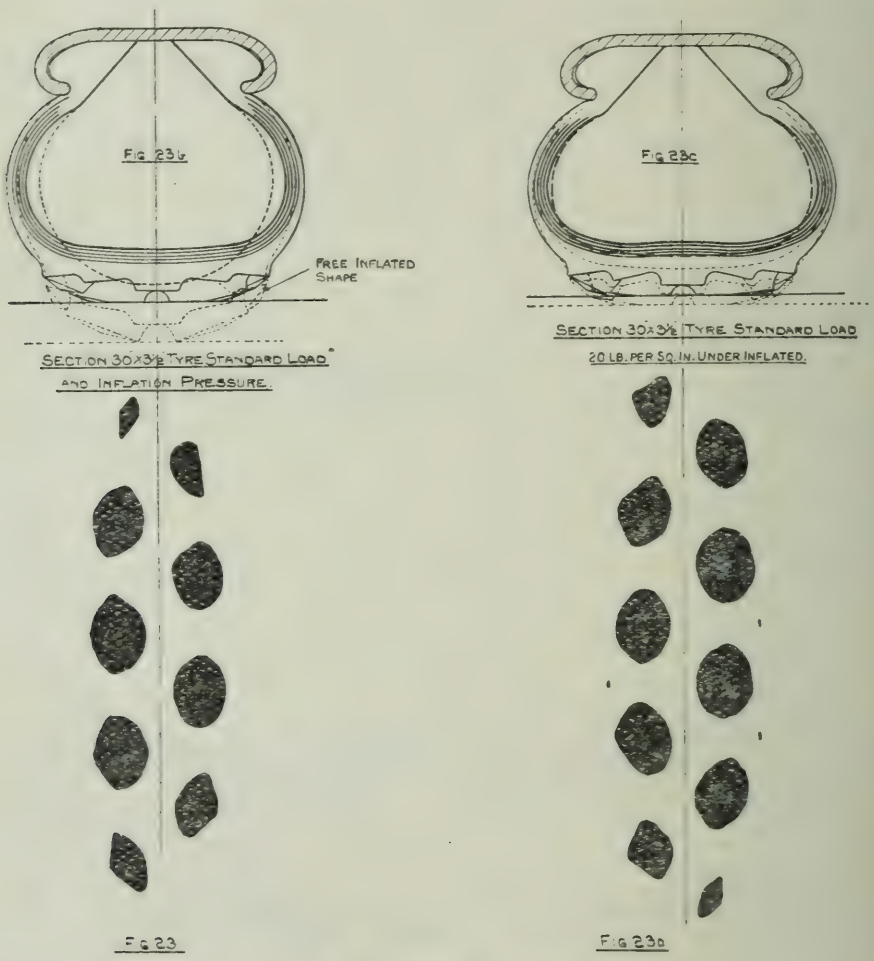


FIG 23b
FREE INFLATED SHAPE
SECTION 30x3½ TYRE STANDARD LOAD AND INFLATION PRESSURE.
FIG 23
FIG 23c
SECTION 30x3½ TYRE STANDARD LOAD 20 LB. PER SQ. IN. UNDER INFLATED.
FIG 23a

FIGS. 23 to 23c.

increased surface in contact with the road with the same tyre with the same load but with 20 lb. less pressure.

Figs. 23b and 23c show cross sections taken through the loaded portions of the same tyres, and give an accurate representation of the increased flexing to which the casing plies are subject under such conditions. It is obvious that the tyre which is inflated to

a lower figure will not have the same life as a properly inflated tyre.

Tyre Manufacture.

The pneumatic tyre is an assembly of prepared "rubbered" parts vulcanised together to form a complete tyre; the parts are

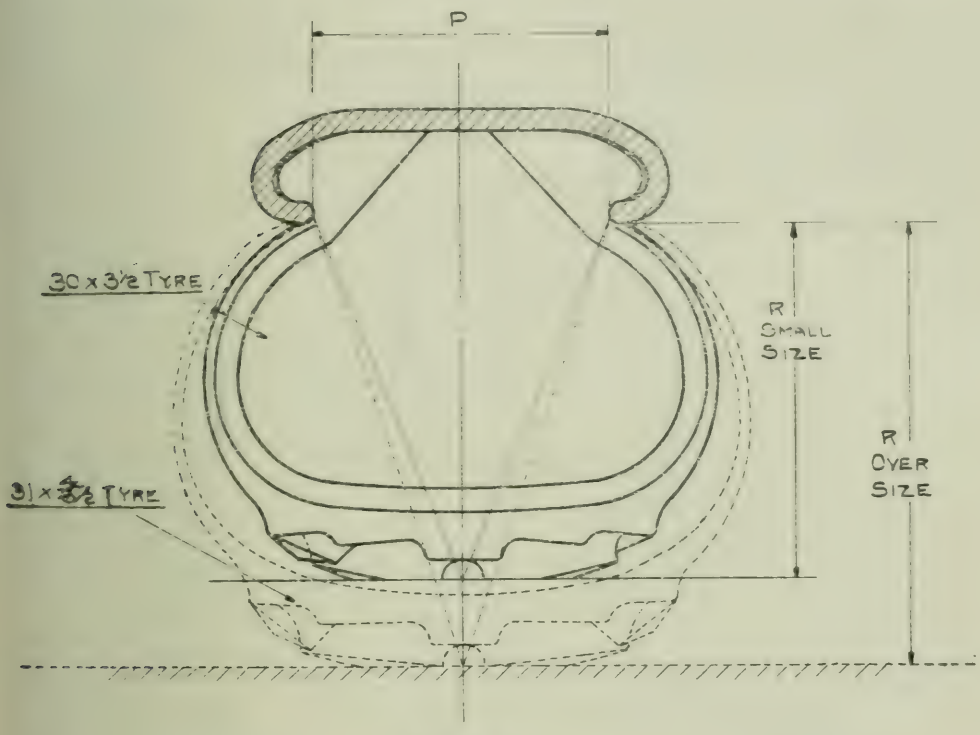


Fig 24

For both tyres

Inflation Pressure 55 lb. per sq in

Load 570 lb.

Rolling of Tyres.

Fig. 24.

built up from semi-finished materials coming under two headings, as:—

1. Prepared canvas.
2. Prepared mixings.

1.—*Preparation of Canvas.*

The casing is the most important unit in the tyre, and the usual methods of dealing with the textile material forming it before delivery to the tyre building department are as follows:—

Textile.—Plain square woven "duck" from high quality Egyption cotton, having the required number of threads per inch

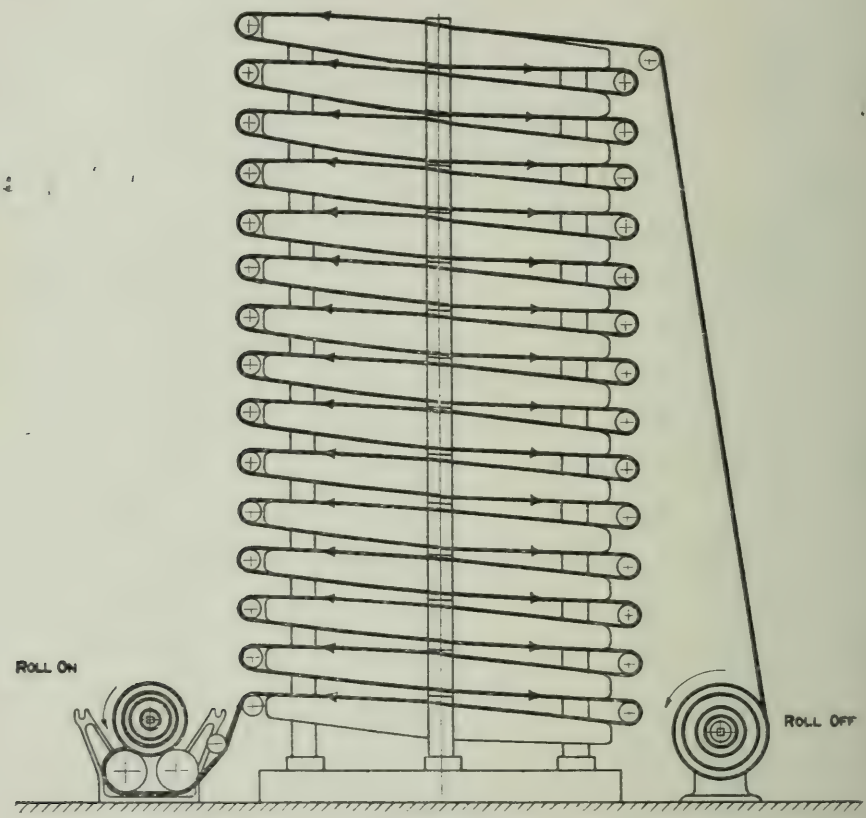


FIG. 26.

FIG. 26.

and the twist in it being such as to absorb the rubber insulating material. The duck is shown in its various stages of rubbering in Plate X:—

Fig. 25, unrubbered or raw.
Fig. 25a, soaked.
Fig. 25b, 1st frictioned coat.
Fig. 25c, 2nd frictioned coat.
Fig. 25d, topped coat.

It is necessary that both warp and weft threads in the completed tyre shall be equally tensioned and of equal strength, and steps

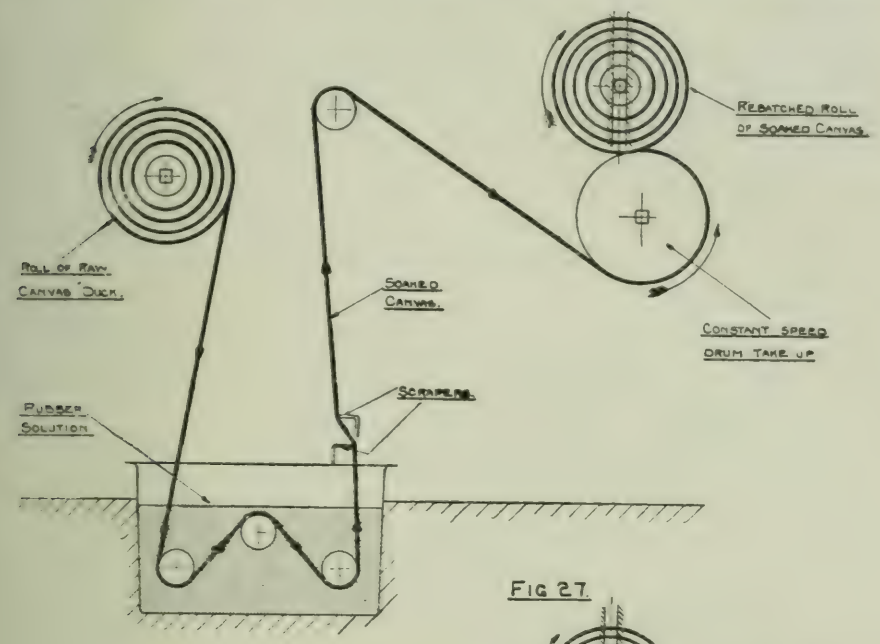


FIG 27.

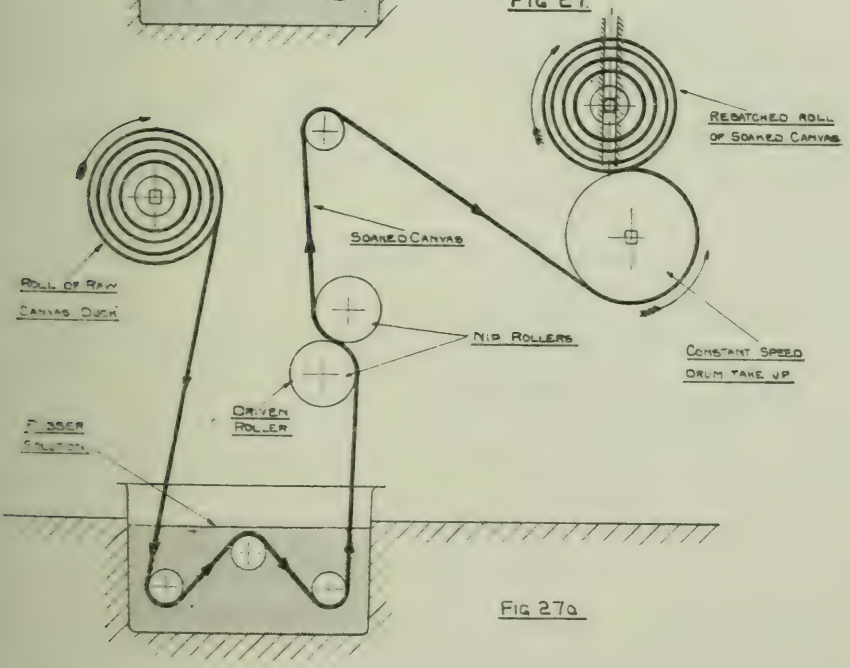


FIG 27a

SOAKING MACHINES

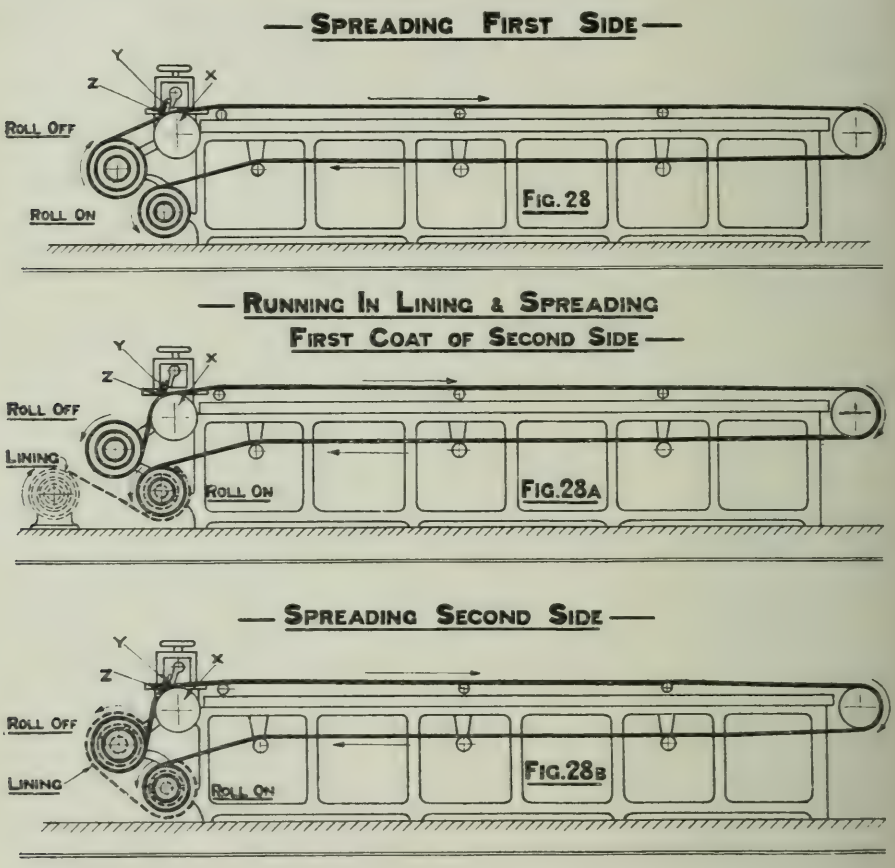
L27

FIGS. 27, 27a.

are taken throughout manufacture at the cotton mills, and subsequently in the tyre factory, to see that such conditions obtain.

Drying.—Before rubbering it is necessary to thoroughly dry the rolls of "duck." This is generally done on either a cylinder drier or a "cell" drier. Fig. 26, this shows diagrammatically the path of the cotton through the drier.

The "cells" are steam chests with curved surfaces top and bottom, and the guide rollers are each positively driven to prevent tension on the canvas, and the machine embodies a constant-speed take-up gear.



FIGS. 28, 28a, 21b.

Soaking.—The first rubbering operation is to soak the canvas in a very dilute solution of rubber and solvent. A common form of soaker is shown in Figs. 27 and 27a, where the path of the canvas can be followed, and it will be observed that the surplus solution is scraped off, or as an alternative to scrapers a pair of "nip" rollers is sometimes used. It is obvious that this simple machine embodies a constant-speed take-up gear.

Drying after soaking.—There are many ways of doing this, the simplest being to pass the rubbered duck over steam-heated chests, merely evaporating the naphtha, which on such a machine is not recovered.

There are a number of satisfactory machines which recover from 70 per cent up to sometimes over 90 per cent of the solvent

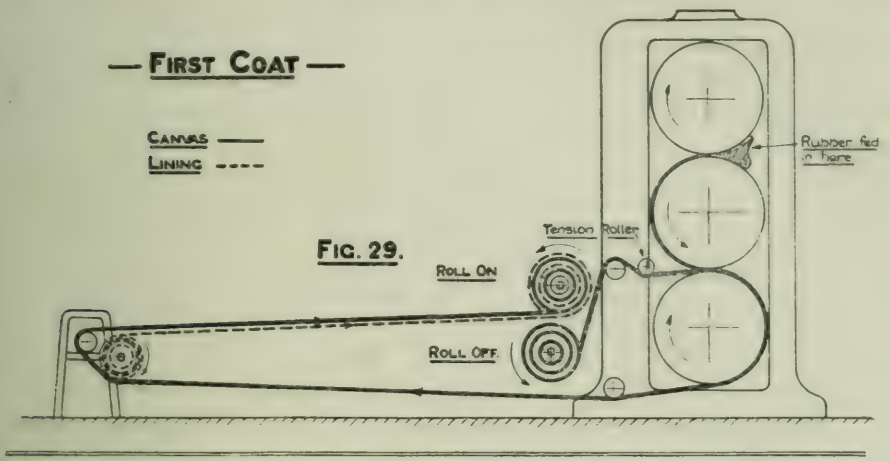


FIG. 29.

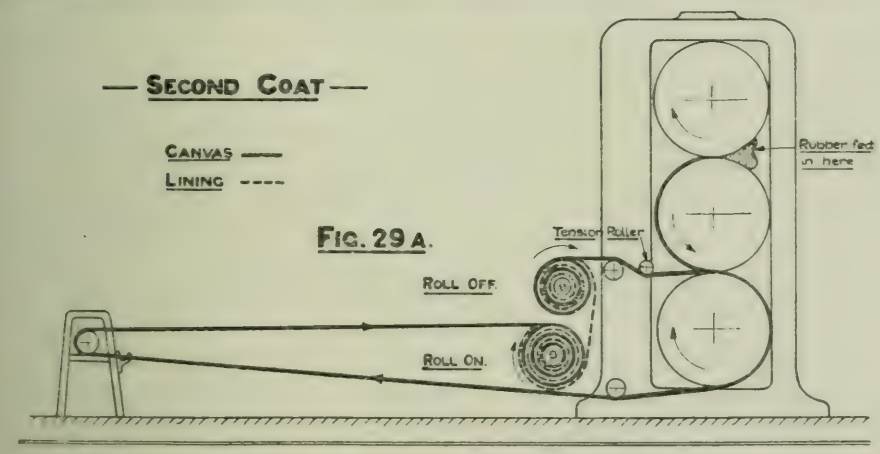


FIG. 29 A.

THREADING DIAGRAM
CALENDER (FRICTIONING)

FIGS. 29, 29a

while drying the soaked material, making a very substantial saving in the cost of manufacture, as the recovered solvent is re-used.

Spreading.—The spreading method is shown in Figs. 28, 28a and 28b. This is a very good method but very difficult to arrange in conjunction with a solvent recovery plant, for though the

quantity of solvent used is not so great as in soaking, yet it is considerable. The principle involved in the spreading machine is that of laying successive thin layers of mixed rubber on to the canvas in a state aptly expressed as "dough." The canvas passes over a rubber-covered driven roll X, the "dough" Z being laid on a "doctor knife" Y, which is adjusted to lay a film of dough against the canvas surface and "spread" it properly.

Between each layer the canvas is dried as it passes over the machine. This method is expensive but very good, as it reduces the danger of damaging the canvas during treatment.

Calendering canvas.—A method practised by some manufacturers is to entirely eliminate the soaking operation and in many cases the spreading operation also, and to rely on the action of "friction" calenders for forcing the rubber into the canvas. A 3-roll friction calender is shown in Figs. 29 and 29a. The canvas is run through the calender twice so that both sides are frictioned. The "frictioning" action is due to the different speeds at which the rolls revolve. The middle roll runs faster than the bottom roll, and the canvas runs at the same speed as the bottom roll; consequently, a rubbing or "frictioning" action occurs which forces the soft rubber into the threads of the canvas.

"*Topping*" *of canvas on calender.*—Though the "frictioning" method is satisfactory for "impregnating" the threads of the canvas, thus ensuring the insulation of adjacent threads in one ply from each other, it does not in itself provide sufficient insulation between plies as built up into a casing. It is usual, therefore, to follow the "frictioning" operation by a "topping" or "skimming" operation which coats one or both sides of the canvas, as shown in Fig. 30. "Topping" calenders also generally have three rolls, but the relative speeds of the rolls are arranged differently from those on "friction" calenders. The top roll runs slightly faster than the middle roll in order to properly sheet out the rubber fed to the top "nip," thus ensuring that an even thickness of film comes round the middle roll. The middle and bottom rolls run at equal surface speeds, and at the bottom nip the function of the rolls is merely that of "transferring" the "film" of rubber to the surface of the canvas.

The rolls are suitably controlled as to their distance apart and as to the temperature of the roll surfaces.

These notes on canvas treatment apply to drying, soaking,

spreading or calendering methods employed for all textiles used in the tyre, not only for the casing plies.

Rubber Preparation.

Compounding.—Each of the rubber parts requires to be compounded from suitably proportioned rubbers, "fillers" and "curing agents" so that it may be well able to carry out its functions in the complete tyre.

Influence of Heat Transmission Differences on Mixings.

Before mixings can be proved suitable for use in tyre construction in actual manufacture, it is necessary to determine that the

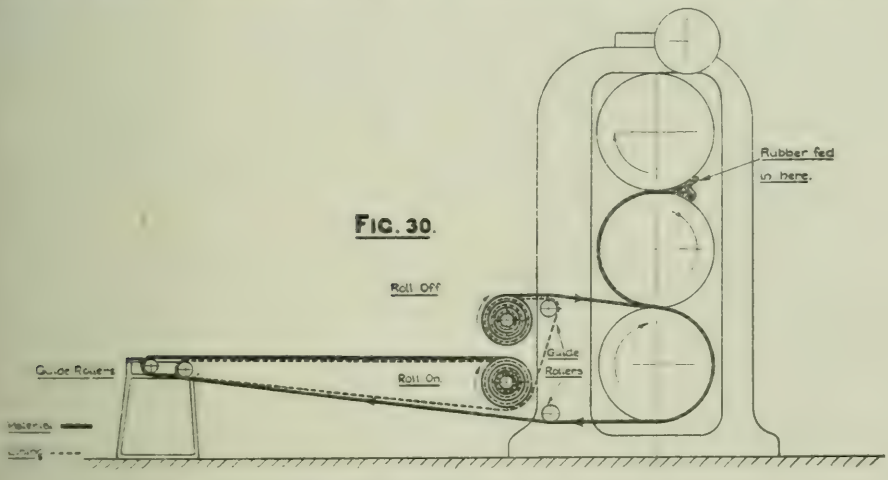


FIG. 30.

THREADING DIAGRAM

Fig. 30.

proportions are such as to ensure correct vulcanisation. No calculation can determine the time and temperature of cures, as it is necessary to allow for the "lag" due to heating the vulcanising vessel and the moulds or wrappings round the tyre and transmitting heat through the metal moulds to the various parts of the tyre contained in the moulds.

Thus it is necessary to ascertain by actually cutting up sample tyres that the interior is properly vulcanised. By suitably varying the amount of curing agents in the mixings, all parts of the tyre can be brought to a satisfactory state in a given time when subjected during such time to a given temperature or range of temperatures. It is of vital importance in tyre manufacture to

make sure of the "cures," and the production methods and manufacturing programme must conform to good "curing" requirements. When once definitely decided upon, cures can be "standardised" for given mixings when dealt with under standard conditions.

Mixing.

The mixing of the rubber with various powders is usually performed on 2-roller mixing mills, the rubber and powders having previously been carefully weighed in batches having the required proportions. The rubber is first masticated on the mill until it is sufficiently plastic for the powders to be absorbed. These are gradually put in, and afterwards the rubber is cut and turned over on the mill so that the whole mass becomes homogeneous. The finished batch is known as "mixed rubber" and as such is delivered in sheet form to the pneumatic tyre manufacturing section.

Having outlined the methods of preparing the canvas and the mixings, we now come to

Pneumatic Tyre Manufacture proper.

Pneumatic tyre factories generally have two preparatory sections:—

A.—*The Rubber and Strip Assembling Section.*

In this department the rubber is calendered and cut to correct gauge and width for such sub-units as side rubber, cushions, etc., and tread rubber and bead cores are prepared. In addition, all rubber and canvas sub-units are conveniently assembled together before being sent to the "Rubbering Section" for attachment to the completed casing.

B.—*Canvas Cutting and Batching Section.*

Here, the various strips of canvas, such as casing plies, bead wrappers, chafing strips, etc., are cut from the prepared canvas rolls into strips and batched for assembling as required into the component parts of the tyre.

Naming of parts and system of assembling from units.

Fig. 31 shows the names of the parts of the $30 \times 3\frac{1}{2}$ tyre section in finished form, the parts from which the complete tyre is made up being shown in units in Fig. 31a.

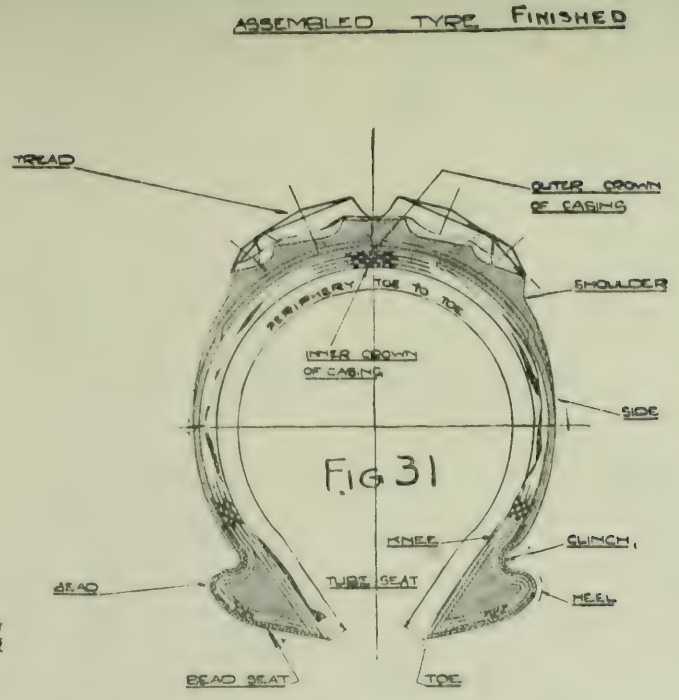


FIG. 31.

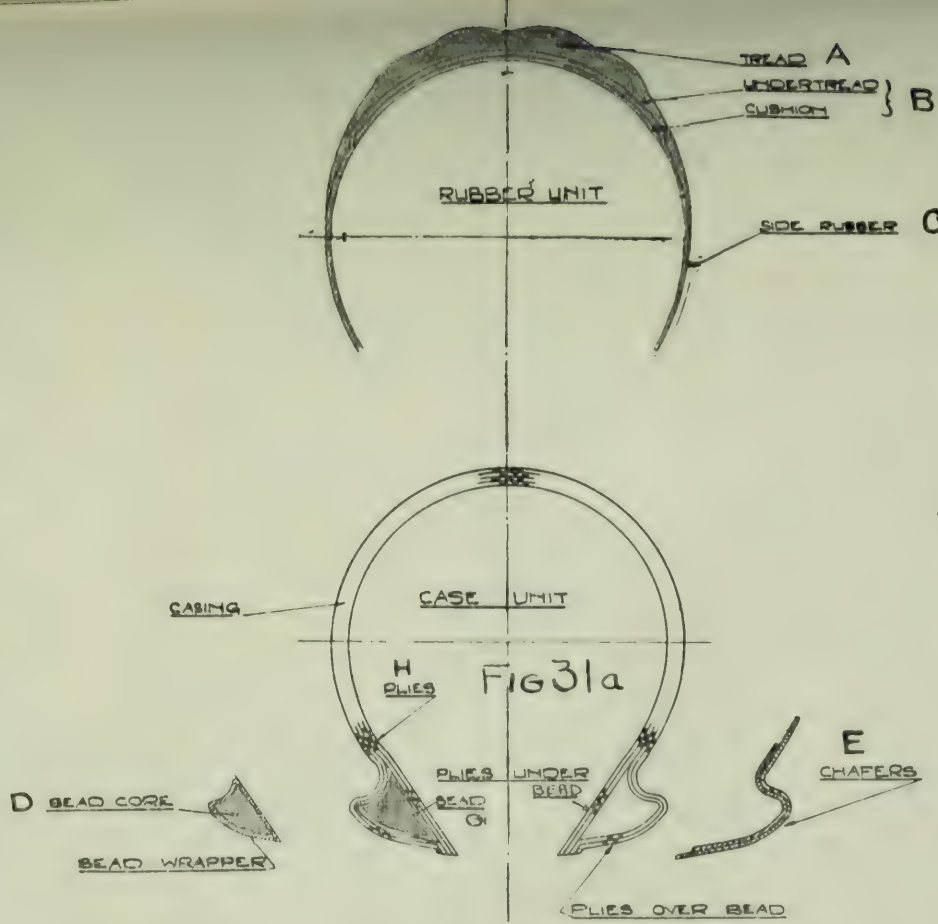


FIG. 31a.

The units are composed of a number of sub-units, which are grouped together under two headings.

Rubber (sub-units).

A. *Tread rubber*, the tread portion proper, which is in the form of long strips, of suitably shaped profile, made from previously mixed rubber.

B. *Cushion rubber*, suitably calendered strips made from previously mixed rubber.

C. *Side rubber*, Ditto.

D. *Bead core*. (Incorporated in complete bead, G.)

Canvas (sub-units).

E. *Chafers*, strips of prepared canvas cut from rolls.

F. *Breaker strips*, prepared canvas cut from rolls.

G. *Beads*, having wrapper strips of prepared canvas cut from rolls and having cores which are of tough vulcanite mixing.

H. *Case plies*, strips of prepared canvas cut from rolls.

Units.

The rubber unit is composed of sub-units A, B, C and F.

The casing unit is composed of sub-units D, E, G and H.

A diagram of the successive steps in manufacture is shown in Fig. 32, on which each of the sub-units can be traced in its progress from the mixed rubber or prepared canvas until the main units are assembled as a complete uncured tyre on a core, to be then vulcanised and finally finished, examined and passed to stock. The diagram shows how sub-units flow along and are assembled into units, and how the main stream becomes finally the uncured tyre.

A.—*Tread rubber.*

The mixed rubber used to make a tread is taken from the mixed rubber stores in a cold and tough state, and it is first necessary to make it plastic so that it can be brought to the desired shape in the tyre. The "box-moulded system" of manufacture requires great accuracy in measurements, and at no point is this

more essential than in regard to the proper thickness and disposition of tread rubber.

Furthermore, non-skid pattern treads call for very careful cal-

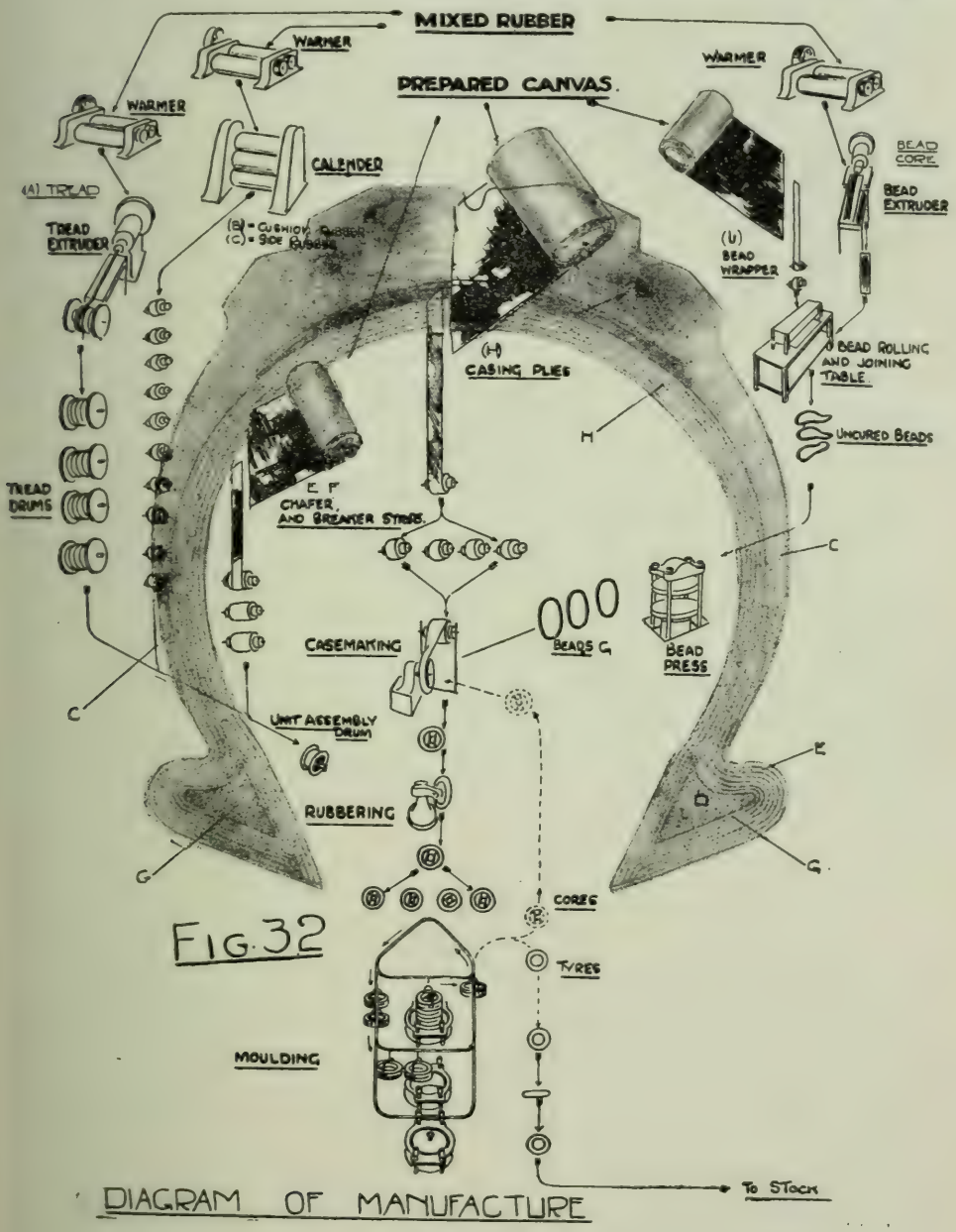


FIG. 32.

Fig. 32.

culations to obtain the correct amount of rubber to ensure perfect moulding. For this reason it is generally the practice to shape raw treads on either profile calenders or "spewing machines,"

these latter being sometimes termed "extruding" or "forcing" machines.

The method of tread manufacture described is that of "spewing."

The rubber is first put on a breaking-down mill and next on to a "warming-up" mill, the processes being shown in perspective in Fig. 33, Plate XI.

The rubber, after being sufficiently worked on the "warmer," is cut off by an attachment in the form of a strip, which is fed in lengths to the spewer. The rubber strip on entering the machine is forced forwards by a water cooled screw through a die which gives it the desired shape.

The tread on emerging from the die runs on to a conveyor belt and is eventually batched up on to a circular drum shown in the general perspective view. The drums on which the rubber is batched are carried on a suitable "stillage" at the end of the conveyor belt in pairs, the conveyor having a suitable tilting end so that it can feed either the top or the bottom drum as required. As the different coils on the drums are filled up, the drums are moved sideways so that they can be completely filled with raw treads.

As an example of the required accuracy in working to limits, it may be stated that a variation in weight of only $1\frac{1}{2}$ oz. over or under is allowed in a tread weighing $3\frac{1}{2}$ lb., or about 2·5 per cent over or under in a length of about 100 inches.

B.—*Cushion rubber.* C.—*Side rubber.*

These are made from mixed rubber which, after being broken down and suitably warmed as described above, is fed into a calender which sheets it out to the required thickness, and which is fitted with knives which cut it into strips of the required width. These strips are batched up on spools by the apparatus shown in Figs. 34 and 34a, Plate XI. These spools with the strip rubber on are made to fit in centres on suitable tables on to which the rubber is drawn and cut to the required length. The spools are also arranged with projections at the end which fit on bars on a "stillage" base for convenience of transport.

It is very necessary to work to close limits of thickness of calendered sheet. This must not vary more than 0·05 mm. or 0·002 in. from the required figure.

D.—*Bead cores.*

Mixed rubber for the bead cores is broken down and warmed up as described above for the tread. It is then fed to an extruding machine which has a multiple die, a number of beads being extruded simultaneously. This machine is similar in construction to the tread extruding machine. The bead cores, however, are not batched up in continuous lengths as are the treads, but are extruded on to a table and cut to lengths. A large number of the cores are put together in suitable trays, which are arranged to load on to stillages for convenience of transport.

E.—*Chafing strips.* F.—*Breaker strips.*

These strips are cut from the previously prepared rolls of canvas to the required width. They are usually cut on the bias at an angle of 45 degrees on a special bias cutting machine, Fig. 35, Plate XII. Some breaker strips, however, are cut on the straight, this operation being performed on a special machine, Fig. 36. The width of the canvas roll is as far as possible such that no scrap is made, each strip being of the required length to subsequently fit the tyre. The strips cut on the straight are continuous lengths, and are cut to the desired lengths as desired without scrap. The strips are batched up roughly on spools, these spools being of similar construction to those used in the calender and described above, so that they can subsequently be mounted on suitable centres on the assembly tables.

G.—*Bead wrappers.*

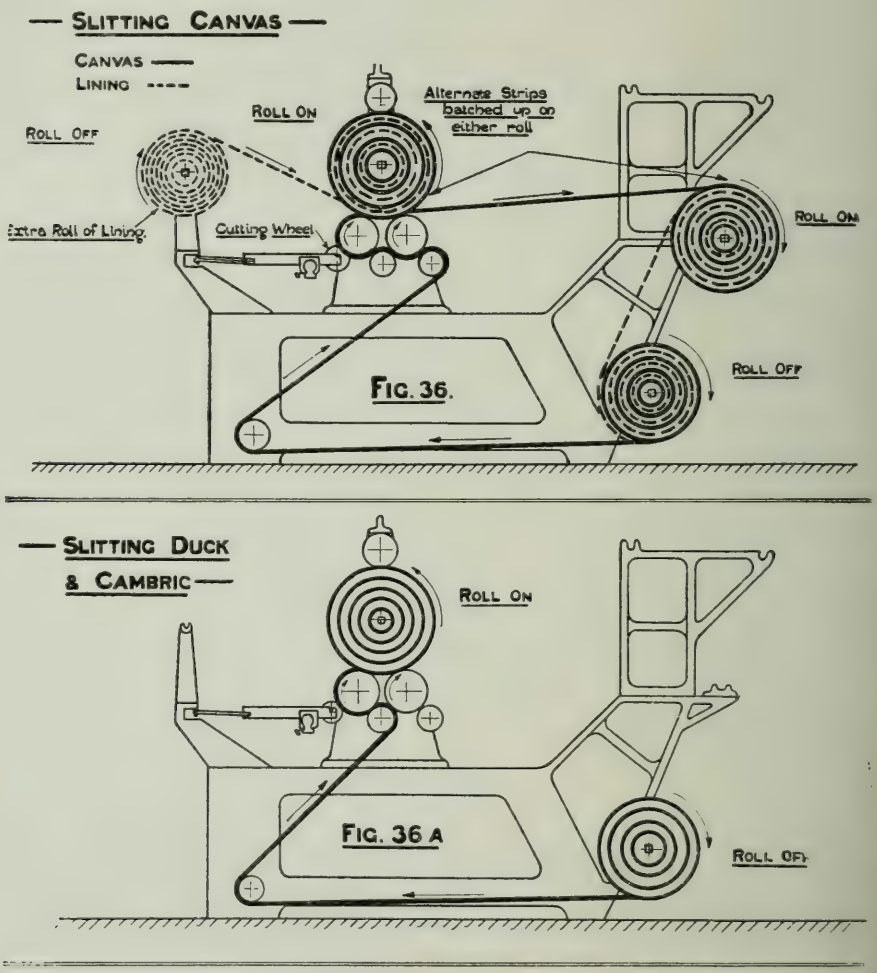
These are cut from previously prepared rolls of canvas on the bias as described for the chafing strips and breakers, and are suitably batched up ready for assembly with the bead cores.

Bead assembly and curing.

The sub-units D and G described above are now assembled together to form the bead. This operation is performed on a bead rolling table, the bead wrapper being laid on the table with the core on it and the wrapper rolled round the core by means of a "rolling board." After "rolling," the bead is joined up to form an endless band. Figs. 38, 38a, 38b, 38c and 38d, Plate XIV, show the various stages in manufacture of a bead built

on the round core principle, and Figs. 39 and 39a, Plate XIV, show sections of an uncured and a cured bead.

The presses, as will be seen from Fig. 37, Plate XIII, consist of an upper fixed platen to which is attached a steam chest, the lower platen being worked by a hydraulic ram and having attached



FIGS. 36, 36a.

to it a second steam chest. In between these is a floating steam chest guided by the pillars of the press, the press thus forming two separate compartments into which moulds can be placed. Usually, two moulds are placed in each compartment, i.e., four beads are cured at once in each press. This cure is only of short duration to give the bead a permanent shape so that it can be

applied to the casing as will be described later. The beads after moulding are gauged for size and shape. The presses may have a multiple number of "daylights," or steam chests, and may do many more beads at one time, but the principle involved is the same. It is the practice of some manufacturers to make their beads in long lengths instead of in "ring" form, but the adoption of such a practice depends generally on the methods adopted for

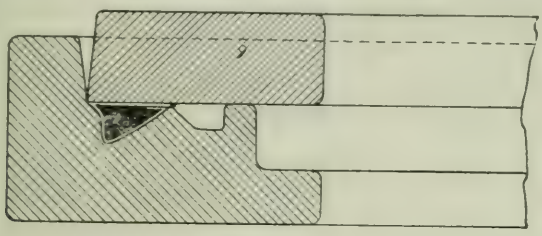


FIG. 40.

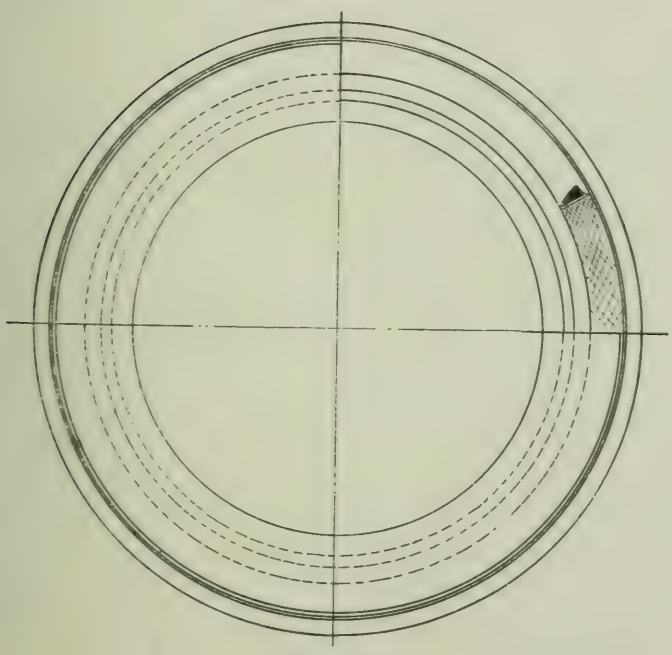


FIG. 40.

"setting" the beads on to the casing itself. There is no doubt that the "ring" type of bead is the best, though not the cheapest. Fig. 40 shows details of a bead mould.

H.—*Case plies.*

These are cut from previously prepared rolls of canvas on the bias cutting machine.

Bias cutting machines may be broadly divided into two classes:—

1. *Horizontal type,* with shear blades. These may be described as a type of guillotine shears, with suitable automatic feeding gear, which feeds the canvas intermittently between each stroke of the shear blades by an amount equal to the width of strip required.

2. *Vertical type.* A machine of this type, using a conveyor and batching arrangement, is shown in Fig. 35. On the left will be seen a framework on which are mounted stock rolls of prepared canvas, also "stripping rollers" over which the canvas is passed to separate it from the cloth lining, and a compensating roller which controls the "let-off" from the stock roll. It is essential that an efficient "stripping" arrangement be provided, as, if the canvas is very tacky and a thorough stripping action does not take place, accurate feeding to the knives would not be possible.

The canvas is finally passed over a second framework which contains the feeding and cutting gear. The roller on the top of this frame, from which the canvas hangs vertically, controls the amount of feed, i.e., the width of strips to be cut, and this can be varied as desired. The strip in its vertical descent passes under, and is held by, a clamp during the cutting stroke. The knife is of the "draw cut" type, and reciprocates across the canvas in a carriage guided by the cross slide, which can be angularly adjusted to vary the bias angle of the strips as desired.

Conveying and Batching Arrangement.

After the plies have been cut they are caused to fall by means of stripping gear on to a suitable conveyor, which carries them in succession past a number of batching turrets, on which they are roughly batched in suitable cloth lining.

As each tyre casing is built up of plies of two widths, i.e., narrow plies going inside the bead core and wider plies going outside and below the beads, it is common practice to join these up on spools in lengths suitable for three to five complete casings per spool. Thus, as the plies cut on the bias cutting machine are programmed in quantities of "wides" and "narrows" as required to suit output, it is necessary to have a second operation in which the narrow and wide plies are correctly assembled and

batched up on a ply batching table, suitable for casemaking machine operators. The arrangements in the batching section depend largely on the method of casemaking adopted.

Ply Batching.

On the batching tables the plies are batched up, narrow and wide together, in the correct order, so that they form a continuous strip, which is later wound on the casemaking machine. Figs. 41

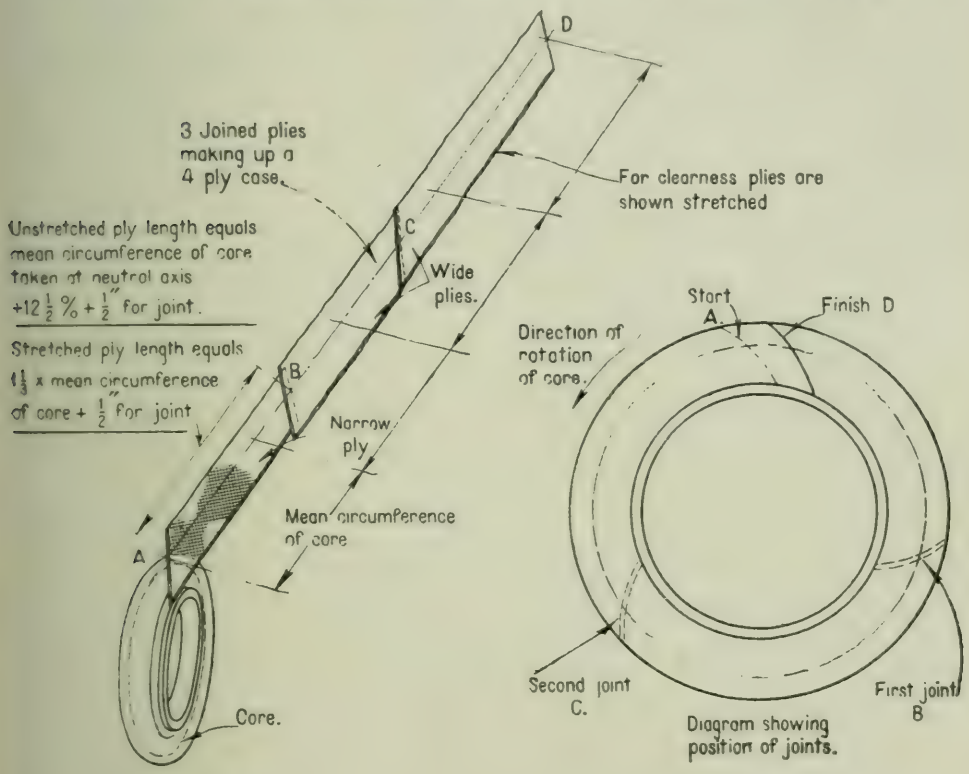

D
3 Joined plies making up a 4 ply case.
For clearness plies are shown stretched
Unstretched ply length equals mean circumference of core taken at neutral axis +12½% + ½" for joint.
C
Wide plies.
Stretched ply length equals 1⅓ x mean circumference of core + ½" for joint
B
Narrow ply
Mean circumference of core
A
Core.
Start A.
Finish D
Direction of rotation of core.
Second joint C.
First joint B
Diagram showing position of joints.

Fig. 41. Fig. 41a.

Figs. 41, 41a.

and 41a show diagrammatically how three plies are formed in one long length for a 4-ply case, and how their joints are arranged so that "overlapping" is obviated, and how the built-up casing has its joints distributed.

Casemaking.

Given a suitably prepared material, the satisfactory performance of a pneumatic tyre depends more on the efficiency of casemaking

methods than on any other mechanical operation in the construction of a tyre. The operation consists in converting bias-cut rubbered canvas strips into the form of a ring-like carcass, having a "horseshoe" shaped cross section composed of a number of plies, and in attaching a pair of bead cores to the carcass interlocked with the plies. In order to properly attach the plies, they must be arranged on the bias so that they can be "stretched."

Fig. 42 is a section through a ply laid on a core, and shows the action that takes place and that a bias-cut strip of canvas which has a circumference corresponding to that of the neutral axis line NA, has all portions "without" the neutral axis line

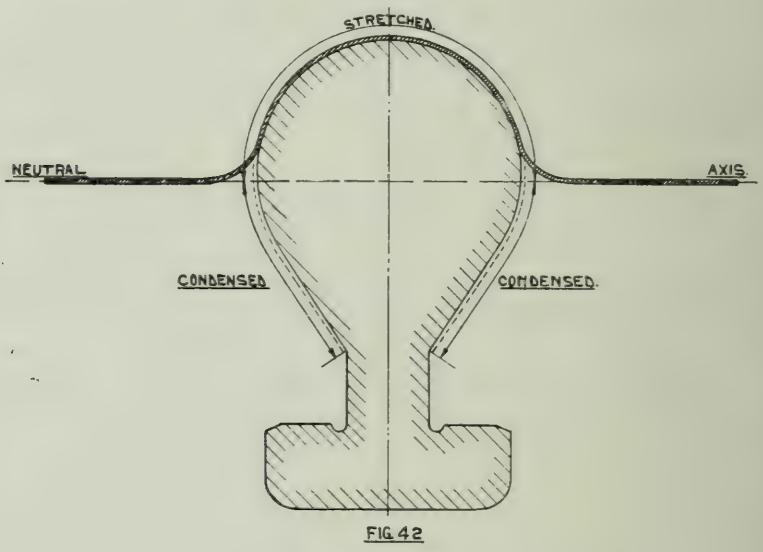


FIG. 42.

"stretched" circumferentially, and all portions "within" the neutral axis line "condensed" circumferentially but stretched radially inwards.

Figs. 43 and 43a, Plate XV, are photographs showing the lay of the canvas warp and weft threads at various points on the surface of the core. These threads in the unstretched canvas form squares and cross each other at 90 degrees. Owing to the stretching of the canvas, the warp and weft threads are angularly displaced, and the squares become diamond shaped at all points except on the neutral axis. Above this line, owing to the circumferential "stretch" and lateral "contraction," the threads form diamonds with their long diagonals lying in a circumferential

direction, the one diagonal increasing and the other decreasing gradually from the neutral axis to the crown O. Below the neutral axis, the formation is the reverse of the above, the long diagonals of the diamonds being disposed radially and the variation from the square configuration increasing with the distance from the neutral axis.

The aim of the tyre manufacturer is to so lay the plies that:—

(a) all threads shall be equally loaded in resisting the bursting action due to inflation and other loads coming on the casing;

(b) all threads shall be equally flexible and yielding when withstanding blows from the road surface or obstacles on the road;

(c) threads shall not be damaged by excessive local stretching or by "puckering" of plies in shaping them to the contour of the core. There are probably more patents taken out in connection with casemaking machines and methods than for any other tyre making operation, but as regards tyres built up from prepared strips on to cores in ring form and of horseshoe section, all well known machines come under one of two types:—

1.—*The "Continuous" Ply-Laying Machine;* or

2.—*The "Intermittent" Ply-Laying Machine.*

1. *The continuous method*, Fig. 44, Plate XVI. In this case the plies are joined together to make a long strip and wound on to the core under tension, each successive ply being shaped and laid against the core progressively and simultaneously. The plies are arranged in such order that the batched length has its joints stepped round the circumference of the core at suitable intervals, as shown in Fig. 41a. With the continuous system it is easy to ensure an even tension throughout the casing, and very little attention is required from the operator to get uniform results.

2. *The intermittent method,* Figs. 44a, 44b and 44c, Plate XVI. The strips are prepared in such lengths that each wraps round the core once with suitable overlap for joint making. They are joined together to form a continuous strip, but after the first ply has been laid on the core the canvas is cut or torn in a bias direction, leaving the first ply in the form of a band round the core.

This ply is now "spun" down at high speed by means of a pair of angularly controlled disks, which are fed inwards towards the centre of the core at a suitable rate in order to condense the canvas and attach it to the core or to the previously laid plies. This operation is repeated until all the plies have been attached. It is feasible by this system to lay down two plies at once and then to spin them down together.

The intermittent system is liable to cause considerable variations in tension at the commencement and finish of each ply, resulting in variations in the angles of the threads at such points.

Satisfactory results depend very largely on the skill of the operator when using this type of machine. By means of the spinning down disks it is, however, easier to lay the plies at a lower circumferential stretch than with the continuous system.

Canvas Tension or Let-off.

Constant stretch or tension on the canvas is most important. The following are three well known methods of controlling the tension or let-off:—

Brake controlled Let-off.

This type, shown diagrammatically in Fig. 45a, Plate XVII, has a serious defect in that it makes no allowance for varying "hardness" of the canvas or for temperature change effects; consequently, accurate stretching depends largely on the skill of the operator. It is better suited to the intermittent type of machine than to the continuous type.

Positive controlled Let-off.

This type, shown diagrammatically in Fig. 45, Plate XVII, is equally effective whether with hard or soft canvas, and temperature variations do not affect the results. The gear does not allow for the gradually increased diameter of the casing as successive plies are laid, and this gives a small increase in stretch on the outer ply. This type is better suited to the continuous type of machine than the "braked" let-off.

Positively controlled compensating Let-off.

This arrangement, shown diagrammatically in Fig. 45b, Plate XVII, lets off more canvas as the diameter of the case increases.

It has, however, a tendency to slippage and to damage the surface of previously laid plies. It is obvious that the means of driving the let-off is through contact of a roller with the surface of the tyre itself. This type is also better suited to the continuous type of machine than the "braked" let-off.

Casemaking Machine Operation.

We will now describe in detail the complete operation of making a casing on a machine of the continuous ply-laying type with a positive let-off. Fig. 46, Plate XVIII, shows a perspective view of a battery of machines each having a stock of raw materials as follows:—

Canvas strips.—On "boxes" from a "buffer stock" stores on suitable stillages; a reasonable number of spare filled boxes are mounted on the casemaking machine as shown, thus the operator never has to wait.

Beads.—A supply of beads ready solutioned and handy on the core turret.

Cores.—A stock of these, both bare and with casings on, is carried on a suitable turret situated in front of each machine, which has two horizontal bars supported by a vertical pivot and capable of being connected up as desired to the casemaking machine, either to put on a new core or take off a finished case and core. This system prevents delays, and the operator of the casemaking machine is able to load and unload the machine without assistance. No lifting is required, only sliding horizontally. The cores, both empty and full, are transported to and from the core turret on suitable stillages by means of elevating trucks.

Sequence of Operations.

These can be broadly stated as follows:—

1. Simultaneous laying of prepared strips from stock rolls.
2. Guiding strip as fed on to stretching or shaping means and re-batching the lining.
3. Stretching or shaping the plies before application to casing.

4. Eliminating any wrinkles or puckers which may have formed in the stretching operation.

5. Attaching the stretched and formed ply to the surface of the core or to previously laid plies.

6. Laying on plies to the core in proper form, particularly the portions of the plies that lie within the neutral axis line.

It is understood that these operations on the continuous laying type of machine are all carried out at one and the same time until the length of batched up strip necessary to make up the tyre has all been "run on."

Machine controls.—Figs. 46a and 46b, Plate XIX, show the type of machine concerned, and the main features of it which require movement or adjustment on the part of the operator.

Operation 1

is to set the suitably prepared inside bead upon a setting ring or spider, which is centred by the spindle on the casemaking machine, so that at the proper time this bead can be attached to the casing and be perfectly concentric with other parts of the casing.

Operation 2.

The core from the previously loaded turret, as shown in Fig. 46, is made to slide across and secured to the spindle of the machine by a suitable "C" shaped washer which does not require the nut to be taken off the machine and thus can be quickly withdrawn.

Operation 3

The core has been suitably solutioned so that the starting end of the canvas can be properly attached to it and will adhere. Suitable shaping and feeding arrangements ensure proper stretching of the canvas on to the core.

The first end of the canvas is run on—initial tension being taken care of by means of a canvas leader or short piece of canvas attached to the first ply, which is torn off when sufficient length

is laid on the core. The purpose of the canvas leader is solely to ensure correct tension at the start of the first ply.

The operator now brings into action a ply-laying head having reciprocating fingers which move radially inwards, stroking down the plies and attaching them to the previously solutioned core surface. This operation is continued until the whole of the inside plies are laid on, when the machine is stopped and a slit is made at each side of the casing allowing the canvas to be pushed upwards so that the beads can be set outside the inner plies.

Operation 4.

Bead setting now takes place, the outside bead having been suitably placed in another setting ring, this outer ring being centralised by contact with suitable portions of the tyre core, and by means of two screws with handles the two beads are brought hard together and attached to the core.

Outer ply laying.—This is done by means of a cradle which is provided with quite a number of small rollers each laying down a portion of the plies round the bead, so that the plies are gradually folded over and properly attached to the bead. This cradle can be lowered away from the tyre casing when required.

Operation 5.

After all the plies have been laid on, the canvas is torn off on the bias whilst still under tension in such a way that the finish of the plies just overlaps the commencement of the tyre.

The surplus canvas is now cut away or trimmed at the bead edges by means of a swinging attachment having a downwardly pressed lever which presses two cutting out knives against the ply edges below the bead.

These knives are compensated to allow for rocking action and very accurately trim out the edges. It is very necessary to reduce the amount of trimming as far as possible, but in practice some slight trimming is necessary to ensure close working to volumes at the bead toes.

The making of a tyre on such a machine takes from four to twelve minutes, according to the number of plies incorporated in the casing. It is apparent, however, that with the progressive

system of working incorporated in this type of machine the action is continuous excepting only for the stop necessary to attach the bead rings. It is obvious, too, that with this type of installation the operator has all his plies brought to him, and does not require any help from other workers. It is a fair example of the application of "buffer" stock methods with a view to getting high efficiency from operators and eliminating stoppages due to irregular supplies or to the temporary absence of workers on whom operators depend for their supplies.

Transport of Tyre Casings on Cores.

The tyre casings, together with the cores on which they have been built, are taken from the casemaking machine turret and loaded on to a stillage for transportation to the rubbering section.

Rubbering, Strip and Tread fitting, etc.

Fig 47, Plate XX, is a photograph of the rubbering section taken from the centre and looking towards the casemaking machines.

Fig. 48, Plate XXI, is taken from the centre, looking in the opposite direction.

In the first view will be seen a turret examining post A with a group of stillages B containing finished cores. At this point the cases are carefully inspected and gauged before being passed for subsequent operations.

The box-moulding system, i.e., moulding of a tyre (built on a rigid core) in a definite confined space between two rigid bodies, makes it necessary to work to very fine limits, both for shape and volume.

The rubbered thickness or "gauge" of prepared casing plies varies in practice in spite of the great care taken at all steps in the preparation of the canvas. It is therefore necessary to carefully gauge the cases and to make due allowance for this variation by varying the thickness of calendered rubber fitted to the casings. This is done by means of a C-shaped gauge, shown in Fig. 49, Plate XXII, which indicates on a dial the thickness of rubber which has to be applied to each side of the case in order to give the correct overall dimensions necessary for moulding.

After the case has been passed by the inspector and gauged, it is lifted off the examining post by the hook C, which is sus-

pended from the overhead runway D. This runway passes over a battery of rubbering stands E, on each of which is mounted a vertical pivot carrying two swinging arms, each of these arms in turn being provided with a centre suitable for mounting a core. Each of these centres is arranged with a pair of collapsible leaves to form a safety device, which effectively prevents the core falling off the centre and injuring the operator. The leaves are arranged with heads of part spherical shape, so that on the core being presented to them they collapse inwards, allowing it to pass, and when the core is in position expand again under spring action and lock it securely. The arms can be swung through the complete circle in a horizontal plane, in addition to which each is separately pivoted on a horizontal axis so that the tyre can be held in any desired position for the convenience of the operator.

A runway is situated over the extreme outer position of the circle of swing of the arms for loading and unloading.

When rubbering the arm is swung back to the side remote from the runway.

As previously mentioned, all the sub-units necessary to complete a raw tyre have been previously assembled as a rubbering unit in a suitable multiple spool. Several of these spools will be seen lying under the tables in Fig. 47, Plate XX.

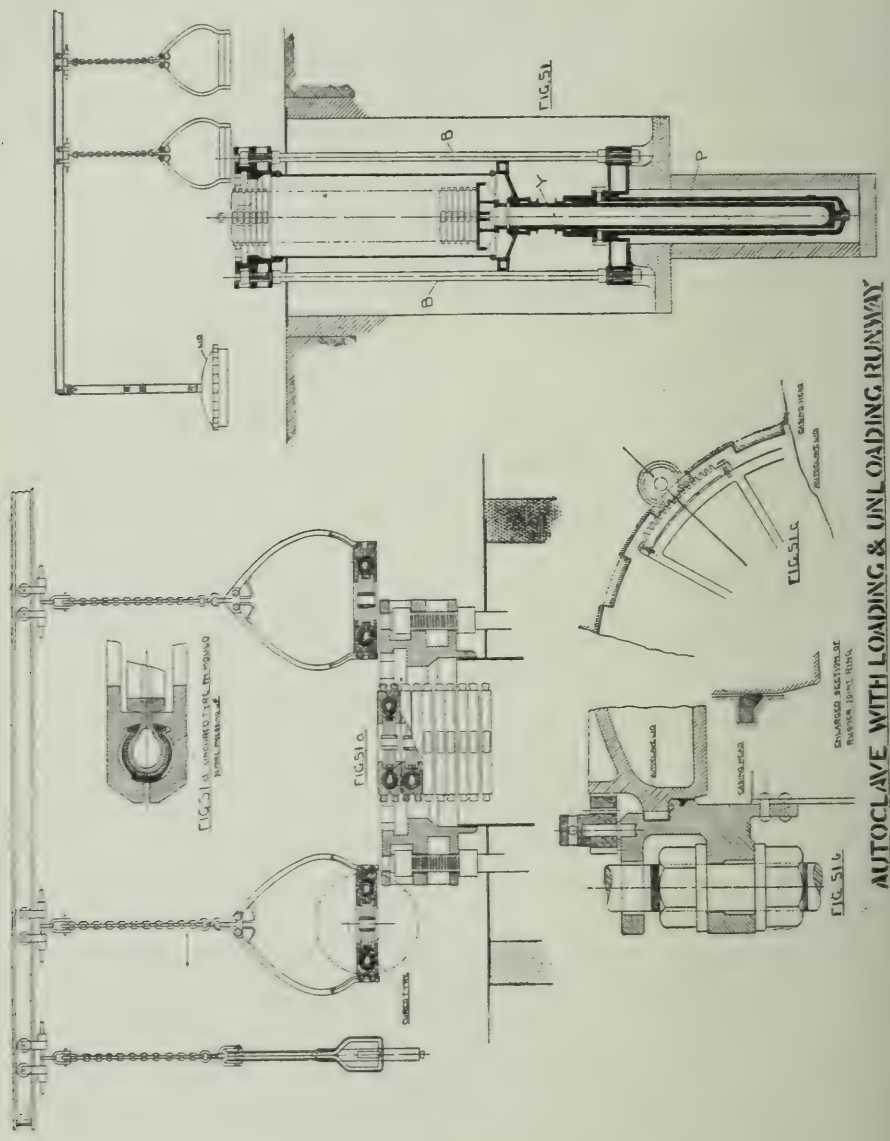
On the top portion of the table the operator unrolls as required each of the strips which have to be applied to the tyre casing. A strip of rubber with its spool and lining unwound is shown at G.

The bead strips which have been previously assembled together are first applied to the casing, where they are carefully rolled down by means of a hand rolling tool shown in Fig. 50, Plate XXIII. Care is taken that they are well rolled down and that no air is trapped between them and the casing. The next step is to put on the pad rubber. This is rolled down against the casing by means of the flat roller shown in Fig. 50a, Plate XXIII.

The breaker strip (if used) is next applied and rolled down in a similar manner, after which the undertread and tread are applied.

When the case has been completely assembled, it is again gauged by means of a similar gauge to that previously described, and

if passed by the examiner is again loaded on the runway D and passed into stock as an uncured case, this stock being carried on stillages with horizontal arms, as used in the casemaking department. Fig. 48, Plate XXI, shows this stock at the far end of the lines of rubbering stands.



AUTOCLAVE WITH LOADING & UNLOADING RUNWAY

FIGS. 51 to 51c.

MOULDING AND CURING. AUTOCLAVES.

The modern tyre curing vessel has been evolved from a simple horizontal "pan" into which bolted moulds were placed and stacked together in piles by hand methods.

French engineers developed the "autoclave," Fig. 51, as a vertical curing vessel in conjunction with a vertically moveable hydraulic ram Y operated from a cylinder P connected by bolts B to the steam vessel. This device is almost universally used for large tyre manufacture and is a great labour saver. There are many different designs in use, but most of the well known tyres are cured in machines which are in principle similar to Fig. 51.

The section shows the cylinder and hydraulic ram, ram table with a pile of moulds on it, and overhead conveyor with suitable lifters by means of which the moulds are loaded into and unloaded from the autoclave.

Fig. 51a is an enlarged section through the head of the autoclave.

Figs. 51b and 51c show details of the bayonet type joint for the lid of the steam pan.

Fig. 51d is a section through an assembled mould with an uncured tyre in it before being "pressed up."

Mould Handling System used for supplying "Autoclave" Presses.

There are many systems in use for the filling and emptying of moulds, and for handling them into and out of the presses, the simplest and most laborious being that of lifting the moulds by the operators with gloved hands. The most complete systems employ a complete installation of conveyors, both on the tables and overhead.

The system to be described is one designed to handle one tyre every forty seconds on each line of five presses of 12 ft. capacity each, and the aim has been to avoid using complicated conveyors with their liability to serious hold-ups in output due to breakdown at any one point, and to be able to change the sequence of operations without having to change expensive plant items.

Fig. 52, Plate XXIV, shows at the far end a row of presses in which the tyres are to be cured. Each one is charged with anything up to thirty-three tyres per cure, according to the tyre section.

The organisation is such that a press "blows" every twenty-one minutes, so that each twenty-one minutes a "heat" of tyres must be dealt with.

At the point "A" there are stillages "B," each having about five tyres on it arranged according to the programme of sizes to suit the moulds to be filled. The endless triangular runway C, with suitable trolleys D and pendant hooks E, enables tyres on their cores to be unloaded from the stillages.

When the bottom half of a mould arrives at the point F, the moulder runs a tyre above this and elevates the table G by the hydraulic ram H, at the same time unhooking E. The tyre and bottom half slide on rollers to the table J, which is elevated by another ram, thus enabling the top half of the mould K to be placed on the tyre.

The operator then puts the hooks L on to a pair of lugs on the top and bottom mould halves, depresses the table J and away goes the filled mould with a push down towards the press to be filled. Fig. 51 shows a section of the press with moulds being put in. A mould suspended from its hooks is placed above the previously piled moulds, the "ram" is raised by depressing a valve lever and up comes the previously piled mould, letting the operator release the hooks of the new mould; the hooks then swing away to let the next mould be dealt with. The ram, of course, gradually descends as the moulds are loaded.

Figs. 3 and 3a show the section of a tyre complete in the mould, pressed up. These moulds are clamped together thoroughly, seeing that the hydraulic pressure on the ram equals 120 tons and upwards, according to the size of tyre dealt with.

The type of valve used for ram operation is one in which high pressure water cannot be admitted until such time as the press is filled up and closed, thus preventing the operators from wasting expensive hydraulic power. The low pressure system works at 400 lb. per sq. in., and the high pressure system at 1,500 lb. per sq. in.

After curing, the moulds are discharged and transported again on the runway to the mould-opening point M, where again a ram-operated table rises and allows the mould to be unhooked. Levers applied by hand open the mould top from the tyre, and the top is taken along on the runway as at N N, where it is cleaned for use again at the point J. The tyre and the bottom half of the mould slide on to another rising table P, at which point the hook R is inserted, the table depressed and the tyre on the core slides on the transverse runway S to strippers. The bottom of

the mould is raised by P again and slid off down the cleaning table T for use again at the point F.

Stripping off the tyre from the core is done by sliding the core on to a turret U, at which point "strippers" loosen the tyre on the core and "soap" the inside of the tyre so that it can be "stripped" from the core. The cores run on another runway, when stripped, to cleaning and solutioning turrets V, from which they are again transferred by a runway to stillages W ready for return to the casemaking department. The tyre, after stripping, is thoroughly washed with warm water and very carefully examined. Valve notches are then cut, the inside surface is painted, and it is then passed to stock as the finished article.

Transporting Methods.

Tyre manufacture is essentially "mass production," but unfortunately many sizes are required, so that the factory must be laid out to suit a programme covering numerous sizes and a number of patterns.

It is seldom possible to arrange the flow of work in such a way that conveyors can be used. The transportation methods covered in these notes are based on the principle of moving "units" or bunches of units.

So far as possible the container used during transport is used as the container or "support means" between, and even during, operations, and also at such periods as the units in question are considered as "buffer stock."

In compiling these notes, there has been no reference to previously published matter, as there is little or none on record; thus, it appeared advisable to deal with the matter as an example, demonstrating the broad principles on which tyre manufacture is carried on.

S.S. and B.E. Tyres.

The advantages of other types of tyres, such as the modern cord type of construction, and the Straight Side method of attachment, might well form the basis of further matter in reference to their design, production and usage.

The Effect of Unsprung Weight on the Tyre.

It appears likely that very greatly improved service will be obtained from tyres which do not have to withstand the "pounding" due to heavy unsprung weights. Improvement could be looked for in the direction of:—

1. Increased casing life due to reduced flexing.
2. Increased tread wear due to reduced "spinning" and consequent abrasion which takes place owing to tyres bouncing on rough roads.

A test such as the following suggests itself to prove the advantage of low unsprung weights:—

1. Compare results on two identical cars having the same total loads per wheel, one car having a lighter body than the other but with weights bolted to its axles near the wheel seats, the tyres being identical.
2. One car with the same load on each back wheel, but one end of the axle having extra load near the wheel seat, the other end having extra unsprung load above the wheel seat.

An analysis showing the unsprung weights on different cars would probably at once explain why some makes of car use up their tyres much more quickly than other cars having approximately the same engine power, back-axle load and body type.

THE DISCUSSION.

Dr. W. R. Ormandy, in opening the discussion, said: This is the first paper that has been read before the Institution on this important subject, and I think that the things which the author has not told us are quite as interesting as those which he has told us. The question of the tolerance in rims appeals to me very much. It does seem an iniquitous thing that if the owner of a motor car happens to have a rim the tolerance of which is on the extreme limits, serious damage may be done to the tyre and its life reduced 2,000 or 3,000 out of a possible total of 8,000 or 10,000 miles just on that account. If the Americans can produce rims in which the variation is so much smaller, it seems to me that this Institution, together with the S.M.M.T., ought to take steps to ask the manufacturers of rims to make something which is less likely to cause trouble than the existing ones. The author told us that some tyre makers allow a possible extension in the bead of 1 per cent and others allow 4 per cent. When the tyre happens to have the limit of 1 per cent, and the rim happens to be to the limit in the other direction, the situation becomes impossible. The author has not dealt with cord tyres, but has kept severely to the ordinary canvas built-up type, and when we realise the large number of operations in its manufacture, and the amount of hand work that there is in it, is not it rather a marvel that the tyres last as long as they do? Possibly the large amount of hand labour accounts for the fact that, in spite of the price of rubber getting lower and cotton coming down to one-third, tyres still remain very dear.

The author dealt with the subject of the canvas employed, but he skimmed over this delicate subject with a great deal of skill. He stated that tyres are generally made of Egyptian cotton, and that they are woven with equal tension on the warp and weft. That is desirable, of course, because the threads are to be used at an angle of 45 degrees approximately to the direction in which the wheel runs. He said very little about the subject of twist on the thread. Now, the strength of a cotton thread is to a very large extent a function of the amount of twist—the number of turns per inch of the thread—and it is easy to get a thread of American cotton which has a shorter fibre at about two-thirds the price of

(Dr. W. R. Ormandy.)
Egyptian cotton which is very much stronger if there is a bigger twist in the American thread and less twist in the Egyptian. Yet the tyre manufacturer has to use Egyptian cotton; he does not really want to do so, but he must if he wishes to get the best results, and it is rather an important matter. I should like to know why the tyre maker prefers a very low twist on his thread when he can get vastly greater strength, not 1 or 2 per cent, but 200 or 300 per cent more, if he puts more twist into the thread. Perhaps it has a bearing on the point raised further on in the paper, that the canvas is so prepared by the cotton spinner and weaver that when it comes from the soaking machine it is penetrated by the rubber. All I can say is, that if any firm can really get the rubber into the thread, they have done something that I have never been able to do. I have dissolved rubber in all sorts of solvents, $\frac{1}{2}$ per cent, 1 per cent, and 10 per cent solutions, boiled it in vacuum, put it under pressure, and even then, when the rubber is dried and the thread is untwisted, every fibre is perfectly free from rubber. Either the colloidal particles of the rubber are so big that the cotton threads act as a filter, or, when the drying operation takes place, the rubber particles are pulled out. I should like to point out that obviously there are a tremendous lot of difficulties in making a tyre, and there are many more than those which have been brought to our notice. The length of time a piece of rubber is in the mixing machine, and the length of time taken in getting the mineral matter into it affects the period of its vulcanisation, that is to say, the amount of work done on the rubber affects the time that is taken in vulcanising. That the tyre manufacturer can get a bead which is half vulcanised to start with, so that, when it is joined up with various plies of canvas and put into the mould and the final vulcanisation takes place, not only is the bead just cured to the right extent and to the required degree of elasticity, but the rubber between the various layers of canvas is just cured and not over-cured, as well as the outer thickness at the top, means a marvellous co-ordination of work, and it cannot be done by any theory. Every single step has to be arrived at by the ordinary process of trial and error. I would like to ask the author a question with regard to the time of curing. Various chemicals can be mixed into the rubber so that it will cure quickly or slowly, and if the cure is arranged to be completed in, say, ten minutes, several different periods are involved. The first period is occupied in getting the mould hot:

then there is the time before the outer surface of the stuff gets hot enough to begin vulcanising, the time required to pass through the thinnest portion, then there is a certain period of time when everything is in equilibrium and the whole thing hot, and then the steam supply is shut off and the mould cools down again, reversing the series of events. The thickest part cools most slowly, and the thinnest part most rapidly. If the total time of vulcanisation is ten minutes, then an error of a minute or two is a serious error. The Japanese, when they made a specification for a rubber cable, realised the fact that errors are introduced by this heating up and cooling down, and they stipulated that the vulcanising time should be six hours, no more and no less. Moreover, they were not satisfied to specify six hours, but they sent a man down to sit through that period and watch it. The Japanese are the only people I have ever heard of who got exactly what they paid for in a rubber specification. I would like to know what is the length of time that is taken in the cure. In tyre manufacture the moulds are the most expensive part, and there is a great temptation on the part of the maker when he is using a one-piece cure to get the moulds out again as rapidly as possible, but the more rapidly the moulds are turned round the greater the effect of slight errors in the mixing or in the time the tyre is under the cure. The quality of the rubber and the durability is enormously altered by slight under- or over-vulcanising. I must be allowed to pay a very hearty tribute of thanks to the author for his paper, which gives a really able explanation on broad lines of how a tyre is made from beginning to end.

Col. A. E. Davidson: My remarks are made solely from the point of view of the user. On page 395 the author states that Straight Side tyres are comparatively unknown in Europe in spite of their great advantages, and I should like to know why it is that we do not get them in Europe. Is it that the manufacturer of the tyre cannot produce them because he has not got the necessary plant, or is it the car designer who is not providing the proper sort of rims? As users I think we are falling between two stools, because I am quite sure that the Straight Side tyre is the better one. From experience of the two types on a certain make of car, the average mileage with the Beaded Edge tyre was about 2,000, and with a Straight Side tyre about 7,000. We all know the difficulties that are met with in tyres with beads. To start with, they are so tight that they cannot be got on, and

(Col. A. E. Davidson.)
when they are nice and easy to go on, they come off without warning. In France we had a lot of trouble with tyres caused by the beads coming away from the rims. On page 437 mention is made of the cord tyre. The figures given above for the extra life of the Straight Side tyres may possibly not be due entirely to the use of these as against the Beaded Edge, but partly to the fact that the Straight Side tyre was made on the cord system. Many cord tyres with beaded edge that have been tried have failed after a short mileage at the junction of the bead and the wall, and if that point of weakness had been eliminated, then the cord tyres would have given very much better service than those with canvas fabric. In this connection, errors in rim dimensions would appear to militate more severely against the Beaded Edge tyres. On page 438 is mentioned the effect of unsprung weight on tyres and their life. I have no comparative results with pneumatic tyres, but with solid tyres I was able to make some observations, and could not trace that tyres on light, i.e., chain-driven axles, ran any greater mileage than those on much heavier live axles. In fact, the figures rather indicate the opposite, namely, that the live axle gives a better mileage than the chain-driven axle, and this leads me to suggest that unsprung weight is only one factor, and not the most important, in tyre wear. I should like to suggest that the suitability of the spring in conjunction with the unsprung axle weight is really a more deciding factor. Watch some vehicle unsuitably sprung—say, a heavily sprung 5-ton lorry running empty. Notice what blows are transmitted through the tyre to the road, and how the whole vehicle jumps about on a rough road. The shocks on the tyre cannot be attributed to the unsprung weight of axle, but to the unsuitability of the springs to the load. The same vehicle fully loaded will jump about much less. In short, it is held that if chassis makers could introduce on their vehicles a spring variable according to load, tyre shocks and road shocks would be reduced. It seems to be only reasonable to assume that other conditions, such as clutch, engine, driver, etc. being equal, tyres will last longer on a vehicle which holds the road well—that is to say, of which the springs are suitable for the weight of the axle and the weight on the chassis at the time—than on one which holds the road badly and of which the wheels are always leaving the ground, and consequently frequently spinning and then being pulled up.

Mr. O. D. North: I must express my gratitude to the author

for his wonderfully clear and thorough exposition of the art of tyre making. It is good to be made to realise the difficulties met with and the compromises necessary in other fields of labour than our own. The author points out the restrictions of clearance imposed on the tyre designer who wishes to develop a tyre with thinner walls, larger section and lower air pressure than usual. Can he tell us the extent of the advantages that would accrue if he had a free hand in this matter? In other words, can he indicate the proportions of what he regards an ideal pneumatic tyre made at reasonable cost?

As I am just putting a new wheel on the market, I am naturally concerned with the probable trend of tyre design, and the author's powerful arguments in favour of the Straight Side tyre with inextensible bead are of great interest to me. The fact that this type is easy to manufacture will naturally be one of the biggest factors tending to its adoption; I can only express the hope that this fact will also be reflected in the selling price!

Does the author consider that the general proportions of the American S.S. rims and tyres make the fullest possible use of the anti-rolling feature? Could the tyre design be improved if the rims were modified? It would be a thousand pities if, right at the beginning of the manufacture of the Straight Side tyre in this country, the very best possible proportions were not standardised.

Particularly does this apply to the so-called "Giant" tyres, and I should regard it as a calamity if the American 24 in. rim were the only one standardised. Something smaller, say 18 or 20 inches, is badly needed if the advantages of pneumatic tyres of large section are not to be hampered in realisation by their preposterous overall diameter, which excludes them from many fields where they would be of great value.

I have been told, by parties not strictly disinterested, that with the "Giant" tyres there is sometimes a tendency for rolling strains to lift the toe of the bead from the rim and thus cause a tube nip, and a cursory inspection of the tyre sections would seem to support this. The walls of the tyre are very thick and the toe of the bead is very short, offering very little leverage for the air pressure to restrain its movements. Could the author tell us if there is any truth in this?

The author's design appeals to me as a fairly easy job for the mould-maker, but it hardly looks circular enough to have a low

(Mr. O. D. North.)
rolling resistance. Too much energy would seem to be dissipated in kneading the corners of the knobs for the tractive effort to be low. Probably the pedal cyclist, who has to supply the energy himself, is the best judge of a tread pattern from this standpoint.

I wonder if the author would care to take us into his confidence on the subject of tread mixtures and vulcanisation? I have noticed enormous variations in the tread life of tyres, and I certainly think some tyre makers have good and bad years, and I should like to know if—given an absolutely free hand and regardless of cost—it would be possible to compound a tread mixture that would give a longer life than those at present in use. If not, could the author say if he thinks a tyre should be designed to stand re-treading at least once?

The one thing that has opened my eyes more than anything else is the author's diagram on page 408 showing the dead-load distortion of a properly inflated tyre. It is much greater than I had realised, and I marvel that casings can be produced to stand this sort of thing for 10,000 miles. Quite apart from the local massage of the tread surface already referred to, we have in this continual deflection of the casing a very appreciable loss of energy, energy not merely lost but diverted to evil ends.

It is sometimes forgotten that the steel wheel on the steel rail is the most efficient combination, giving a rolling resistance of only 15 lb. per ton. Next to this comes the steel tyre on a smooth hard roadway, and then the solid rubber tyre under similar conditions. At high speed on rough surfaces the energy losses due to impact give the advantage to the more yielding tyre. And again on very soft road surfaces at slow speeds the pneumatic should give a lower rolling resistance than a rigid tyre, for the yielding pneumatic tyre distributes the load over a wider area, reducing the intensity to such an extent that the soft road bed is not displaced appreciably. The hard tyre, however, localises the pressure and displaces the material downwards and to each side, using energy which is not in any part given back on the receding side of the wheel, since the average road material is plastic but not resilient. Thus, the hard wheel on a soft road is, as it were, always running uphill, whereas the pneumatic may be pictured as running up a hill in front but also running down a lesser hill behind.

Given reasonably hard roads with a smooth surface, there is little doubt in my mind that at ordinary speeds solid tyres give a lower rolling resistance than pneumatics, unless the latter are

over-inflated, and I can well believe that by stiffening up a pneumatic with internal filling pieces of rubber and using a smaller air tube, deflection under load, rise of temperature, fuel consumption and comfort would be reduced. The logical trend of that line of reasoning seems to be towards the solid or cushion tyre. The trouble is, however, that many of our roads are in such a condition that comfort of passengers and reduction of shock on axles compels the use of the yielding pneumatic tyre.

Major Wheeler: Without in any way disparaging the author's remarks on the purely technical side of the subject—I refer to the manufacture—I am sure I shall be echoing the views of several if I say I would rather have had him devote the second part of his paper to, say, "tyres, their ills and cures."

I should like to make a reference to the design of the internal section of the tyre. When air is pumped into a tyre we get a fluid in compression, and a compressed fluid always tends to make its containing cover or shell truly circular. Consequently, if the internal section of the tyre is designed oval, or, as we have seen from the diagrams, rather egg-shaped, a greater pressure is necessary to hold the cover to that shape than would be necessary to maintain true circularity. It seems to me, therefore, that the cover at certain parts has to be strengthened more than need be the case if a truly circular interior were designed. If my contention is correct, it is surely possible to make a saving in material with the usual accompanying advantages, and I should like to hear the author's opinion on that particular point.

Mr. Macbeth, in replying on the discussion, said: As the same questions were in some cases asked at more than one of the meetings, I have dealt with each point as one without special reference to the speaker.

B.E. rims.—Rim tolerances.—In reply to Dr. Ormandy, I would say that rims made by tyre manufacturers who are responsible for tyre performances are liable to be much more accurate than rims made by firms who do not make tyres also. Checking of rims is not organised in this country, thus it is possible for rims to be put on cars without proper supervision: only organised checking control can deal with this matter. Expert examination is required, as rims may pass the gauges and still have defects, as, for instance, sharp lips or square lips. It is also necessary to ensure that a rim which may have been right when made is not subse-

(Mr. Macbeth.)
quently distorted in fitting to wood felloes or when spoking up into a wire wheel; it is in fact necessary to see that rims are correct when the wheel is finished.

Stretch percentage.—A tyre designed for 1 per cent stretch on a "mean" rim will still have 0·5 per cent stretch when fitted to a "minimum" rim, the variation in circumference of lips of rims being practically 0·5 per cent up and down from "mean" figures.

Mr. North asks if American size S.S. rims are satisfactory, and make the fullest use of the anti-rolling feature. I think they do, and that such rim standards are admirably designed to suit the sections fitted to them. I do not consider that S.S. tyres of standard sizes could be improved by altering standard sections of rim.

Unsprung weight.—In reply to Col. Davidson, I would say that satisfactory springing must be considered in conjunction with unsprung weight, and is certainly of at least equal importance. I do not agree that shocks on tyres cannot be attributed to unsprung load, as it is only necessary to observe a car or lorry running on a "pot-hole" road, when the chassis may be comparatively free from vertical vibrations, and yet a heavy axle will "pound" and bounce, whereas a light one will remain generally in contact with the road surface. Even if springs are in good condition, their design has a great bearing on the unsprung weight, as is obvious when comparing cantilever springs which reduce unsprung weight with semi-elliptical springs which increase it.

Col. Davidson instances a badly sprung 5-ton lorry as imposing great shocks on tyres when empty. While it is true that with modified springing such a vehicle when running light would hold the road better, yet it is also a fact that if the unsprung weight had been reduced it would have very materially helped the unloaded vehicle to maintain the wheels in contact with the road.

Cotton.—Amount of twist.—It is a fact that excessively twisted yarns are not readily insulated; hard twisted yarns are also very liable to cause early failure due to frictioning action between warp and weft. The effect of impregnation and vulcanisation has a very important bearing on the strength of the yarns in the finished tyre, as it is obvious that a yarn which is very little twisted will be stronger when its fibres adhere together due to the presence of vulcanised rubber between them.

Impregnation.—In practice impregnation does satisfactorily insulate thread from thread when properly carried out.

Vulcanisation.—Time of cures.—This varies for different types of tyres and different methods of moulding and vulcanising. Thus, a light cycle tyre cured on the Doughty Press can be perfectly cured in three minutes, whereas a solid tyre made in a heavy mould may take four hours or more. As I have already pointed out, the methods used in the factory, particularly the curing methods, must be so regulated as to ensure that quality has the first consideration.

S.S. rims.—Col. Davidson asks why S.S. tyres are not obtainable in Europe. They are obtainable from some British manufacturers. There are several reasons why the S.S. tyre was developed earlier in the U.S.A.; they are principally as follows:—

American manufacturers were not so successful in making a sound B.E. type, suffering as they did very seriously from rim cutting due to faulty bead design and construction.

Chauffeurs are scarce in the U.S.A., therefore easy fitting became more imperative.

The S.S. rim has only lately been adopted as a British standard, and war and post-war conditions have made it extremely difficult to equip British rim and tyre factories for the manufacture of wheels and tyres to suit the standard adopted.

The ideal design of tyre.—In reference to the making of a tyre when the designer does not require to conform to standard dimensions, I would define this as one allowing of increased vertical displacement under shock or when encountering road obstacles; such a tyre having larger section, thinner walls and lower air pressure requires a special rim to accommodate it in order still to retain a normal outside diameter.

Giant pneumatics.—There is already a call in the U.S.A. for a giant pneumatic tyre with about 20 in. rim diameter, and such a tyre would better suit omnibus requirements than the large diameters having 24 in. wheels. There is no trouble at all with giant tyres due to tube nipping, in fact all such tyres make use of a "flap" (as in Fig. 14) which effectually prevents such nipping on all sizes of S.S. tyres.

Giant tyres are not so liable to puncture as smaller and thinner tyres, in fact only the strongest and stiffest kind of spike would remain unbent when attempting to pierce a giant tyre with its reinforced tread.

In case of deflation giant tyres have not proved a source of

(Mr. Macbeth.)
danger from overturning, the result being the same as when a solid tyre comes off the wheel, when in effect the vehicle drops some 4 in. to 6 in. at one side and rests on the brake-drum.

Tread design.—Non-skid patterns which have a good fore-and-aft grip cannot well be as efficient against rolling resistance as plain or ribbed patterns and still retain their great "safety" features. The particular pattern illustrated has, under normal running conditions, a comparatively narrow track, and is designed to give little rolling resistance, but great gripping powers even under the worst conditions combined with maximum life as a non-skid and as a casing protector.

Tread mixings.—As already pointed out, these can be varied at the discretion of the manufacturer, but it is well understood by tyre manufacturers that a tread having a high load as compared with the road contact surface must be well able to resist cutting and abrasions. Tyre makers generally use the best mixings they are able to make, and a steady improvement has been made in this respect. In fact often the tread is more durable than the casing.

Tyres.—Their ills and cures.—Major Wheeler raises a point of very great importance, and I would say that a volume of useful matter could with advantage be written on this subject. If the average user would carefully study the literature already published by the leading tyre makers on this subject and would then spend a little of his time examining into the condition of his tyres so that they get reasonable attention as given to other parts of the car, the increased service from the tyre would be very appreciable.

I suggest as a simple system the following:—

1. Use each of five tyres in turn as the spare.
2. Change the position of one tyre each week.
3. The tyre taken out of service to be taken off the rim, examined, repaired if necessary, thoroughly cleaned inside and outside, all rusty scale removed, and the inside thoroughly dried.
4. The rim examined, dents taken out, sharp edges filed, rust removed and the rim repainted.
5. The tube examined and repaired, the valve examined, and the tube refitted to the spare wheel.
6. It is obvious that in following out such a routine system it will be an advantage to have in the garage a sixth tyre and tube so that in making the routine changes the spare

wheel can be refitted with a tyre already attended to, and thus the spare wheel will not be held up except when requiring attention to the wheel itself.

I would suggest that the user should consider his car tyres as he does his shoes. Thus clean and dry them as required and repair them soon enough so that the "uppers" remain sound.

Shape of section.—The circular section of tyre is very desirable, and tyres generally are designed as nearly as possible to give a circular section when inflated. Square woven canvas makes some distortion inevitable by reason of the fact that the outer portion of the casing is stretched circumferentially and the inner portion radially inwards. Also, distortion of the bead portions due to stretch on the rim has a bearing on the resultant sectional shape.

Tyres on mud surfaces.—The value of pneumatic tyres (due to the small increase of area in contact with the road with increasing load as compared with solid tyres, as pointed out by Mr. E. B. Wood) is more marked still when it comes to the use of giant non-skid pneumatics which enable heavy loads to be carried and hauled over snow and soft ground where solid-tyred vehicles cannot be used.

Effect of exposure to heat and sunlight.—Tread and side rubber mixings when properly compounded can and do withstand such exposure well without hardening or cracking, and the ability to withstand such conditions is taken fully into account by reputable makers. Another point which must depend on the user's vigilance to some extent is the increase in internal pressures in tropical countries. It is obvious that, under hard driving conditions, tyres used in hot countries are subject to periodic over-inflation, and unless this point is watched mileage results will be very appreciably affected.

The use of security bolts is not advisable with a properly designed tyre—in fact, their use is responsible for much nipping of tubes—and to-day the leading tyre makers do not advocate their use. The frictional grip of the beads of the inflated tyre against the rim is so much in excess of the grip of the tread on the road that creeping cannot take place unless the tyre is run absolutely deflated.

In conclusion, I think that if this paper makes the tyre people and the chassis people come closer together, it will do a lot of good to the tyre manufacturers. The tyre manufacturers are in very

(Mr. Macbeth.)
close touch with one another, but they seldom talk about technical matters to each other. I think it is thought that the tyre man is a rule-of-thumb expert, but that is not the case at all. Tyre making is a very strenuous operation, and personally, as an engineer for about the last twenty-five years, I think there is more applied engineering in tyres than in anything else I have ever seen.

COMMUNICATION.

Mr. E. B. Wood wrote: When in Mesopotamia in 1916 with a view to explaining to the military authorities the reason why a pneumatic tyred lorry was more suitable than a solid tyred lorry for use on the stoneless plains of alluvial mud of that country, I made some investigations as to tyre impressions. I found that the general characteristics of the pneumatic tyre are that, with increasing load, the length/breadth ratio of the impression remains approximately constant, and the pressure per sq. in. of area rises very little, as is to be expected, the pressure on the ground at the centre of the impression probably differing little from that near the edges. In the case of the solid tyre, the greater part of the extension is transverse, i.e., parallel to the axle. With a 50 per cent increase of load, the length increases only 3 per cent and the breadth 23 per cent. The length/breadth ratio decreases and the mean pressure per sq. in. increases with the heavier loads. In this case, however, the pressure at the centre of the impression is probably some 50 per cent higher than the mean pressure, with a correspondingly greater destructive effect on roads made of dry mud.

I should like to ask the author if he could give us any information on (1) the action of high atmospheric temperatures, and (2) the action of sunlight, on tyres. Though I have no data, from a photographic point of view I did not find the actinic value of Mesopotamia sunlight quite as high as I expected.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

FEBRUARY, 1921.

The Eighth Ordinary General Meeting of the Session was held at the Chamber of Commerce, New Street, Birmingham, on Thursday, February 24th, 1921, at 7 p.m., Sir Henry Fowler in the chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper* was then read and discussed:—

"The Design and Manufacture of Pneumatic Tyres,"
by COLIN MACBETH.

There were present 120 Members and visitors.

* See p. 379, *ante*.

THE DISCUSSION.

Mr. F. A. S. Acres, in opening the discussion, said: I have a strong impression that if a tyre is somewhat on the easy side, the rib tread will be more destructive to the casing than a tread of the ordinary round type, such as the grooved tread, owing to the flexing of the sides. One form of tyre made by a well-known French firm was moulded flat so that the tread was compressed when the tyre was blown out. This was supposed to give greater resistance to cutting, and I should like to know what the author has to say about that. The author quoted a rim of the type of the Warland as being easy of detachment. I agree with him that it saves a great deal of muscular effort, but I once timed several men fitting a new tyre on to a plain rim and on to one of these detachable rims, and the solid rim was actually quicker, because the loose rim is so difficult to hold steady. With regard to the effect of speed on tyres, I have seen a tyre mounted on the armature of an electric motor running at a peripheral speed equal to 130 miles an hour, and the tread stripped completely from the cover, and at Brooklands I have seen a cover with the tread absolutely disintegrated. It did not come off in strips but in little more than dust, and it was a first-class tyre. I shall be interested to hear what the author has to say on that. He also touched on the effect of under-inflation. Many drivers do under-inflate their tyres for the sake of comfort, but I have found that under-inflation causes the plies of the fabric to separate at each side of the tread, where the tread proper narrows down into the sides.

Mr. R. Selz: When I used to drive my own cars, I always took good care to select a brand of tyre which showed a cut, because with a compressed tread it is impossible to know what is going on inside the cover. If a tread which is in compression as some of the Continental tyres are gets cut, the compression sets up a suction which will take in the wet and the canvas will start rotting before the damage is noticed. A tyre which will show a gash can be cemented up or re-vulcanised, and will then give a very good mileage. I have done some competition work and racing at Brooklands, but I have never had a tyre fail. The mounting of tyres is a matter of skill more than anything else. I have

(Mr. R. Selz.)
heard people talk about detachable rims, but personally I prefer to fit a tyre in the ordinary way, with beaded edge and keep the water out. There is more damage done to tyres by water than by rough usage, and I think the manufacturers of all tyres are to be congratulated on the fact that they are able to turn out tyres which do not require five or six holding-down bolts. With regard to the shape of the tread, there are certainly points about the rubber studded tyre which make it, perhaps, the best at the present time on the score of resiliency.

Major B. W. Shilson: I think that the progress that has been made in tyre design is at least as good as that which has been made in car design. The demands which a car makes on a tyre are very great indeed, as can be gathered when travelling behind a car and watching the way in which the back-axle is bumping about on the road. Therefore, I think it is all the more gratifying to get the tyre manufacturer with us and hear his views and his difficulties. I would also like to thank the author for the very interesting diagrammatic way in which he shows what we might call his operation sheet, and I think that if applied to other branches of the industry it would enable a saving to be effected, at all events in handling.

Mr. P. J. Joyce: I should like to hear whether the Doughty process, which was used by the Dunlop Company some years ago, is used now.

Mr F. G. Woollard: I should like to ask whether any method has been devised for preventing punctures in the very large tyres that are now being used, more in the United States than here, on heavy vehicles, because it seems to me that a puncture on one of these giant tyres, which would probably let the vehicle down by anything from five to seven inches, might be rather disastrous, especially if the camber of the road should happen to be the wrong way and the vehicle were going at anything like speed. I should also be glad if the author would add a few words on the subject of unsprung weight.

Another point I should like to ask him about is whether we are tending to soft large tyres—or are we going to keep to the small relatively hard tyres? I mention this because I saw it suggested in a paper before the Society of Automotive Engineers, apparently in all seriousness, that we should do away with unsprung weight as we know it at present and spring the whole of the car on large

soft tyres. I should like to know what the author's opinion on a process of that sort would be.

Mr D. V. Ryan: My experience of re-treaded tyres has been that they invariably meet an untimely end through a blow-out, long before ordinary wear and tear has made any impression on the tread. These blow-outs appear to be caused by the wall parting from the bead during the excessive internal pressure which obtains when the tyre strikes a large obstruction on the road. The point I would like cleared up is whether this fault is caused by the procedure adopted in re-treading a tyre, or whether the author is of the opinion that the strength of the walls did not warrant the operation. I might add that these repairs were carried out by a very reputable firm. I have noticed this occurrence on several of the leading makes of American tyres.

Mr. J. Duffy: I should like to ask the author his opinion of the Palmer process of building the casing. Is it the fact that they build the casing with a single strand of cord to provide a very flexible casing that will stand tyres being blown up comparatively soft? I believe that the Palmer Company claim that as one of the advantages of their tyres.

The President: I do feel, as the author has pointed out, that the closest co-operation is required between the tyre maker and the chassis maker, a good deal closer co-operation than has taken place in times past. We are, perhaps, too apt to look upon the tyre as the last thing we put on the car, and the last thing that need be thought about with regard to the car. I believe that is what the author is anxious to drive home to us, and I am certain that it could not have been done in a more effectual manner.

Mr. Macbeth, in replying on the discussion, said:—

Eliminating unsprung weight.—Mr. Woollard asks what the effect of doing away with springs would be. I think there is no doubt that such a practice would be a step backwards, as it would mean that the whole of the vehicle would represent unsprung weight. Given perfect roads there is no need for springs with good tyres, but we shall never be able to ensure all roads and tracks being such as will enable springs (or their equivalent) to be eliminated, as even a tyre of twice the ordinary sectional depth would not be able to allow for half the vertical displacement provided by ordinary springs.

Also, springs allow of very considerable canting of one axle in

(Mr. Macbeth.)

relation to the other without serious body-racking or undue momentary loading on tyres. A well sprung lorry allows of sufficient "roll" to the body and ensures that the wheels can maintain contact with very irregular road surfaces, and allows for the swaying action taking place due to centrifugal forces when cornering.

Large soft tyres.—There is an undoubted tendency towards the use of a large soft tyre.

Reduction of unsprung weight.—I think there are many directions in which this end should be sought, thus:—

(a) Try and remove all brakes from the back-axle, confining them to sprung portions, as at the gear-box. The great saving in unsprung weight would allow of greatly increased strength of transmission details so that no failure should occur such as to make braking ineffective.

(b) Removing brake gear from the back-axle will facilitate the better connection of the springs to the back-axle in such a way that the point of load application on the axle more nearly coincides with the wheel position, thus reducing the bending moments on the axle and allowing of a lighter axle with safety.

(c) The successful application of the cantilever type of springs to lorries would greatly improve springing and reduce unsprung weight.

(d) The combination of rear wheels having brake-drums and internal annular gears arranged in one piece with the rims should all allow of reduced back-axle weight.

One-piece B.E. rims versus *split or loose flange.*—I agree with Mr. Acres that, provided that beaded edge tyres have a reasonable stretch when fitted to the rim, the one-piece rim is preferable to the loose flange or collapsible type.

Re-treading policy.—The perfect tyre is one which will gradually wear out without failure of any one portion prematurely. Thus, if under normal conditions such a result was obtained it is questionable if it is advisable to make a casing to outlast its tread—rather concentrate on increased tread life. If, however, a well-balanced tyre has to stand abnormal punishment on stony roads and hard wear in hilly countries, it may be found that the tread surface is worn out while the casing is still good for many thousands of miles: such a tyre should be capable of being easily re-treaded if it has been properly cared for.

Causes of defective re-treading.—Mr. Ryan asks if re-treading

failures are not sometimes attributable to lack of original strength in the tyre in question: I would say that quite a number of makes of tyres are not suitable for re-treading generally because a poor grade of cotton has been employed or unsuitable mixings or cures have been made use of.

Treads.—Treads run at high speeds on the track have a very high factor of safety against failure due to the centrifugal forces set up tending to separate the tread from the case. The instance mentioned was either due to initial defects or to the particular tyre used being unable to withstand the great heat set up in the tread at abnormally high prolonged speeds.

The tyre which broke up (as regards its tread) into little pieces was probably very considerably overcured in manufacture.

Compressed Treads.—The particular type referred to by Mr. Acres and Mr. Selz as having a compressed tread was, I believe, designed in order that a cheap tread mixing could be used without a serious tendency to open gashes when cut.

I understand that the tyre referred to is no longer made with this feature, and am of the opinion that the compressed tread feature was obtained at the expense of undue strain and distortion in the casing itself.

Effect of tread design on casing life.—Regarding Mr. Acres' remarks as to the destructive effect on the casing of a ribbed as compared with a grooved tread, I am of the opinion that the ribbed pattern, due to its greater flexibility at the tread portion, is distinctly less destructive to the sides of the casing than a cross-grooved pattern.

A good non-skid pattern flexes more on stony roads than a plain pattern, which asks for more of the flexing to be done by the sides of the tyre.

The Doughty curing system, as mentioned by Mr. Joyce, was first used by the Dunlop Company for cycle tyres and motor-cycle tyres and is so used to-day. This process refers to the moulding and curing of tyres by machine and saves a very considerable amount of time and labour, where it can be applied, over more usual methods. The type of press shown on Fig. 7 for making B.E. small tyres works on the same principle, though it does not make the tyres to final shape.

Cord tyres, as referred to by Mr. Duffy, are, I believe, made by building up on a machine, laying strand after strand until two complete plies form the casing. The particular make of tyre re-

(Mr. Macbeth.)

ferred to is very efficient as regards power losses, as is exemplified by its use for track-racing purposes. I do not think that the makers claim any special suitability for their tyres for running with lower inflation pressures than usual.

The modern cord tyre is a development of the fabric cycle tyre —being assembled from multiple plies of warp threads (which have only an occasional light weft thread inserted to facilitate handling), which, as compared with woven canvas, can be made up into a tyre which is more efficient and resilient and less subject to disintegration in use. I have not dealt at any length with the cord tyre or the S.S. method of bead construction as I thought it advisable to deal with the type of tyre and attachment which is well known to all motorists in the British Isles.

In conclusion, I would like again to emphasise the desirability of combining the "technical forces" of the chassis maker and tyre manufacturer as regards all matters appertaining to the unsprung parts of automobiles.

Research work taking wheels and tyres fully into account would, I believe, lead to reduction in costs and improved road results—for instance, the following points would benefit and be standardised to a considerable degree:—

1. Rim sizes reduced.
2. Rim attachments standardised, enabling valve connections and tyre attachment tools to be simplified.
3. Hubs standardised.
4. Brake-drums and drum attachments standardised.
5. Mud-guard clearances standardised, enabling tyres and springs to be more definitely standardised.
6. Springs and springing improved.
7. Spring attachments improved.
8. Axles lightened and strengthened.
9. Axle attachments standardised.
10. Transmissions cheapened, due to being able to cater for attachment to uniform attachment points on the axles.
11. Jacking arrangements standardised, thus making standard jacks possible and cheapening them.
12. Incorporation of jacks with the chassis, due to ability to supply a standard accessory.
13. Improved tyres and wheels more easily commercialised, due to ability to cater for more defined requirements.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

EXTRAORDINARY GENERAL MEETING.

An Extraordinary General Meeting of the Institution was held on Wednesday, March 2nd, 1921, in order to further consider and, if deemed expedient, to confirm the Special Resolution* embodying the alterations in the Articles of Association necessitated by the conditions attached to the grant from the S.M.M.T., and which were passed, with an amendment, at the Extraordinary General Meeting held on February 9th.

The President moved that the Resolution, with the amendment, as read be confirmed. Mr. Clarkson seconded the motion, and on being put to the Meeting the Resolution, with amendment, was carried *nem. con.*

* See p. 375.

ANNUAL GENERAL MEETING OF THE INSTITUTION

HELD ON

WEDNESDAY, MARCH 2ND, 1921,

AT

THE INSTITUTION OF MECHANICAL ENGINEERS,

STOREY'S GATE, LONDON, S.W. 1.

The Minutes of the previous Annual General Meeting were read, confirmed and signed.

The Secretary then read the notice convening the Annual General Meeting.

On the proposition of Mr. Marshall Arter, seconded by Lt.-Col. E. A. Rose, the Annual Report and Accounts were adopted.

It was then announced that Mr. Geo. Watson had been elected President for the Session 1921—22, and that Lt.-Col. D. J. Smith, Dr. W. R. Ormandy, Major B. W. Shilson and Mr. G. Douglas Leechman had been elected Vice-Presidents.

It was announced that the ballot for Council had resulted in the following being elected to fill the vacancies:—

Brig.-Gen. R. K. Bagnall-Wild.
Mr. A. E. Berriman.
Mr. Harry Brearley.
Mr. H. G. Burford.
Col. R. E. Crompton.
Mr. J. H. S. Dickenson.
Mr. L. H. Hounsfield.
Mr. G. H. Lanchester.
Mr. L. A. Legros.
Mr. Max Lawrence.
Lt.-Col. J. S. Napier.
Mr. A. J. Rowledge.
Mr. G. J. Shave.
Mr. H. Kerr Thomas.
Major Chas. Wheeler.

On the proposition of Lt.-Col. T. M. Hutchinson, seconded by Mr. H. D. Walthall, Mr. J. Stewart Mallam was re-elected Auditor to the Institution for the year 1921 at an increased fee of twenty guineas.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

MARCH, 1921.

The Ninth Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1, on Wednesday, March 2nd, 1921, at 8 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following list of Members who had completed their election since the previous Meeting was posted up:—

MEMBERS.

WALTER BONALLACK (London).
HENRY THOMAS FOWLER (Basingstoke).
GEORGE HEY (London).
WILLIAM YOUNG (Huddersfield).

ASSOCIATE MEMBERS.

FREDERICK CHARLES ASHBY (London).
WILLIAM THOMAS CHATFIELD (S. Africa).
VIVIAN INGRAM RACKLEY CLARK (Shields).
ALBERT HENRY GODFREY GIRLING (London).
CEDRIC MELDRUM (Sheffield).
ORVILLE RUPERT SPENCER (Birmingham).
CYRIL HEATH STEPHENSON (London).
HERBERT OLIVER WOOD (Bradford).

Associates.

Arthur Bayliss (Sheffield).
Arthur Gordon Roy Williams (Twickenham).

Graduates.

John Hatton Berry (London).
Harry Boden (London).
Robert Bolton (Derby).
Harry Braimbridge (New Brighton).
Donald Lombard Gammon (Guildford).
George William Shepherd (Sheffield).

Probationary Graduates.

George Herbert Graham (Romford).
Albert William Hallpike (Walthamstow).
Edward Alan Thomas (Birmingham).

Transfers.

Oliver Dawson North (London).

The following paper was then read and discussed:—

"Automobile Steels," by Dr. W. H. Hatfield.

There were present 125 Members and visitors.

FURTHER NOTES ON AUTOMOBILE STEELS.

By Dr. W. H. HATFIELD.
(Member.)

There were a number of interesting points which arose in the discussion upon the author's paper on "The most suitable Steels for Automobile Parts,"* but perhaps the most important was the question of the true elastic range of the steels dealt with. For purposes of design it is essential that the Wöhler range or its equivalent should be accurately determined for the various steels in their different conditions.

In replying to the discussion, the author intimated that they had been conducting at the Brown-Firth Research Laboratories for some considerable time an investigation designed to establish the actual fatigue range of the different steels, and that he hoped to be in a position to place the results before the Institution this Session. An investigation of this kind takes a long time for completion, and this will be realised when the experimental side is considered.

The machine employed is the Wöhler rotary bend test, and full particulars, together with a diagram of the arrangement of the machine, will be found in Appendix I. The test piece rotates at 1500 revs. per minute, and the procedure during the experiments consisted in gradually modifying the load on the bar until that load was attained which the bar would carry without rupture for ten million revolutions. It will thus be seen that the test piece which revolved ten million revolutions has been going day and night for practically five days. Some test pieces were run for a much longer period, and the fact that one such test piece broke after thirteen million revolutions, whilst being an excellent test, by no means indicates the capacity of the material for running to infinity.

In view of such facts it is clear that the time required for com-

* See Proc. I.A.E., Vol. XIV., p. 503.

pletion of the investigation would have to be considerably extended before dogmatic values could be stated. It is certainly clear that the determination of the truly elastic range of steel is a lengthy and accurate process. The present research has been in hand for two or three years, and the data already derived, it is hoped, will be sufficient to produce a valuable discussion.

On reference to Appendix I., it will be seen that the method of calculating the stress on the section where the bar breaks is based on the assumption that the material may be taken as a perfectly elastic body. It is at once obvious that if the stressing is such that the bar breaks, it has not behaved as an elastic body, and hence the figures given for the stress carried by the bar are only nominal. This, however, is discussed in the Appendix. As indicative of the results obtained, a series of experiments with wrought iron might be described. This wrought iron had the properties shown in Table I.:—

TABLE I.

Analysis.

	per cent
Carbon	0·06
Manganese	0·12
Silicon	0·13
Sulphur	0·023
Phosphorus	0·25

Mechanical Test results.

Tensile.	Elastic Limit, tons per sq. in.	11·7
	Yield Point, ,, ,,	14·1
	Maximum Stress, ,, ,,	23·0
	Elongation, per cent	29·0
	Reduction of Area, per cent	48·4
	Izod Impact, ft.-lb.	44·0
	Stanton Blows	300
	Sankey, Energy, ft.-lb.	977
	Arnold Reversals	106
Torsion.	Elastic Limit, tons per sq. in.	9·3
	Probable Maximum Shear Stress	17·8
	Degrees Twist	773
	Brinell Hardness Number	114

Load.	Revolutions.
± 28·0 tons	270
± 25·5 ,, ...	1,400
± 19·5 ,, ...	90,300
± 15·0 ,, ...	644,800
± 14·0 ,, ...	1,722,800
± 12·0 ,, ...	8,300,000

Following closely on Wöhler's own work, we first loaded the wrought iron to a stress of 15 tons per sq. in., and rupture occurred after 644,800 revolutions. We reduced the load to 14 tons and subsequently to 12 tons, and at the latter figure we had apparently reached a high value for resistance. We commenced our experiments with wrought iron owing to its high capacity for plastic deformation, and thought it would be of considerable interest to see how it behaved, hence the fact that, after approaching what appeared to be the elastic range of the material, we tried some much heavier loadings. The maximum stress of the material was proved to be 22·34 tons per sq. in. in tension, and loadings of 19·5 tons, 25·5 tons, and 28 tons per sq. in. nominal stress were applied. In Table I. will be found the values obtained, and it appeared to us remarkable that with really heavy loadings the wrought iron resisted such rapid repeated reversals of stress for so long.

If, for practical purposes, 10,000,000 revolutions is considered as approximating to the fatigue range of the material, it will be seen that this wrought iron might be predicted to be capable of withstanding ± 11·7 tons per sq. in., but if the data derived is plotted, it may be determined that this stress of 11·7 would have to be lowered to between 7 and 8 tons for a successful life to infinity. As, however, even in the automobile industry, parts are called upon to withstand hundreds of millions of revolutions, a value selected somewhere between 11·7 and 7/8 tons may be taken as indicative of the possibilities of the material for practical purposes. For motor car parts, a good life would be represented by 200,000,000 revolutions. It will, therefore, be seen from these observations that it is extremely difficult to say arbitrarily, even with the aid of substantial experiments, exactly what the safe range of stress is. Nevertheless, if comparisons of different materials are made of their behaviour over 10,000,000 revolutions, it may be assumed, at any rate until experimental evidence to the

contrary is available, that the behaviour for this period of time will be indicative of the comparative response over longer periods.

If Mr. Stromeyer's method of plotting the data derived from these tests* is employed, an experimental insight into the real values for fatigue ranges may be obtained. Whilst discussing this matter, however, there is one point which should not be lost sight of by engineers, namely, that here in this test we have a steady consistent load, and allowance must be made when thinking of service conditions for occasional shocks, excessive loading, etc., and their effect upon the possibility of substantially increasing the value of subsequent occasional stresses.

Before going on to the automobile steels, it might be useful to describe the results obtained from brass bars in the "as rolled" and in the annealed condition. This was the same brass concerning which the author has already published experimental data.† The analysis and mechanical properties are given in Table II.:—

TABLE II.

Brass.—Cold-rolled from $1\frac{1}{4}$ in. to $1\frac{1}{8}$ in.

Analysis.—Copper, 65 per cent; Zinc, 34·6 per cent.

		As rolled.	Annealed at 600° for two hours.
Tensile.	Elastic Limit, tons per sq. in.	15·0	4·5
	Yield Point, ,, ,,	21·3	5·8
	Maximum Stress, ,, ,,	24·5	20·0
	Elongation, per cent	34·0	72·0
	Reduction of Area, per cent	58·6	70·0
Torsion.	Elastic Limit, tons per sq. in.	13·9	4·1
	Maximum Stress, ,, ,,	19·1	18·7
	Degrees Twist	1163	1344
	Izod Impact, ft.-lb.	33, 34, 32	37, 40, 38
	Charpy ,,	19	$27\frac{1}{2}$
	Stanton Blows	1204	358
	Arnold Reversals	220, 240	426, 400
	Sankey, Energy, ft.-lb.	3067	3380
	Brinell Hardness Number	120	69

* Manchester Steam Users' Association.

† Institute of Metals.

TABLE II.—*continued.*

LOAD.	COLD ROLLED. Revs.	ANNEALED. Revs.
± 19·5	136,700	—
± 15·0	1,079,700	—
± 10·6	11,600,000	1,025,900
± 8·25	—	3,580,000
± 6·0	—	11,000,000

These values are remarkable. It will be seen that the cold-rolled brass which was work-hardened behaved very well. It has been held that if through cold-work the maximum stress in tension is increased, the resistance to compression is substantially, if not proportionately, decreased. These data suggest that this is not the case.

One more interesting material which was examined was Duralumin, the analysis and mechanical properties of which are stated in Table III.:—

TABLE III.

Duralumin.

Analysis.

	per cent
Carbon	trace
Maganese.....	0·66
Silicon	0·19
Aluminium	94·90
Copper	4·18

Modulus of Elasticity.—4600 tons per sq. in.

Mechanical Tests.

Tensile.	Elastic Limit, tons per sq. in.	14·1
	Yield Point, ,, ,, ...	21·4
	Maximum Stress, ,, ,, ...	29·9
	Elongation, per cent	16·0
	Reduction of Area, per cent	21·0
Torsion.	Shear Stress at Elastic Limit, tons per sq. in.	8·6
	Apparent Maximum Shear Stress, tons per sq. in.	15·75
	Degrees Twist.....	2270

Izod Impact, ft.-lb.	18, 18, 18
Stanton Blows	230
Arnold Reversals	22, 28
Brinell Hardness Number	118
Sankey, Alternations	2
,, Maximum Bending Moment	50
,, Energy to fracture, ft.-lb.	160

Compression.

Yield Point, tons per sq. in.	20·3
For 2 per cent Compression	Load = 25·0 tons per sq. in.
For 5 ,, ,,	,, = 29·5 ,, ,,
For 10 ,, ,,	,, = 35·2 ,, ,,

Maximum = 23 per cent Compression, Load 40·2 tons per sq. in. Test piece sheared.

Load.	Revolutions.
13·5	236,800
10·5	1,134,600
9·0	10,000,000*

The results are not complete, but it is quite clear that the fatigue range of this Duralumin is entirely disproportionate to the figures obtained from the tensile test, when a comparison is made with the wrought iron figures previously given.

Automobile Steels.

Turning now to automobile steels, the same steels in as nearly as possible the same conditions as described in the author's paper before the Institution in April, 1920,† were used for the experiments, with one or two additional carbon steels.

The results obtained will be found in Tables IV., V. and VI.:—

* Unbroken.

† *Loc. cit.*

TABLE IV.—*Carbon Steels.*

Carbon, per cent	0·31	0·45	0·52	0·60	0·81	0·90
Manganese, ,,	0·67	0·70	0·60	0·69	0·87	0·55
Silicon, ,,	0·14	0·15	0·19	0·20	0·22	0·11
Sulphur, ,,	0·048	0·044	0·035	0·04	0·034	0·036
Phosphorus, ,,	0·04	0·036	0·025	0·039	0·028	0·031
Elastic Limit, tons per sq. in.	20·05	21·5	19·68	27·0	20·86	23·16
Yield Point, tons per sq. in.	20·79	22·9	24·80	29·16	31·3	32·4
Maximum Stress, tons per sq. in.	35·17	41·2	41·48	48·6	60·72	59·4
Elongation, per cent	30·5	25·0	25·0	22·0	13·0	11·0
Reduction of Area, per cent	54·6	47·0	39·17	37·9	22·97	15·5
Normalised :—	850° C.	820° C.	875° C.	816° C.	825° C.	850° C.
Wöhler Reversals. ± 32·0 tons	—	—	—	—	28,500	—
± 28·0 ,,	—	—	—	—	77,900	78,700
± 26·0 ,,	—	—	24,900	—	—	—
± 25·5 ,,	6,650	—	—	—	—	—
± 25·0 ,,	—	—	—	112,900	—	205,700
± 24·0 ,,	—	—	—	—	712,500	—
± 23·0 ,,	—	—	—	497,900	—	—
± 22·0 ,,	—	—	—	—	1,833,500	538,500
± 21·6 ,,	100,300	18,600	—	—	—	—
± 20·0 ,,	—	—	216,100	—	—	1,309,000
± 19·5 ,,	—	—	—	—	—	—
± 19·0 ,,	—	840,000	—	—	—	—
± 18·0 ,,	—	—	750,000 / 620,300	—	12,449,400*	—
± 17·0 ,,	520,900	1,782,000	—	—	—	—
± 16·0 ,,	—	—	16,460,000*	—	—	—
± 15·3 ,,	1,720,000	—	—	—	—	—
± 15·0 ,,	—	10,000,000*	—	—	—	—
± 13·5 ,,	3,247,000	—	—	—	—	—
± 12·5 ,,	—	—	—	—	—	—
± 11·7 ,,	10,000,000*	—	—	—	—	—
Safe range for 10,000,000 revolutions	12·8	15·5	16·5	18·0	19·0	16·5

* Unbroken.

Table V.

Case-hardening Steels.

	Carbon Steel.	5 per cent Nickel Steel.	
Carbon, per cent	0·11	0·14	
Manganese, ,,	0·73	0·29	
Silicon, ,,	0·10	0·16	
Sulphur, ,,	0·018	0·018	
Phosphorus, ,,	0·02	0·009	
Nickel, ,,	0·12	4·84	
Treatment.	Normalised 900° C.	Normalised 900° C. Water Quenched 760° C.	Normalised 900° C. Water Quenched 760° C.
Elastic Limit, tons per sq. in.	14·2	12·0	—
Yield Point, ,, ,,	16·6	17·0	41·9
Maximum Stress, ,, ,,	26·3	30·1	60·58
Elongation, per cent on 2 in.	37·0	33·0	17·5
Reduction of Area, per cent on 2 in.	61·8	63·3	46·9
Wöhler Reversals. ± 30 tons	—	—	956,000
± 27 ,,	—	—	—
± 25 ,,	—	—	10,000,000*
± 19 ,,	—	1,770,000	—
± 17 ,,	—	12,000,000*	—
± 15 ,,	496,600	10,000,000*	—
± 13 ,,	2,437,500	12,000,000*	—
± 11·5 ,,	10,000,000*	—	—
Safe range for 10,000,000 revolutions.	11·7	17·5	26·5

* Unbroken.

Table VI.

Alloy Steels.

	Air Hardening Nickel Chromium Steel.	3 per cent Nickel Chromium Steel.	Chromium Vanadium Steel.	Nickel Steel.
Carbon, per cent	0·30	0·32	0.46	0·39
Manganese, ,, ...	0·40	0·60	0·64	0·55
Silicon, ,, ...	0·105	0·12	0·19	0·11
Sulphur, ,, ...	0·627	0·03	0·28	0·039
Phosphorus, ,, ...	0·033	0·03	0·31	0·031
Chromium, ,, ...	1·38	0·76	1·48	nil.
Nickel, ,, ...	4·07	3·41	0·19	2·96
Vanadium, ,, ...	nil.	nil.	0·33	nil.
Treatment.	Air Hardened 820° C.	Oil Quenched 850° C. Tempered 600° C.	Oil Quenched 850° C. Air Cooled 650° C.	Oil Quenched 850° C. Tempered 600° C.
Elastic Limit, tons per sq. in.	—	47·0	38·6	30·8
Yield Point, tons per sq. in.	90·5	56·0	48·6	41·7
Maximum Stress, tons per sq. in.	109·6	62·0	56·5	49·51
Elongation, per cent on 2 in.	9·0	21·0	22·0	24·0
Reduction of Area, per cent	21·0	62·0	60·4	63·6
Wöhler Reversals. ± 40 tons	215,900	—	—	—
± 37·5 ,, ...	—	—	73,000	—
± 35 ,, ...	759,700	—	—	—
± 34 ,, ...	—	176,800	—	—
± 32 ,, ...	—	—	507,600	—
± 30 ,, ...	10,000,000*	3,169,000	—	—
± 29 ,, ...	—	—	3,628,000	—
± 27 ,, ...	—	10,000,000*	—	—
± 25 ,, ...	—	—	—	232,000
± 24 ,, ...	—	—	10,000,000*	1,247,000
± 22 ,, ...	—	—	—	—
± 21 ,, ...	—	—	—	10,000,000*
Safe range for 10,000,000 revolutions.	32·0 ?	28·0	27·5	21·5

* Unbroken.

A careful study of these figures, incomplete though they are, will be found to place the steels in a certain order of merit, and to enable some opinion to be formed of the relationship of the Wöhler results to values obtained from the tensile tests. In Table VII. will be found the safe range for 10,000,000 revolutions in percentage of the limit of proportionality (primitive), yield and maximum stress in tension:—

TABLE VII.

	Percentage of Limit of Proportionality.	Percentage of Yield.	Percentage of Maximum Stress.
Wrought Iron	100·0	83·0	50·8
0·31 Carbon Steel	63 8	61·8	36·4
0·45 ,, ,,	72·1	67·7	37·6
0·52 ,, ,,	83 8	66·5	39·8
0·60 ,, ,,	66·7	61·7	37·0
0·81 ,, ,,	91·1	60·7	31·3
0·90 ,, ,,	71·3	50·9	27·8
Carbon Case-hardening Steel (Normalised)	82·4	70·5	44·5
,, ,, ,, (Hardened)	145·8	102·9	58·1
5 per cent Nickel Case-hardening Steel (Hardened)	—	63·2	43·75
3 per cent Nickel Chromium Steel	59·6	50·0	45·2
Chromium Vanadium Steel	71·25	56·6	48·7
3 per cent Nickel Steel	69·8	51·6	43·4
Air Hardening Nickel Chromium Steel	—	35·4 ?	29·2

Commenting on these percentages, the author considers that the carbon case-hardening steel results in the hardened condition are misleading, since the outer layers of the Wöhler bar will be harder than the interior layers; hence the Wöhler values would be expected to be somewhat disproportionately higher than the tensile values (the high Wöhler value in the cold-worked brass may similarly be explained).

The air hardening nickel chromium steel which gives such low values, is not misleading, the Brinell value of the Wöhler piece being slightly higher than that of the tensile piece.

The other results may be taken as reliable within ordinary experimental error, and the author does not consider that he is

diminishing the value of this experimental evidence when he draws attention to the difficulties in the way of obtaining in a somewhat heterogeneous material, that absolutely uniform condition necessary for accurate quantitative deductions. What deductions may be drawn from these figures? In the first place, it is felt that the relationship to the primitive limit of proportionality is erratic, and he is inclined to place little importance on the latter values. When we come to the relationship to the yield and to the maximum stress, we seem to be on firmer ground, although in considering even such relationships we must remember the arbitrary manner in which the yield is taken, and the real value of the figure stated as the maximum stress. After such reservations, however, useful deductions can be drawn.

It would appear that for a wide range of carbon content in the carbon steels (0·31 to 0·81) the percentage of the yield is 60·7 to 67·7, and the percentage of the maximum stress 31·3 to 39·8. With wrought iron the ratio is higher, whilst with carbon over 0·81 per cent the ratio is abnormally low.

With the high-tensile steels (nickel, 3 per cent nickel chromium and chromium vanadium) the percentage of the yield is 50 to 56·6 and that of the maximum stress 43·75 to 48·7. The air hardening nickel chromium steel is the only material tested in the truly hard condition, and the low values of 35·4 and 29·2 respectively provide food for thought and subject for further experiment.

Conclusions.

Having some twelve months ago promised to deal further with automobile steels this Session, the author makes no apology for presenting these data in their present form, feeling as he does that engineers generally will be equally interested with himself in the progress of the investigation.

The outstanding fact which is derived so far is that the high-tensile alloy steels (maximum stress in tons per sq. in. 50/65) have a materially higher fatigue range than carbon steels of the same hardness. This observation concerns the normalised condition of the carbon steels; the hardened and tempered condition of such steels has not been dealt with for the reason previously stated, that is, the unsuitability of carbon steels for the hardening and tempering operations except in the thinnest sections. For 10,000,000 revolutions the safe range of stress for carbon steels, as used in the motor industry, is from 12 to 18 tons per sq. in.

For alloy steels the comparative figures are 21·5 to 28 tons. Here is indeed a sound argument for the extended use of high-tensile alloy steels for highly stressed parts. Some idea of the true ultimate Wöhler range of the steels may be gleaned from the plotting of the data in Fig. 4.

The author hopes that the experimental work here described will, if possible, still further increase the interest which the engineer is taking in the elastic ranges of the materials. The question is an extremely complicated one, and one which calls for the earnest work of many investigators if the matter is to be put on a satisfactory quantitative basis. Much excellent work has been done in the past by investigators at home and abroad, and a brief summary of this will be found in Appendix II.

Before closing, the author would like to thank Messrs. Thos. Firth & Sons, Ltd., and Messrs. John Brown & Co., Ltd., for their consent to the publication of the results of the investigation, which was carried out in the Brown-Firth Research Laboratories. He is indebted to Mr. F. C. Fairholme who particularly desired concentration on this problem. His thanks are also due to Mr. G. R. Bolsover, Assoc.Met., and Mr. G. Stanfield, M.Eng., B.Sc., and also to Mr. E. Eadon for assistance rendered in the investigation. He is particularly indebted to Mr. G. Stanfield for the preparation of the Appendices.

APPENDIX I.

CALCULATION OF STRESS IMPOSED IN WÖHLER TESTS.

The stresses due to bending action are tension in the upper portion of the "critical cross section," and compression in the lower portion. The stresses are greatest at the highest and lowest points respectively of these areas.

If the load applied is not such as to exceed the elastic limit of the material,

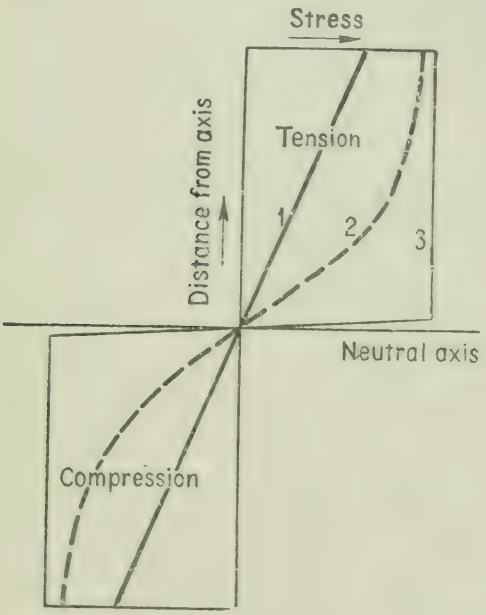


FIG. 1.—Distribution of stress over section—

(1) Below the elastic limit.
(2) Above the elastic limit.
(3) An extreme case.

then since stress is proportional to strain, according to the ordinary laws of elasticity the stress existing at any particular point in the section is proportional to the distance of that point from the neutral axis. Hence the stress increases uniformly from zero to a maximum as we pass from the neutral axis outwards. Under these conditions the integrated value of the moments of resistance to

bending offered by the successive small elements of area in the cross section, when taken over the whole section, is

$\frac{\pi}{32} d^3 f$ Where d — diameter at that section,
and f — stress (tensile or compressive at the point where it has maximum value).

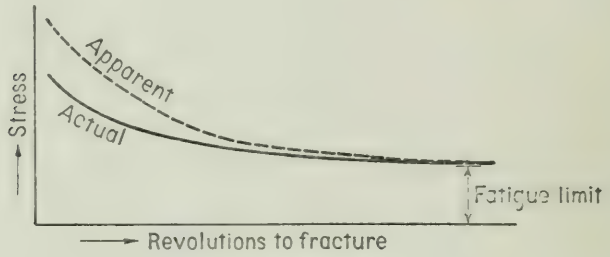


FIG. 2.

This total resisting moment just balances the bending moment acting on the section due to the external loads.

Hence $\frac{\pi}{32} d^3 f = W \times 1$ (*i. e.*, load × leverage)..(1).

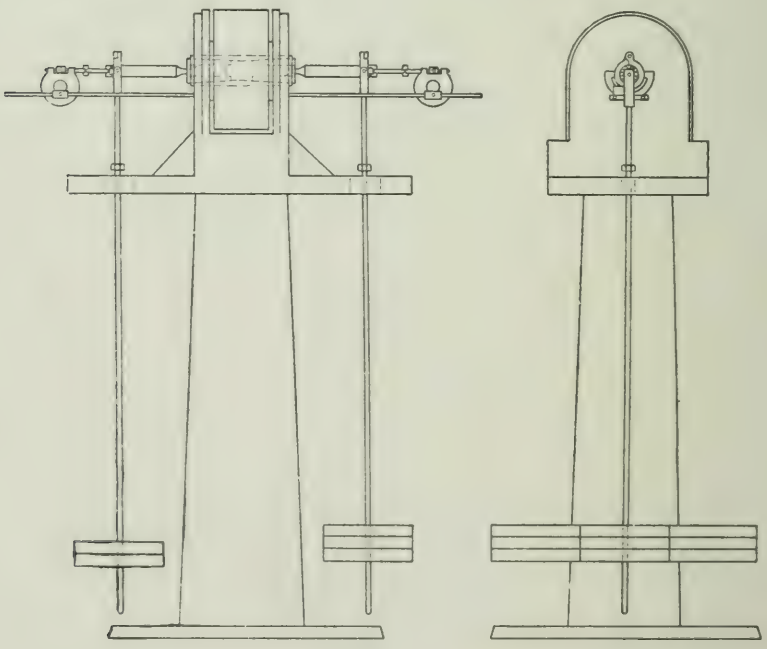


FIG. 3.

If the load W is increased sufficiently the maximum stress set up is in excess of the elastic limit of the material. The proportionality of stress to strain does not then hold, and the change of stress as we pass from the neutral axis of the section outward is not a proportional one, but more in accordance with the dotted curve in Fig. 2. The exact distribution of stress will vary considerably

according to how far the elastic limit is exceeded, and according to the shape of the stress-strain diagram for the particular material.

The formula given above for moment of resistance may be looked upon as

$$\left\{\begin{matrix}\text{area of}\\ \text{section}\end{matrix}\right\} \times \left\{\begin{matrix}\text{average}\\ \text{stress}\end{matrix}\right\} \times \left\{\begin{matrix}\text{effective average distance}\\ \text{of point of action of}\\ \text{stress from neutral}\\ \text{axis.}\end{matrix}\right.$$

or

$$\left\{\begin{matrix}\text{area of}\\ \text{section}\end{matrix}\right\} \times \left\{\left\{\begin{matrix}\text{maximum}\\ \text{stress}\end{matrix}\right\} \times \text{factor } x_1\right\} \times \left\{\begin{matrix}\text{maximum distance}\\ \text{from neutral axis}\\ \times x_2.\end{matrix}\right.$$

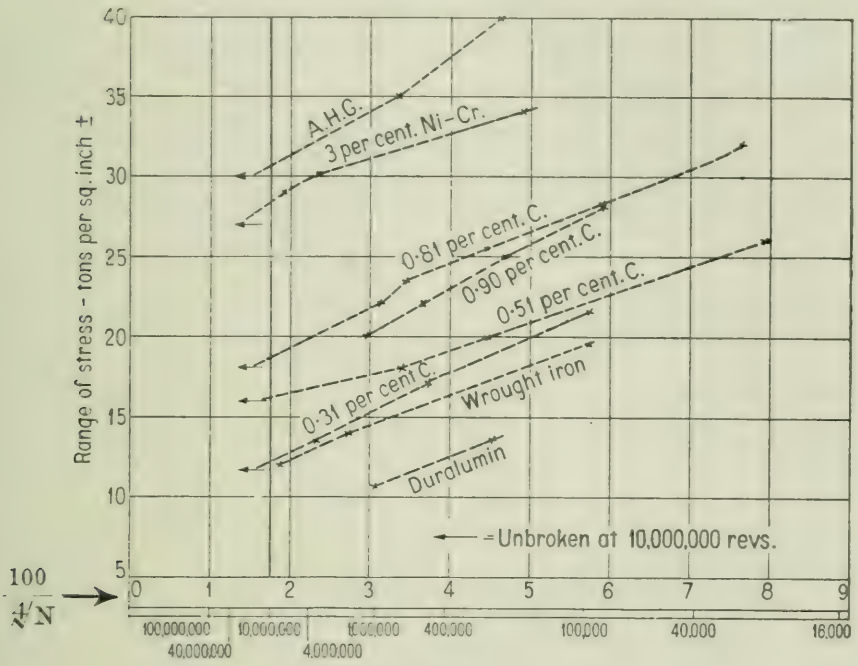


FIG. 4.

← = unbroken at 10,000,000 reversals.

Below the elastic limit $x_1 = \frac{4}{3\pi} \times x_2 = \frac{3\pi}{16}$.

$$\therefore \text{M.R.} = \left(\frac{\pi}{4}d^2\right) \times (f) \times \frac{4}{3\pi} \times \frac{(d)}{2} \times \frac{3\pi}{16} = \frac{\pi}{32}d^3f.$$

Above the elastic limit x_1 increases owing to the change in the shape of the diagram, and x_2 also may increase or decrease.

The product $x_1 \times x_2$ will in general increase.

In an extreme case in which the stress distribution is according to (3) in Fig. 1, then $x_1 = 1$ and $x_2 = \frac{1}{3}$.

$$\text{Hence W} \times 1 = \text{M.R.} - \frac{\pi}{4}d^2(f \times 1) \times \left(\frac{d}{2} \times \frac{1}{3}\right) = \frac{\pi}{24}d^3f.$$

$$\text{or } \frac{4}{3} \times \frac{\pi}{32}d^3f.$$

Thus in the extreme case, f may be only three-quarters of the value which would be given if calculated by the formula (1).

In determining the actual "fatigue limit" the above variation in formula does not affect the result arrived at as the fatigue limit since the transition from formula (1) to formula (2) is a gradual one, and as such merely modifies the position of the early part of the curve (see Fig. 2) plotted between stress and number of revolutions to fracture (*i.e.*, the portion of the curve corresponding to a small number of revolutions). It does not appreciably affect the other portion of the curve, since there the formula is substantially correct, and approaches nearer to absolute truth the nearer we approach the fatigue limit of the material.

A diagram of the machine employed will be found in Fig. 3.

APPENDIX II.

BIBLIOGRAPHY OF WORK DONE ON ALTERNATING STRESS TESTS.

A. Wöhler. Report 1871. See Unwin's "Testing of Materials of Construction."

Carried out repeated tension, repeated torsion, rotary bending tests at 60 to 80 revs. per minute, and gave safe limits of stress for indefinite life for some materials. He showed that for fluctuating load the range of stress applied is more important than the maximum stress.

Spangenberg. See Unwin's "Testing of Materials of Construction."

Made further experiments supporting Wöhler's results.

Sir B. Baker. See Unwin's "Testing of Materials of Construction."

Rotary bend tests.

Bauschinger. See Unwin's "Testing of Materials of Construction."

Repeated tension tests, &c. Measured original elastic limit, and that acquired during repetition of load—also the effect on the maximum stress, produced by repetition of stress. Distinguished between primitive and "natural" elastic limits, and deduced that the natural elastic range is the same as that which can be applied indefinitely without fracture.

Gerber. See Unwin's "Elements of Machine Design."

Deduced parabolic relation between "Safe range of stress" and lower limit of range to be applied.

Wegraugh. Formulae for safe range of stress. Proc. Inst. Civil Engineers, p. 63.

J. Gilchrist. Engineer, 1900, Vol. 90, p. 203.

Wöhler's Theories on Materials under repeated stress.

Ewing and Humfrey. "The Fracture of Metals under repeated alternations of Stress." Phil. Trans. Roy. Soc., Vol. 200 A., pp. 241—250 (1902).

A study of the mechanism of fatigue failure under the microscope.

O. Reynolds and J. H. Smith.	"Experiments with a throw Testing Machine, for reversals of Direct Stress."	Phil. Trans. Roy. Soc., 1902, p. 265.

Stress produced by inertia of reciprocating parts. Reduced safe ranges obtained with higher speeds. (1900—2400 revs. per minute.)

J. Morrow.	"Extensometers."	Inst. Mech. Engineers, 1904, p. 469.
Richards and Stead.	"Alternating Bending on Overheated Steels."	Iron and Steel Inst., Vol. 2, 1905, p. 85.
Gardner.	"Effects caused by Reversal of Stresses in Steel."	Iron and Steel Inst., 1905, Vol. 1, p. 481.
Stanton and Bairstow.	"Resistance of Iron and Steel to reversals of Direct Stress."	Inst. C. E., 1906, Vol. 166, p. 78.

With speed 800 revs. per minute obtained results in accordance with Wöhler. In these tests the tensile end of range was twice the value of the compressed stress. Also showed the weakening effect of sudden changes in section.

Freminville.	"Effects of Vibration."	Rev. de Metallurgie, 1906, Vol. 3, p. 61, and 1907, Vol. 6, p. 833.
Sulzer.		Zeit. des Ver. deut. Ing., 1907
F. Rogers.	"Microscopic Study of Strain in Metals."	Jour. R. Microscope Society, 1907.
Stanton.	"A New Fatigue Test for Iron and Steel."	Iron and Steel Inst., 1908, p. 54.

In this test the test piece is in the form of a ring rotating between three rollers. The test piece is at the same time subjected to alternating bending and to abrasion.

F. Rogers.	"Heat-Treatment and Fatigue of Steel."	Iron and Steel Inst., 1909, pp. 119 and 141.

Showed the effect of annealing on the resistance to alternating stress on some materials.

E. Nusbaumer.	"Notes on some Alternating Stress Tests."	Int. Assoc. Testing Matls., 1909.
Fremont.	"Effects of Vibration."	Bull. de la Soc. d'Encouragement, 1909, Vol. 1, p. 653.
L. Bairstow.	"Elastic Limit under Cyclical Variation of Stress."	Phil. Trans., 1909, Vol. 210, p. 35.

Investigated stress-strain diagrams during conditions of fatigue testing at 2 revs. per minute. For equal and opposite limits of stress there is a cyclical permanent set, which increases during a test if the range is too high. For limits of range of stress not equal and opposite, there is in addition an accumulation of "set" in one direction until the material has adjusted itself. The elastic limit of the material may be increased in this way, but to a limited extent, depending on the value of the lower limit.

Guillet.	"Effect of 'Damping' in the Testing of Metals."	Rev. de Metallurgie, 1909, Vol. 6, p. 885.
W. Giesen.	"Special Steels in Theory and Practice."	Iron and Steel Inst. (Carnegie Mem.), 1909, p. 54.
W. Mason.	"Mild Steel under Combined Stress."	Inst. Mech. Engineers, 1909, p. 1205.
C. A. M. Smith.	"Compound Stress Experiments."	Inst. Mech. Engineers, 1909, p. 1237, and
C. A. M. Smith.	"Researches on Elasticity."	Eng. Rev., 1911.
Howard, J. E.	"Resistance of Steels to Repeated Alternating Stresses."	Congress Inst. Assoc. for Testing Materials, 1909.
E. Rasch.	"Determination of Elastic Limit by a Thermo-electric Method."	Int. Ass. Testing Materials 1909.
E. M. Eden.	"Endurance of Metals under Alternating Stresses and Effect of Rate of Alternation."	Univ. of Durham Phil. Soc., 1909-10.

Between 300 and 1300 revs. per minute endurance is independent of speed.

Faroux.		Vie Automobile, July 23rd, 1910.
Henry.		Rev. de Metallurgie, 1910, Vol. 8, p. 1171.
Boudouard.	"Testing of Metals by the Damping of Vibrations."	Bul. de la Soc. d'Encouragement, 1910-11, p. 545, and Comptes Rendus, March 4th, 1910.
J. H. Smith.	"Experiments on Fatigue."	Iron and Steel Inst., 1910.
Blount, Kirkaldy and Sankey.	"Repetition of Stress."	Inst. Mech. Engineers, 1910, p. 715.

Made comparative tests in tension, impact tension and repeated bending.

C. E. Larard.	"Behaviour of Ductile Material under Torsional Strain and Restoration of Elasticity."	Inst. Civil Engineers, 1910, p. 332.
A. Martens.	"Change of State in Metals under Mechanical Strain."	Preuss. Akad. Wiss. Berlin Sitz. Ber. 11, p. 209, 1910.
Eden, Rose and Cunningham.	"The Endurance of Metals."	Inst. Mech Engineers, 1911, p. 839.

Investigated the effect of speed of alternation on endurance, and found no variation. Interruption of test showed no effect.

Zeigler.		Rev. de Metallurgie, 1911, Vol. 8, p. 655.

L. B. Turner. "Strength of Steels under repetition of Stress." Engineering, July, Aug., Sept. 1911.

Found endurance limit in alternating tension lower than primitive elastic limit.

E. L. Hancock. "Strength of Steel from Structural Shapes." Amer. Soc. Test. Materials, 1811.

E. L. Hancock. "The Elastic Properties of American Ingot Iron." Eng. Record, 1st July, 1911

J. O. Roos. "Endurance Tests." New York Congress, Inst. Assoc. Testing Mats., 1912, Vol. 2, p. 1, and Page's Weekly, 12th July, 1912.

Investigated the effect of speed of alternation and found very little. He investigated also the effect of size of fillet radius and found that a small fillet reduced the strength, also obtained comparative results between rotary bend tests and alternating impact test.

Kommers. "Repeated Stress Testing." Int. Assoc. Testing Materials, 1912.

Dudley. "Repetition of Stress." Iron and Steel Metallurgist, Vol. 7, 134, and 6th Congress Inter. Assoc. Testing Materials, New York, 1912.

Hopkinson and Williams. "Elastic Hysteresis of Steel." Proc. Roy. Soc., Vol. 87, p. 502, 1912.

Measured the heat dissipated due to mechanical hysteresis when specimens were tested under alternating stress within the elastic limit. Deduced that at 120 cycles per sec., the loss is about 30 per cent less than in slow tests. The rate of dissipation of energy reached a steady value.

B. Hopkinson. "A High-Speed Fatigue Tester." Proc. Roy. Soc., Vol. 86, p. 131, Jan. 1912.

Describes a machine employing an alternating electromagnet and the principle of resonance in which alternating stress tests are made at 6000—7000 per minute. At these speeds the endurance appears to be considerably greater than at speeds up to 2000 revs. per minute, although the limiting range may not be different.

B. P. Haigh. "Alternating Stress Testing Machine." Brit. Assoc. Rep., 1912.

B. A. Committee on Stress Distribution. "British Association Report on Alternating Stress." Brit. Assoc. Rep., 1913.

Lake. "Repetition of Stress." Met. and Chem. Eng., 1913, Vol. 2, p. 144.

B. P. Haigh. "Endurance of Steel under Alternating Stresses." West of Scotland I. S. Inst., 1913.

W. C. Popplewell. "Slow Reversals of Stress and Endurance of Steel." Inst. C. E., 1913-14, p. 264.

Repeated and confirmed some of Bauschinger's work. Recommends a method for predicting endurance limit as follows:—"Apply a number of reversals of stress just below the primitive elastic limit, and measure the new elastic limit in tension and compression. Repeat this a few times, using for each test a different value for the 'range of stress' applied. Deduce from the results what range of stress can be applied, so that this range will coincide with the new elastic range measured afterwards." The rotating beam in his bending tests was supported at both ends.

S. J. P. Thearle. "Note on some Cases of Fatigue in the Steel Material of Steamers." Inst. Naval Architects, 1913.

C. E. Stromeyer. "Fatigue Limits." Proc. Roy. Soc., 1914, Vol. 90, p. 1411.

C. E. Stromeyer. "Memo. on Fatigue Testing." Report to Manchester Steam Users Assoc., June, 1914.

Employed a calorimeter method for detecting fatigue limit. This is a fairly rapid test; agreed with ordinary methods.

H. M. Howe. "A Study of Simple Overstrain." Amer. Soc. Test. Materials, June, 1914.

Discusses the effect of this on the elastic limit and on the effect of subsequent reversals of stress.

P. Ludwick. "Relation of Static Strength to Fatigue Strength under Fluctuation of Load." Zeitscher. Vereines Deutsch. Ing., Feb. 8th, 1913.

J. H. Smith and Wedgewood. "Stress-strain Loops for Steel in a Cyclic State." Jour. Iron and Steel Inst., 1915, Vol. 1, p. 365.

H. Jordan. "Disturbance of Elastic After effect by Elastic Hysteresis." Deutsch. Phys. Gesell. Verh. 17.23, Dec., 1915, p. 423.

A. N. Mitinsky. "The Proportional Limit of Metals." Am. Inst. Min. Eng., Sept., 1915.

J. L. Uhler. "Vibratory Tests on Cast Steel." Am. Foundrymen's Assoc., Sept., 1915.

C. E. Stromeyer. "Law of Fatigue." Inst. Naval Architects, 1915.

W. Mason. "Speed Effect and Recovery." Proc. Roy. Soc. A., Vol. 92, 1915.

W. Mason. "Alternating-Stress Experiments." I. Mech. Engineers, March, 1917.

Describes alternating-torsion and bending experiments on mild steel. He obtained stress-strain loops, &c. Concluded (1) if alternated beyond the proportional range, cyclical set increases at first rapidly, then slowly. (2) Limiting shear stress in bending tests the same as that in torsion tests. (3) Solid specimens tend to give higher values for

limiting stress than hollow specimens. (4) A cyclical set does not necessarily mean ultimate breakdown, in say 100 million alternations.

Guest and Lea.	"Torsional Hysteresis of Mild Steel."	Proc. Roy. Soc., December 7th, 1916.
W. C. Popplewell.	"The Influence of Speed in Endurance Tests."	Engineer, October 20th, 1916.

Concludes that a slightly greater endurance is obtained with higher speeds. This is in opposition to Reynolds and Smith.

D. J. MacAdam.	"Alternating - Torsion and Impact-Testing Machines."	Amer. Soc. for Testing Materials, June, 1916, and Mech. Engineers, March, 1917, p. 61.
R. W. Bailey.	"Ductile Materials under variable Shear Stress."	Engineering, July, 1917.
B. P. Haigh.	"Experiments on the Fatigue of Brasses."	J. Inst. Metals, 1917, p. 2.
F. J. Schlink.	"Fatigue of Metals."	Eng. News Record, June, 1918.
A. R. Fulton.	"Experiments on the Effect of Alterations of Tensile Stress at Low Frequencies."	Brit. Assoc. Rep., 1919, p. 484.
W. E. Dalby.	"Researches on the Elastic Properties and the Plastic Extension of Metals."	Phil. Trans. Roy. Soc., Vol. 221, p. 117, 1920.

THE DISCUSSION.

Dr. Walter Rosenhain, in opening the discussion, said: The author has carried his experiments up to ten million reversals, whereas some others have not gone much above three million reversals, and it is a little suprising to me personally to find that the increase above the latter number makes very much difference. Whether that will be confirmed ultimately, when the work is repeated under conditions, perhaps, of greater refinement, remains to be seen, and at the moment it rather suggests that there should be a little reserve in our conclusions from the author's data. Dealing with the experimental side of the work, I would like to ask the author what precautions he took to avoid the occurrence of vibration in his machine, because even a small amount of vibration has a very serious effect on the results of tests of this kind. In his work at the National Physical Laboratory, Dr. Stanton takes extraordinary precautions to avoid any possible risk of vibration, not only by placing the machine on a large concrete block with very solid foundations, but by using other devices, such as vanes and plates immersed in thick oil to insulate the load. The reason why vibration matters bears very much on the whole question of fatigue, and was investigated some years ago by a man of the name of Smith, who published a paper which at the time did not attract a great deal of attention, but his results are now being verified. They show that if a test piece is fitted with an extensometer and loaded in tension at a certain intensity of stress, a departure from proportionality of strain to stress occurs; this is what we call the elastic limit or the limit of proportionality. It is not the same thing as the fatigue limit, but if an alternating stress under compression and tension of say only $\pm$ 4 tons per sq. in. is added to an existing static load on a test piece of a material of which the Wöhler limit is of the order of 14 tons, with a limit of proportionality of from 15 to 18 tons, depending on the exact condition of the material, and if the elastic limit is then determined on the extensometer, it is found that the departure from proportionality coincides in the most surprising manner with the actual fatigue limit as found by the Wöhler test afterwards. While discussing this the other

(Dr. Walter Rosenhain.)
day, I suggested that it was rather like tapping a thermometer to prevent the mercury sticking, because if the metal is agitated by a slight alternating load the apparently false value of elastic limit under ordinary tensile stress is destroyed and the metal begins to deform at the same point as under the fatigue test. The question is being followed up, and I hope that further light may be thrown upon the subject, because if it does it will avoid the necessity for waiting for ten million reversals at 1,500 per minute.

Dr. HATFIELD: Each ten million test piece was revolving for five days continuously.

Dr. ROSENHAIN: Then it might be worth while to go to 2,000 reversals per minute, which is quite safe.

In connection with the test piece it has been found that more consistent results are obtained if these are made hollow. The reason is that the distribution of stress in a cantilever rotating in that way is not actually known, although the author's calculations are quite correct so far as conventional theory goes; I am not sure, however, that they will give results strictly corresponding to the facts, because deformation takes place and we do not know whether the distribution of stress occurs in a uniform manner or not. If, however, the test piece is reduced to a thin tube instead of having a complete circular section, the stress is very much more nearly uniform, and although there will still be a departure from the calculated stress it is not likely to be nearly so great. There are, however, objections to hollow test pieces in the case of steels which have been hardened and tempered, because if they are bored out after treatment they are apt to distort and give an oval section, and in some cases misleading results have been obtained on that account.

Now, turning to the author's actual results, I would like to say that so far as steels are concerned he has not treated carbon steels fairly, because he has given a comparison between normalised carbon steels and tempered and quenched alloy steels. His argument that carbon steels cannot be successfully hardened and tempered, except in thin sections, may be right or wrong, but the comparison of the materials is a little unfair (in its technical sense) on that account. It would have been very interesting to see what the difference would have been between these alloy steels put into as nearly as possible the same micro-structural condition as the carbon steels for comparison. I think even there

he would probably have found the alloy steels better, not very much better, but still better than the carbon steels, because there is no doubt that the presence in solid solution in ferrite of nickel and chromium stiffens that ferrite as against a carbon steel and, therefore, gives a higher Wöhler figure. In the hardened and tempered material I do not think, even in carbon steels, that we have ferrite at all; I think we have an entirely different substance the character of which I will not mention, because I do not want to draw a red herring across the discussion. Its constitution, however, is undoubtedly similar in all hardened and tempered steels, but with this difference, that in the one case it is iron-carbon alone, and in the other case it is iron-carbon plus nickel and chromium, with the corresponding improved result.

One of the most interesting points in the paper was contained in Tables II. and III., one dealing with Duralumin and the other with brass. The figures given for the tensile strength of Duralumin are a great deal higher than usual. I have tested many specimens, and they have rarely given a maximum stress of 29·9 tons or an elongation as low as 16 or 14 per cent. That material is unusually hard for Duralumin, although the fatigue limit found by the author agrees with that found by the Wöhler test at the National Physical Laboratory, and by a totally different kind of test in which a flat bar was bent backwards and forwards to a known curvature by means of an eccentric. All these tests agreed in giving somewhere about ± 10 tons as the alternating stress limit for Duralumin. It is very surprising, but very disappointing, that these aluminium alloys give such low alternating stress limits. It must be remembered that to increase the alternating stress range of a material by even 10 per cent of its value means a great deal more than increasing the ultimate tensile stress by 20 or 30 per cent. With regard to the change in the properties of Duralumin with time, a month or two, or even twelve months, is not enough. I have recently had the opportunity of testing some Duralumin which was a year old when it was first tested and is now over eleven years old, and it undoubtedly has undergone a further change in the interval. I must confess that the author's figures, indicating that cold-worked brass gives a higher fatigue range than the same material in the annealed condition, are altogether surprising to me and in entire contradiction of everything that has yet been done on the subject. I do not know how much has been done on non-ferrous metals, but on

(Dr. Rosenhain.)
steels Bauschinger, Stanton, Bairstow and others have shown that raising the elastic limit in tension by cold-working does not shift the fatigue range. If the same thing is not the case in non-ferrous alloys it would be extraordinarily interesting to prove it, and if the author has proved it he has removed by that the worst of the outstanding difficulties in the path of his pet bugbear, the amorphous theory.

Major H. P. Philpott: The fatigue range of the nickel chromium steel in the air hardened condition is very low in relation to the ultimate stress, and this is distinctive for the air hardened steel, which gives a fatigue range something less than 30 per cent of the ultimate stress. I have always felt a bit doubtful as to the use of steel in such a condition. We know that in the tensile test the stress-strain diagram departs very early from the straight line, giving a very low limit of proportionality in relation to the ultimate stress. I know that for many parts of aircraft for which this steel in the air hardened condition was intended to be used during the war, we frequently substituted a nickel chromium steel in the hardened and tempered condition, and without any detrimental effect on the life of the part. This may have been covered by the fact that the design was heavy enough for the steel of lower tensile stress. The fatigue range found by the author for the high tensile stress condition is very little higher than that for the tempered conditions of the alloy steels. Thus the author quotes a fatigue range of about 32 tons per sq. in. with an ultimate stress of nearly 110 tons per sq. in. obtained from the air hardened condition, and fatigue ranges of 28 tons per sq. in. with an ultimate stress of 62 tons per sq. in. and 27·5 tons per sq. in. with $56\frac{1}{2}$ tons ultimate. Having regard to these values, is there much practical advantage gained by using the steel in the air hardened condition?

Mr. A. A. Remington: I am very much interested in the author's research, but I think it is a great pity that he has expended such an enormous amount of labour in carrying out tests with test pieces of a form that, while they give concordant results between themselves and enable certain calculations to be made from them, do not represent practice. I do not think that any automobile engineer would dream of revolving a cantilever of unequal section under considerable stress or of making a cantilever for such conditions out of normalised carbon steel, so that these figures become only of interest as a comparison from a

scientific point of view, and are not in themselves directly useful.

The author finds that when he calculates the stress in these Wöhler test pieces by the usual static formula they will carry a higher calculated ultimate tensile strength than will a simple tensile specimen of the same material, which, of course, is only to be expected. He also finds that they will carry a considerable number of alternations at a stress which is calculated to be considerably above the maximum tensile strength of the material. He then makes a correction for that stress, and says that the real stress in the specimen is less than the stress that is calculated by some formula, and I should like to ask him to go into far

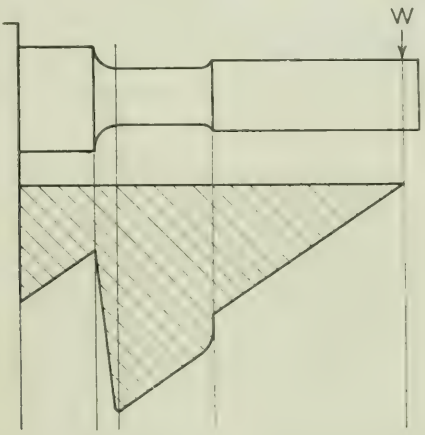


FIG. 5.—Wöhler test piece. Stress distribution diagram.

more detail in regard to that particular question in his reply to the discussion.

The criticism of the test pieces is based primarily on the irregularity of the stress distribution. The normal Wöhler cantilever test piece and the corresponding form of the skin stress distribution diagram are shown in Fig. 5. It will be seen that the maximum skin stress occurs at only one section, and the rise to this maximum is comparatively rapid, adjacent sections being stressed to a much less extent, as is also the metal within the skin. If, then, deformation takes place, not only is the stress lowered by the additional support afforded to the most highly stressed area by the inner metal, but also by the support from the adjacent less highly stressed metal, the phenomena being exactly parallel to the case of the higher apparent maximum specific breaking

(Mr. A. A. Remington.)
strength obtained in the tensile machine from a notched bar as compared to the usual smooth parallel specimen.

In the case of the modified Wöhler beam specimen as used in the Eden-Rose experiments, this defect is overcome, and uniform bending is obtained throughout the full effective length of the specimen without any sudden changes of stress, as shown in Fig. 6.

Fig. 7 shows the cantilever specimen as deflected under load. In this condition the specimen is in the same condition as a loaded spring, and will have a definite rate of vibration depending on the deflection and expressed by the "pendulum" formula

$$t = \pi \sqrt{\frac{D}{g}}$$

t being the time of a single oscillation.

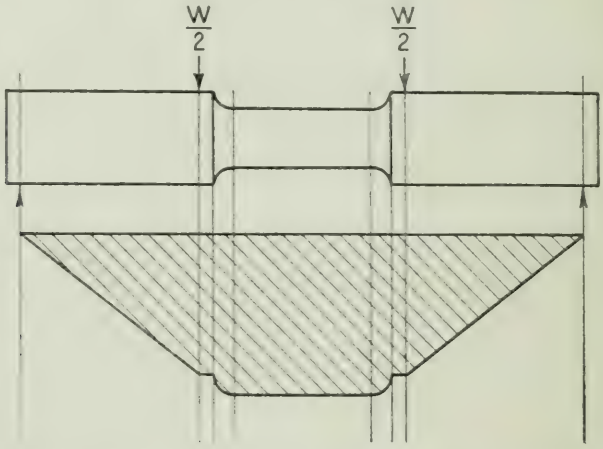


FIG. 6.—Eden-Rose test piece. Stress distribution diagram.

It is usually assumed that the speed at which a Wöhler test is run is immaterial, and this is probably true for speeds far removed from synchronisation with the oscillation speed, but it would be interesting if the author would ascertain in regard to the tests what is the deflection and the corresponding critical speed. In any case, as strain under a gravitational load takes a definite time to develop, as expressed by the above formula, when the test piece is running the maximum stress must be less than when it is stationary by the amount the point of maximum stress, when running, lags behind the static point of maximum stress which is exactly opposite to the load.

This lag, which is a time interval for a given deflection, will

therefore vary directly as the speed, and the deflection measured when running should be less than when standing, the difference bearing some relation to the speed of rotation, and would be one more factor making a difference between the apparent and the actual stress.

What the author has been testing for us is in reality the effect of a notch in a beam under alternating stress, a condition of affairs which no automobile engineer would tolerate. The beam type of specimen, shown in Fig. 6, would have given results of considerably greater utility.

I presume the stress was calculated from the usual formula $(\pi/32)\, d^3\, f$, and that involves another reason why the calculated stress is higher than the true stress, in so far as the expression $\pi/32$ is only true so long as the material is not deformed. Since the material deforms elastically or plastically, the value 32 in the divisor modifies, and the extent of the modification is considerable

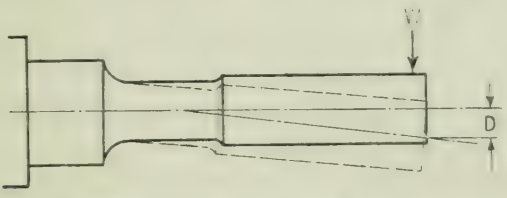


FIG. 7.—Wöhler test piece showing deflections (exaggerated).

when the strain is considerable. It would be interesting if the author would give the static deflection of the test piece under each load, and also the deflection when running before failure commences. This information would enable a considerable amount of light to be thrown on the question of the difference between the apparent stress and the actual stress. In connection with the results obtained from the carbon steels, I am not satisfied as to the reason why the high carbon steels show lower results, and I should be glad if the author will add the nickel and chromium content to the analysis of the carbon steels. Is he quite sure that there was no nickel or chromium in all the carbon steels he tested, because slight amounts, particularly of nickel, are liable to affect the results?

Except from a purely scientific point of view, a Wöhler test on the core of a case-hardened steel specimen in the water quenched state is of little utility. In the first place, the case which would be present in practice and would profoundly modify

(Mr. A. A. Remington.)
the results is absent, and further, the quenching of the specimen, if it were quenched in finished dimensions, would also modify the results. Will the author inform us as to the diameter of the specimens at the time of heat-treatment and the amount machined off?

The author tells us that he has tested many specimens up to ten million reversals, and he is on safe ground in telling us the stress which the material will stand at that figure, but when he extends the curve to infinity he is on very dangerous ground. If the tests are plotted against the reciprocal of the number of reversals, any extension of the curve is still dangerous, because the number of reversals between ten millions and infinity is not much less than between one million and infinity, and therefore there is great danger of inaccuracy in suggesting that what happens at infinity is very closely approximated to by what happens at ten million revolutions.

Dr. HATFIELD: My point is that it is not the same thing. There is a great difference between the two.

Mr. REMINGTON: I think it is quite possible that the stress at which specimens will stand infinity reversals is zero, although there is so far no definite evidence.

The limit of proportionality of air hardening steel was referred to by Major Philpott, and it is of course extremely low. Possibly the vibration method would make a difference and show that the limit of proportionality bears some relationship to the considerably higher Wöhler figure of air hardening steel. In summing up, the author showed that the Wöhler value of alloy steels, taking an average for the range, was approximately one and a half times that for carbon steels in the normalised state. Assuming that the carbon steels in the hardened and tempered state show somewhat higher Wöhler values, the ratio would be less than $1\frac{1}{2} : 1$. It is unfortunate that that ratio does not compare very favourably for alloy steels with the present price ratio between carbon and alloy steels.

Dr. HATFIELD: I suggest that prices should be ruled out of this discussion.

Mr. REMINGTON: Am I in order, Mr. President, in referring to the price ratio in its relation to the Wöhler test ratio?

The PRESIDENT: I do not think that is bringing in commercialism very much. The only thing is that, having regard to fluctuating prices, you would have to be very careful in what

you said, if you put it in that way. We are naturally governed by prices from the design standpoint.

Mr. REMINGTON: I was saying that the price ratio of alloy steels does not compare favourably with the difference in Wöhler value.

Dr. LESLIE AITCHISON: I spend the greater part of my days now in examining broken automobile parts, and I find that about 95 per cent of them have broken from fatigue, and generally from fatigue under a rather complicated system of stresses such as the Wöhler test does not really help us to investigate. Nevertheless, I agree with the author very largely that the Wöhler test is the most convenient method of determining the resistance which materials do exert to fatigue stresses. Both during the war and since the war I have been engaged in carrying out, or directing the carrying out of, a series of fatigue tests which are very similar to those which the author has himself superintended, and, curiously enough, the steels that I have examined have been very similar to those which he has examined. As a result, I disagree with him not only on his numerical values, but upon some of his theoretical conclusions. This disagreement, however, is one which might be explained by differences in the form of the specimen and the method of testing. However, I do claim that our results are entitled to a certain amount of respect, for two reasons. The first reason is found in the qualifications which those gentlemen themselves possess. A lot of the work has been done at the National Physical Laboratory by Dr. Stanton, and more has been done by Professor Robertson, of Bristol, by Dr. Haigh, of Greenwich, and by the testing department at the Royal Aircraft Establishment. The second reason is that they have been supported by fatigue tests which have been carried out not only in the Wöhler machine by the Wöhler method, in which the material is stressed in tension and compression alternately by bending, but in the Haigh machine by the Haigh method, in which the material is stressed alternately in tension and compression by a direct pull and push. Moreover, in the Wöhler test the stress is concentrated on the outer skin of the material, whereas in the Haigh method the intensity of the stress is equal right across the section. The fact, therefore, that the results obtained by the Haigh method very largely confirm the tests obtained by the Wöhler method tends to make me more inclined to believe the results which we have obtained rather than those which the author has put forward in his paper.

(Dr. Leslie Aitchison.)

The author stated that he finds the elastic range is distinctly different from the Wöhler range with certain of his materials—that the limit of proportionality, as he calls it, is lower than the Wöhler range which he obtained. I would like to ask him what he really means by that, what he really understands by the elastic range. In that connection I would venture to put forward some rather interesting figures which have been obtained in some recent experiments in connection not only with this particular point, but also with the limit of proportionality found in fully hardened steels. In a series of experiments which has been carried out on a 5 per cent nickel steel, possibly almost exactly similar to that which the author has experimented with, the specimen was quenched right out, and therefore hardened drastically, and

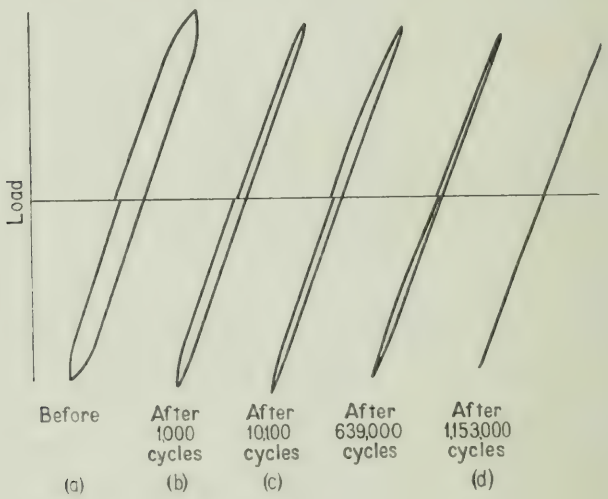


FIG. 8.

was consequently in a condition in which it possessed a comparatively low limit of proportionality. Actually, the tensile strength of that steel was about 59 tons. The observed elastic limit of the material was very low, only about 12 or 13 tons. The specimen gave the hysteresis loop in a stress/strain curve shown in Fig. 8a. An exactly similar specimen was then stressed by alternating tension and compression in the Haigh machine, and it was found that, after stressing for only a very small number of reversals, the hysteresis loop was beginning to close up, Fig. 8b. The further application of alternating stresses resulted in the production of a hysteresis loop which was still smaller, Fig. 8c, until finally, after about a million alternations of stress, the hysteresis loop was no longer a loop, but was to all in-

tents and purposes a straight line, Fig. 8d. When the material had been stressed at a varying number of alternations, the stresses were examined statically, i.e., the limit of proportionality was examined, and it was found that the application of quite a small number of variations of cycles of stress had raised the apparent elastic limit appreciably, and that whereas before any alternating stress was applied the elastic limit of the material was only about 12 or 13 tons, after applying 1,000 revolutions it had risen to 15·5 tons, and after applying 1,000,000 revolutions it was about 23 tons per sq. in., although in no case was the material stressed beyond what had previously been determined as the apparent elastic limit. This, of course, is a very definite confirmation of the point that Dr. Rosenhain mentioned in connection with the work of Smith, and is another instance, probably, of tapping the thermometer. Whether it is purely a question of tapping the thermometer or whether it is something producing a structural alteration will have to be decided by further work.

I disagree with a great deal of what the author has presented in the shape of results, because we found in most of the work which we have done that there was a comparatively definite and constant relationship between the fatigue range, i.e., the sum of the plus and minus fatigue stresses when the material is stressed under equal alternating plus and minus stresses and the maximum stress. The sum of the plus and minus stresses, being the fatigue range, was approximately equal to the maximum stress, and we found this result held for a very large variety of steels. I would mention that the investigations were not all done on the same machine nor on the same kind of specimen. They were done by different people at different times on different pieces of apparatus, and it was found that the plus and minus fatigue stresses added together, generally speaking, were equal to the maximum stress. The actual steels which were tested were firstly an air hardening nickel chromium steel, not only in the air hardened condition, as the author adopted, after tempering at temperatures from 500° to 600° C.; nickel chromium steel and chromium vanadium steel in the same condition as the author used; plain carbon steel, both normalised and hardened and tempered; plain carbon case-hardening steel; 3 per cent nickel and 5 per cent nickel case-hardening steels, and also cold-worked steels, both in the cold-worked condition and after re-heating at such temperatures as would produce an appreciable alteration

(Dr. Leslie Aitchison.)
in the elastic limit, and it was a surprising thing that the majority of these steels—in fact, I think, all but one—gave figures which showed that the ratio between the fatigue range and the ultimate stress varied only from 0·96 to 1·04, which I think will be agreed was a very close degree of regularity. I would only refer to one point about these results, namely, that it is desirable to test carbon steels and alloy steels in the same condition of structure, i.e., after similar heat-treatment, or presumably, after such heat-treatment as will produce similar structures, namely, the hardened and tempered structure. In the tests I have referred to, that was actually done, and the hardened and tempered material was examined in each case, and it was found that the hardened and tempered carbon steel had the same ratio between its fatigue range and maximum stress as had the hardened and tempered alloy steel.

It appears now that it is very desirable that whatever work is done upon fatigue should be in the direction of finding, if possible, some relationship between the fatigue properties and the properties which can be measured by the engineer with a reasonable amount of ease and success. It is highly desirable to know the fatigue range of the steel, and all such work as that which the author has done is of immense value in going towards that end, but the actual determination of the fatigue limit of a piece of steel is not a thing to be entered upon lightly, and is a thing which I am sure all engineers will oppose, in so far as specification purposes are concerned, until such time as we can say that the fatigue range has some relationship to a property which can be determined in less than five days.

Mr. L. A. Legros: I would like to add a little to what Dr. Rosenhain and Mr. Remington have said, to the effect that when a piece that is revolving all the time with the load suspended from the end is subjected to bending, it is important that something should be known about the stress transference that takes place due to the speed of rotation; it is also important that a record should be made of the amplitude of the vibration, because a very small vibration will cause a very large increase of stress. The range of deflection corresponding to the observed vibration should be measured, so that it may be known whether the stress variation is of the order of 1 per cent or of 5 or even 10 per cent.

Another point is of great importance. The figures given show a comparison of tests made by direct tension with the calculated

tensile stresses for a beam of circular section. Any material that is tested in bending gives quite different results according to the stress distribution. In a beam of joist section, there is a very small area near the neutral axis subjected to low stress, and a big area at the flange subjected to high stress, but if a rectangular section is taken the distribution of stress is quite different, and whereas the strength of the joist will correspond closely with the result of calculation, the strength of the solid rectangular beam will exceed the calculated strength in a ratio which may be called θ. If the beam instead of being of rectangular section is of circular section there is a worse stress distribution, and the ratio θ is higher, and if a cruciform section is taken a still higher value for the ratio θ is found. This ratio in the case of cast iron has been found to have a value rising from 1·2 in joist section to from 1·9 to 2·0 for a turned cylindrical beam,* and it may reasonably be expected that the value will be of the same order for steel and perhaps even greater for the tubular test pieces that have been suggested.

In order that any deductions of real value may be made it appears therefore that it will be necessary to compare the yield-point, the elastic limit and the breaking stress actually found in direct tension and also in bending with the stresses produced in the stress repetition apparatus.

Mr. S. N. Brayshaw: I should like to ask the author whether he has done anything in the direction of varying the speed of the reversals. I rather think that there may be a critical speed at which the best results would be obtained. If the piece is deformed and then released, it will take a certain time to return, say, one-thousandth part of a minute. I take it that in that case 1,000 reversals would be the speed at which the best result would be obtained. The piece would be stressed if it were compelled to return more quickly than it would do naturally. On the other hand, a very slow speed might be expected to give worse results. The author has worked at 1,500 reversals per minute, but I should like to see the results of a few trials at 150 and at 15, incidentally taking something over a year for each specimen. At all events, without going to such an extreme, I would like to know more about the effect of different speeds and if there is a critical

* Proc. Inst. C. E., Vol. LXXXVI., pp. 235 *et seq.* De Segundo and Robinson. Experiments on the relative strength of cast iron beams. The ratio is given as θ on p. 244, and its values on pp. 248 and 249.

(Mr. S. N. Brayshaw.)
speed. Of course, it would vary with the design of the specimen and also with the material. We should presumably get more consistent results by eliminating all vibration, but if it were possible, I should prefer not to eliminate but to control it, so as more nearly to approach the usual conditions, and consequently give more useful knowledge.

Dr. A. P. Thurston: I agree with the author that the elastic limit is more or less useless as a basis of comparison of the properties or strength of various steels. At the beginning of the war, for instance, it was quite a common thing to get air hardened axles or air hardened steel parts in which the elastic limit was certainly not above 15 tons per sq. in., although the ultimate strength was at least 90 tons. The yield point was found to give fair results, but we got a term which gave more reliable results, and this we defined as the "effective strength," which is the stress at which there is a permanent set of one-tenth of one per cent of the length. This set is one-fifth of the set at the yield as defined in standard specifications. This experience, I think, fairly agrees with that of the author.

Another extremely interesting point brought out by the author is the effect of cold-working. Great use was made during the war of the properties given to steel by a combination of cold-working and heat-treatment, and plain carbon steel so treated was used in many cases to replace more expensive alloy steels. It was found, for instance, that if a plain carbon steel, say 0·3 carbon, were subjected to cold-working, a tensile stress of about 65 tons per sq. in. could be obtained with practically no ductility. If this steel was then subjected to "bluing" or tempering at about 400° to 450° C. the tensile strength could be increased by about 5 tons and the elongation to about 7 per cent. By applying this process to steel tubes during the war the output of the country was increased in 5 months from 800,000 ft. per month to 2,500,000 ft. per month without any special government contract. Moreover, the tubes complied with a higher specification and were cheaper, and I never came across any trouble owing to failure from crystalisation or fatigue. Has the author made any experiments to ascertain how far mechanical change in the shape of the crystal adds to the life of steel under fatigue?

There is one further point which came home to me very clearly as a user of steel, i.e., the importance of carrying out all tests under conditions as nearly identical as possible with those

under which the steel is to be used. This is one point where I may appear to differ from the author, but I think the difference is more apparent than real. I fail to see the use of the Wöhler figure for steel which is never going to suffer reversals of stress. If the steel is going to be used under bending it should be tested under bending. If under tension, then a tensile test is good enough. If it is to be subjected to a limited vibration, then by all means subject it to a vibration test, but under the combination of stresses to which it will be subjected in use. By attention to this point many unexpected difficulties were solved. I found that I could increase considerably the strength and life of many metal parts subject to compound stresses by so designing their shape that when the skin stress passed the elastic limit the yield of the material allowed the stress to be efficiently distributed. If, for instance, a beam is loaded until it begins to yield, and the elastic limit in tension is greater than the elastic limit in compression, the stress runs across the section, crosses the neutral line, which shifts its position, and then attains rapidly a much greater maximum tensile skin stress than the maximum compressive skin stress. The skin stress calculated from the failing load will obviously differ considerably from the actual failing skin stress. This does not matter very much if the results are to be used only in calculating the strength of beams, but if, in addition, other loads are to be applied, it matters very much. For instance, if the beam is subjected to axial end loads in addition to bending moments, then the change in the position of the neutral axis, due to the uneven stress diagram on the section, allows the end loads to introduce an added bending moment which is entirely neglected in calculation. I have found cases during the war in which the actual failing stress differed by as much as 35 per cent from the apparent failing stress. May not something of this sort account for some of the unexplained differences in the Wöhler and other tests?

A final point is with regard to the properties of nickel chromium and other alloy steels tempered, say, between 200° and 400° C. As is known, the impact or Izod test results fall very much between these temperatures, although many of the other properties are maintained. It is clear, I think, that the Wöhler test is dependent in some way on the shape of the crystal and also in some way on the variation in the elastic limit or limits of proportionality in tension, compression and shear. Has the author investi-

(Dr. A. P. Thurston.)
gated the effects of the Wöhler test, and can he give more information on the point?

Mr. S. Whyte: We have, at the Associated Equipment Company, carried out a large number of tests on the Wöhler type of machine, and I can quite agree with the author that it takes a considerable time to get any results; Dr. Aitchison, upon this point, is rather optimistic. I should think that to study one type completely with varying heat-treatments would take at least four or five months, so that it can be seen that fatigue testing is not a thing which is going to be used as a rapid standard test for the properties of a steel. What we really want to do is to correlate this test with some other more simple one, and that is the aim of our research. Unfortunately, so far, we have not been able to confirm the results which Dr. Aitchison has put forward. We have found with certain steels, such as nickel chromium air hardening steel giving 105 tons tensile strength in the air hardened condition, that if we temper through a range up to 650° C. there is an increase in the tensile strength or maximum strength up to about 200° C., and then it falls down gradually between that and 650° C. With the fatigue test the safe range of stress in the air hardened condition is approximately 32 tons per sq. in. This rises to about 34 tons per sq. in. at 200° C., and then falls very low (to approximately 28 tons) between 300° and 400° C. Between 400° C. tempering, the safe range rises higher than that of the air hardened condition (over 40 tons per sq. in. at 500° C.) and then falls uniformly.

This is borne out so far as we have gone by torsion tests. Torsion tests made on air hardened nickel chromium steel will give a shearing stress of 100 tons per sq. in. in the air hardened condition. This will fall to 67 tons per sq. in. when tempered between 300° and 400° C. The angle of twist in the air hardened condition is 90 degrees, while between 300° and 400° C. it falls to 21 degrees. The drop for these temperatures is therefore very marked indeed.

Fig. 9, Plate XXV, is a micro-photograph of the air hardened nickel chromium steel showing martensite. Between 300° and 400° C. this changes, and the needle structure of martensite almost disappears, as shown in Fig. 10, Plate XXV. Beyond 400° C. it entirely disappears and is replaced by probably troostite. The change from the martensite is not the regular one, and a definite intermediate product seems to be present.

With regard to the question which was raised by Mr. Remington, if Prof. Coker's method of studying the distribution of stresses is used, it will be found that with a notch in a loaded beam, the neutral axis goes up into a sharp point at the base of the notch (Fig. 11, Plate XXVI), and the stress increases very rapidly in a short distance, so that the stress diagram is not a rounded curve. This effect of a notch also explains why it is necessary to have a good radius of, say, one inch on the test pieces for the Wöhler test. In addition, it is absolutely necessary to remove all the fine machine marks by polishing, otherwise the results will not be concordant.

One point that might be mentioned in connection with fatigue tests is that chromium vanadium steel appears to give better results in service than 3 per cent nickel medium carbon steel. Comparing the safe range of stress for chromium vanadium steel against the nickel steel for heat-treatments on each, so as to have approximately the same tensile strength, the former gives a higher safe range as determined by the Wöhler test.

Dr. Hatfield, in replying on the discussion, said: The discussion has brought out a number of points of value. This investigation is likely to last for a number of years, and it is useful at this stage to have the opinions of other investigators on the work they are doing. Several speakers have been inclined to take this research on its own merits, but I would again direct those gentlemen to the bibliography published in the paper, which contains between seventy and eighty references to other papers which have been presented, some of them covering objections which have been brought forward to-night, and it was only after a careful study of this existing work that the method described in the paper was adopted. The beauty of the Wöhler rotary bending test is that it is simple and calls for a very workmanlike apparatus, and there are not those risks of introducing variables which may produce very discordant results which exist in many of the more widely used auxiliary tests.

Turning first of all to Dr. Rosenhain's remarks, I am glad that he thinks that ten million reversals is quite a long run with these specimens, and so it is, but we have in some cases carried the test on to thirteen millions and even sixteen millions, and the fact that we have had rupture at the thirteen and sixteen millions clearly indicates that long though we have run these test bars,

(Dr. Hatfield.)
we have not run them long enough. Still, I think Dr. Rosenhain will concede that the method by which we plot these curves does give an indication that to have a number of points on the curve ending at ten millions will provide engineers with something really useful. The question of vibration which was raised by Dr. Rosenhain is quite important, and has had our attention. Perhaps next time he is in Sheffield he will come and examine the machine. It is, of course, a fact that in a research with a machine of this kind, the vibration, within limits, actually varies in the course of the experiment, so that, when considering these results or comparing them with those of any other investigator, such variations must be allowed for as factors which lead to a certain amount of experimental error, against which I have already uttered a warning. I may say that in the early stages of this investigation, before we commenced to take readings seriously for record purposes, this machine was not running truly. We had a lot of vibration, and we completed the test on one steel under substantial vibration, and yet when the test was carried out with minimum vibration, the lines practically fell on top of each other, so that I am inclined to think that vibration is not quite as important as it might appear on the surface. There is one point that Dr. Rosenhain made which I want to controvert. We have used solid test pieces, and Dr. Rosenhain has suggested that we should use hollow cylindrical test pieces. On theoretical grounds I agree with him, but the difficulty of producing a perfect hollow cylinder free from internal machining defects is so great that, in view of the large number of tests, we discarded the idea in favour of the solid test piece as being a much safer routine form of test piece, and I think Dr. Rosenhain in effect said the same thing before he finished speaking. The Duralumin we worked with was actually obtained from one of the aircraft factories, and was typical of the material used during the war. I think the figures I have given in regard to raising the limit by cold-working in the case of brass are of fundamental importance. They have a very considerable theoretical value, and yet I am able to say that we have experimentally, with steel, gone over this ground as to the effect of cold-working on the tensile and compressive strengths, and find that the views previously held are not borne out even in static work; we have been able to confirm in steel this very important fact which I have put forward to-night, that metals when cold-worked give a higher fatigue range than when annealed.

That set of facts, which I have no doubt will be confirmed by other investigators, does form a definite advance in our knowledge of cold-worked metals.

Captain Philpott spoke in rather a deprecating manner with regard to air hardening steel. It should always be borne in mind that whilst, as I have pointed out, the percentage of maximum stress shown in the Wöhler test is low, yet the fatigue range of the air hardening nickel chromium steel is substantially higher than the fatigue range of any other steel I showed.

I am rather surprised at the attitude which Mr. Remington took up with regard to the manner of testing employed. This Wöhler rotary bend fatigue test has been known and appreciated for many years by numerous investigators at home and abroad. Mr. Remington now launches several criticisms at the rotating cantilever test piece. Each criticism applies equally well to the Wöhler beam specimens used in the Eden-Rose experiments to which reference is made. The suggestion that in the cantilever specimen there will be a definite rate of vibration depending on the deflection is equally applicable to the beam specimen, of which fact Mr. Remington must be perfectly well aware. When Mr. Remington talks of the time necessary to develop strain, surely he is not suggesting that it takes time to develop stress (stress travels at 17,800 ft. per sec.). Mr. Remington is suggesting that the stress actually increases without increase in load. Perhaps he will think this matter over again.

With regard to Mr. Remington's remarks upon the method of calculating stress, this is fully explained in Appendix I., and therefore does not need further discussion except to say that exactly the same criticism applies to the beam form of test piece as to the cantilever.

I do not propose to criticise the Eden-Rose form of test, because I am quite in agreement that it is a good form. So, indeed, are the other forms of fatigue test which Wöhler himself used.

Mr. Remington states that in his opinion the stress at which specimens will stand infinity reversals is zero. I do not agree with this view, neither do physicists generally. Were Mr. Remington's suggestions true, the whole usefulness of the theory of elasticity would fall to the ground.

As regards heat-treatment, except in the cases where the specimens were tested in the hard condition, they were treated

(Dr. Hatfield.)

in $1\frac{1}{5}$ in. diameter round bars. The case-hardening steel bars were machined after heat-treatment.

I am interested to see Mr. Remington's abhorrence of the notch effect. This indicates a tendency to conversion on his part which I welcome. Whilst discussing the effect of notches, is Mr. Remington aware that there is evidence to show that the influence of a notch in a cantilever test piece is not influenced by the notched brittle or notched toughened condition of the material?

I can assure Dr. Aitchison that the percentage yields which we have found, the dissimilar yields for the different steels, are amply established, and I feel sure that if he goes over this ground again he will confirm the values which I have put forward to-night. I was glad to hear Mr. Whyte say he took the same view.

COMMUNICATIONS.

Baron G. Steinheil wrote: It is interesting to note the behaviour of wrought iron as stated at the beginning of the paper, and I do not think that motor car designers pay sufficient attention to these properties, otherwise wrought iron would be used for certain parts of cars which have to resist shocks and vibration. This certainly might involve a little extra weight, but the safety of the car would be much increased. Many accidents have been caused by the breakage of such parts as front-axles, steering links, etc., made from alloy steels having a high limit of proportionality, approaching very nearly the maximum stress, and only a small elongation. If such parts were made of wrought iron, they would show signs of damage before they broke, thus giving ample warning of the need for replacing them. I am convinced from the following experience of my own that wrought iron can be successfully used for back-axle half-shaft: The right hand driving half-shaft of the back-axle of my car, which was of a Russian make, broke while I was motoring in a remote part of Russia. The part in question was made of alloy steel (NKHr Bismark Hütte), and broke near the collar of the square engaging the hub of the wheel, owing to too small a radius at the change of section. As no spare could be procured, I obtained a wrought iron bar in a village workshop, and from this I turned a replica of the broken axle, filing up the square fitting the hub. The return journey (about 350 miles) was made over a variety of roads at speeds up to 60 miles per hour. Upon arrival at the destination I found no fault in the new part and put it back again to see how long it would last with the car in daily use. Three years later that car, still with the same axle, was requisitioned at the outbreak of war by the military authorities. I think that this case proves that wrought iron can be used successfully for the parts mentioned above.

In reply to the written communication, Dr. Hatfield wrote: I was much interested in Baron Steinheil's account of the use made of native wrought iron for the half-shaft of the back-axle of his car. It was certainly a very interesting contribution to

(Dr. Hatfield.)

the discussion, and his experience is not a surprise to me. It will be noticed in the Wöhler tests that the wrought iron stands very considerable plastic deformation above the elastic range before fracture. At the same time, I am of the opinion that the axle-shaft referred to must have been of good dimensions, sufficient, at any rate, to take care of normal stressing.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

April, 1921.

The Tenth Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James' Park, London, S.W.1, on Wednesday, April 13th, 1921, at 8 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following list of Members who had completed their election since the previous Meeting was posted up:—

Members.

Gerald Bunn (Birmingham).
Raymond James Mitchell (London).
Frank Newton (London).

Associate Members.

Herbert Clively Ellis (London).
Kennard Barnett Emerson (London).
Ernest William Johnson (London).
Clarence Evelyn King (Luton).
Bruce Howard Longstaffe (London).
Dorothee Aurelie Marianne Pullinger (Miss) (Dumfries).
William Somerville Renwick (London).

Graduates.

Alan Ibbotson Greaves (Grahamstown, S.A.).
David Arthur Hughes (Liverpool).

Probationary Graduates.

Horace Sydney Claude Bassett (Crowthorne).
Joseph Bentley (Cleckheaton).
Stanley Thomas Henn (London).
John Charles Douglas Howland (Geneva).
Frank Senior (Bradford).
Denys Stephen Dodsley Williams (London).

Transfers.

George Coombes (London), Associate Member to Member.
John Lamming (Sedgeley), Probationary Graduate to Graduate.
Harry Frederick Richardson (Birmingham), Probationary Graduate to Graduate.
Cyril William Passmore (Wolverhampton), Probationary Graduate to Graduate.
Herbert Leslie Vinsen (Luton), Probationary Graduate to Graduate.

The Chairman then presented the Daimler Premium for Session 1919-20 to Mr. D. L. Prior for his paper on "Epicyclic Gearing."

The following paper was then read and discussed:—

"A Suggested Programme for Automobile Research," by Geo. W. Watson.

There were present 68 Members and visitors.

A SUGGESTED PROGRAMME FOR AUTOMOBILE RESEARCH.

By GEO. W. WATSON, M.I.Mech.E.
(Vice-President).

In approaching the task of preparing a suggested programme for automobile research, the author does so with great diffidence, feeling that the matter is one with which it is not possible to deal adequately within the limits of any one man's personal experience. He has, therefore, obtained the views of other members of the Institution, and to them, and particularly to Mr. A. A. Remington, he is indebted for much valuable assistance and advice.

Research and standardisation have been among the pressing needs of the automobile industry for many years, but it was the upheaval of 1914 which brought this need more prominently before us. A meeting of the Council of this Institution, in September of that year, was specially convened to decide the lines upon which the Institution could best help the industry during the anxious period then before us, but the lack of funds prevented them taking any serious steps forward. The first definite move in the direction of research was taken, the author believes, in 1915, by the Technical Committee of the Society of Motor Manufacturers and Traders, and shortly afterwards a joint committee of that Society and the Institution of Automobile Engineers was formed to undertake certain research work in connection with wrought steels for the automobile and aircraft industries. Part of the work of that Committee is recorded in Report No. 75, issued by the British Engineering Standards Association. The prosecution of researches into steels and their heat-treatment was continued, but seriously delayed by the war, but the final Report was published in October of last year, and is a most valuable and classical work on the investigation into the physical properties of wrought steels of standard compositions under varying heat-treatment. The author, therefore, does not propose to deal further in this paper with the subject of alloy steels, except to suggest that investigations be

made into the properties of permanent magnet steels, such as are required for the manufacture of magnetos, and an inquiry into the production of small steel castings. He would like, at this stage, to record the view that the thanks of the whole automobile industry are due to the Steel Research Committee, and to its Chairman, Mr. A. A. Remington, for the great services they have rendered by their work in producing this valuable Report.

Before proceeding with his paper, the author would like to emphasise the great need of research into the numerous problems affecting automobile design and construction, and the enormous advantages which can accrue to the industry by collective efforts. Many motor manufacturers within recent years have established experimental research departments, and are now provided with well-equipped laboratories in which a great amount of extremely useful work can be carried out. It would be unreasonable, however, to expect any firm to give to its competitors the results obtained, at great expense, in their own laboratory, and the firm would naturally want to keep the results secret in order that they may obtain the particular advantage due to their enterprise. From the national point of view, however, we must look further than this, as the prosperity of one individual firm is of small moment compared with the prosperity of a whole industry, such as that which we represent.

The problem of automobile research can only be dealt with effectively by the close co-operation of all motor manufacturers, and it was with this object in view that the Research Association of the Association of British Motor Manufacturers was formed. This Association has the support and financial assistance of the Department of Industrial Research to the extent that the Government is prepared to put up £1 for each £1 subscribed by the members of the Association for carrying out any programme decided upon by the Research Council. The Government, in fact, say, "We will help you if you will help yourselves." Apparently the British motor manufacturers, as a body, have not given that support to the scheme which its importance deserves. No doubt those firms which have so far stood aloof have done so for reasons which they themselves consider sufficient, but the author cannot help but think that they are holding back because the advantages of collective research have not been emphasised so strongly as they might be. It is hoped that the discussion on this paper will remove any doubts which may exist on that point, and that

members will have valuable suggestions to make as to the best means of carrying any approved programme into effect. The primary object of the present paper is to set up a series of pegs upon which to hang the practical suggestions which, it is hoped, will be forthcoming during the discussion.

The author desires to direct the attention of the investigators of the Automobile Research Association to the vast amount of scientific work which has been carried out during the past six years by, or at the instigation of, the Air Ministry, and suggests that the results of that work should be sought out, carefully analysed, interpreted, and recorded for future use, before any further investigations on the same subjects are undertaken. He would also counsel them to keep in mind, during all their researches, the advisability of the ultimate standardisation, both of processes and details of construction, and that a close liaison be maintained between the Research Association and the British Engineering Standards Association.

The author proposes in this paper firstly to outline some of the subjects on which research work appears to be desirable, and, secondly, to indicate in a more or less general way how and where such researches might be conducted. He hopes this paper will form a useful basis for a discussion of the whole question of automobile research.

1.—Low-Grade Fuel for Internal Combustion Engines.

In setting out the proposed programme, an attempt has been made to follow some sort of sequence, commencing with the source of power employed in automobiles, namely, the fuel. Most of the problems affecting internal combustion engines will doubtless be dealt with by independent research associations, such as the British Research Association of Liquid Fuels for Oil Engines, but there are many problems concerning the use of possible fuels for automobile engines which may best be dealt with by automobile engineers, partly because their requirements are so different from those of designers of stationary and marine engines, that it is proposed to include their consideration in this paper. The fuel situation has become so all-important a factor in the life or death of the automobile industry that no time should be lost in investigating the possible limitations of low-grade fuels, such as crude mineral oil, tar oil, creosote, and the various distillations of coal, shale, peat, and allied carbonaceous deposits.

Research in this direction should not take the form of a series of experiments with one or more proprietary makes of carburettors, but rather should have in view the discovery of more effective means of producing a perfectly intimate mixture of air and fuel, and its ignition within the combustion chamber of an engine or turbine. Such investigations may lead to the discovery of means other than the electric spark, of starting the process of combustion. That there is room for such research is shown by the fact that it does not appear possible to produce a satisfactory mixture of a heavy oil with air by the aid of any ordinary form of carburettor, neither has it been found possible to effect complete combustion by means of an electric spark, unless the fuel has been vaporised by heat to a considerable extent, which process introduces the objection of expanding the gas before it enters the combustion chamber, thus reducing the volumetric efficiency of the engine.

These difficulties have, to a limited extent, been overcome in the Diesel engine by compressing air to a pressure above the spontaneous ignition temperature of the fuel, and then introducing the fuel, together with a smaller quantity of air at a still higher pressure, when combustion occurs spontaneously. It is not suggested here that the air injection or solid injection types of Diesel engines are the ones to be aimed at by the automobile engineer, and, indeed, these engines, introducing, as they do, such objectionable features as compressors and pressure storage accumulators, are by no means suitable for automobile purposes, although perhaps justified for stationary or marine purposes, where high speed and light weight are not essential.

Such research work as may be undertaken with low-grade fuels should be directed towards ascertaining the simplest, lightest, and most efficient means of producing a perfect mixture of air and fuel, and its ignition under various degrees of compression. Experiments should be made to determine:—

(a) The shape and proportion of fuel orifices.

(b) The effect of varying pressures on the inlet and discharge sides of such orifices.

(c) The effect of varying degrees of humidity in the atmospheric conditions.

(d) The effect of viscosity of the fuel.

(e) The effect of the temperatures of both fuel and air on the rate of discharge and the ultimate mixture and combustion.

(f) The effect of varying degrees of compression of the mixture.
(g) The best means of igniting charges of various fuels at different degrees of compression.
(h) The effect of feeding the fuel by force pump, or air pressure, through a spraying device.
(i) The distribution of the gas in a multi-cylinder engine.
(k) The proportions and shape of the combustion chamber and inlet and exhaust passages to produce the maximum mean effective pressure and economy.

It may be argued that the last-named item constitutes an entirely separate field of inquiry, but the author is of opinion that it has such a powerful influence on the thermal efficiency of an engine that it should be considered concurrently with the fuel investigations. The experiments on a small engine, described in the "Automobile Engineer" for August, 1919, contain evidence of the importance of the shape of the combustion chamber when using petrol fuel, and it is only reasonable to suppose that the effect may be even more marked when heavier fuels are employed.

It is always difficult, in investigations such as those advocated, to interpret the commercial value of any conclusions which may be reached. This is particularly so with regard to fuels. For instance, there is difficulty in estimating the relative advantages of paraffin and petrol for agricultural tractor engines, because, whilst paraffin may always remain cheaper than petrol per gallon, in use it is generally found to give from 20 to 25 per cent less output, and there is the further disadvantage that, with an engine of normal design, on account of leakage of fuel past the piston rings, the lubricating oil in the crankcase sump becomes contaminated with paraffin, with consequent danger to the big-end bearings. Commercially, therefore, it is not yet proved that any advantage is gained by the use of the cheaper fuel, and as this question is one of serious moment in the development of power farming, it is one to which research should be directed.

2.—Petrol and Mixtures thereof.

According to figures issued by the Society of Motor Manufacturers and Traders, the amount of petrol produced in the United States in 1918 was just over 3,500,000,000 gallons. During 1919 it was still well under 4,000,000,000 gallons, the increase being, approximately, 12½ per cent. This increase is by no means proportionate to the increase in the number of motor vehicles, excluding

motor cycles, as 6,146,617 were in existence in the United States in 1918 against 7,558,848 in 1919, showing an increase of 23 per cent. It is perfectly clear, therefore, that in the United States at least, the number of cars has increased out of all proportion to the petrol production. It seems, indeed, that not only will the United States be unable to export petrol, except at enhanced prices, but the advisability of prohibiting such export is actually now being considered. It would appear from the evidence at present before us that the only satisfactory solution of the fuel problem, so far as light-weight engines are concerned, is the use of alcohol or mixtures of alcohol with benzole, ether, and other fuels. In the light of the foregoing, and in view of the experiments which have been carried out in the Shoreham Laboratory for the Shell Co., it may not be considered necessary to make further researches with petrol fuel. The "Shell" series of researches have been very exhaustive, and some of the results were published in the March issue of the "Automobile Engineer."

3.—Alcohol, Ether, Benzole and Mixtures thereof.

The whole question of the use of alcohol-benzol and other mixtures is now being dealt with by a sub-committee of the Empire Motor Fuels Committee. On behalf of that body, a programme of researches on alcohol and alcohol mixtures was drawn up by Dr. Ormandy, Mr. Ricardo and the author, and Mr. Ricardo is now carrying out the work in a special engine. It is hoped that the results of this research work will definitely establish the economic value of such fuels, and form a valuable guide to engine designers.

Up to the present all engine research work has been, more or less, privately conducted by interested parties, and the only notable large-scale experiments with alcohol so far published in this country are those carried out by the London General Omnibus Company, who, after making a series of tests of alcohol-benzol and other mixtures, permitted the results to be published in a paper read by Mr. G. J. Shave.* These experiments, although made with an engine of standard design, in which the compression was varied by attaching aluminium plates to the piston crown, and by other temporary expedients, were most encouraging, and it is intended, during the tests now in progress, to repeat these experiments under conditions which will give accurate and con-

* See Proceedings, Imperial Motor Transport Conference, 1920.

clusive results. The records of these, and all other experiments made on behalf of the Empire Motor Fuels Committee, will be published as early as possible, and the author suggests that the Automobile Research Association can render valuable aid to this work by collaboration and financial assistance.

The completion of these fuel tests, it is believed, will establish definite data governing the design and operation of internal combustion engines running on alcohol, alcohol-benzol, and similar mixtures containing varying proportions of certain kinds of denaturants. The objects of these tests are: (a) to ascertain which of these mixtures may be used with commercial economy in internal combustion engines of existing design, and (b) to establish data governing the design of internal combustion engines for the future.

The first series of tests is being carried through with commercially pure alcohol, that is to say, a fuel containing 95 per cent volume of alcohol and 5 per cent volume of water, this being the purest condition in which alcohol can be produced under commercial conditions. On completion of the tests with pure alcohol, similar tests will be made with varying mixtures of alcohol, benzole and ether, and the earlier of these tests will be repetitions of those carried out by the London General Omnibus Company.

The programme of tests is practically the same for each particular fuel, or each mixture of fuels, and is as follows:—

Series I.—Tests for power output and consumption over the complete available range of mixture strength with wide-open throttle at piston speeds of 800, 1200, 1600 and 2000 ft. per minute, and at compression ratios from 3·8 up to 7 : 1. During these tests constant heat will be maintained electrically to the carburettor.

Series II.—Throttle tests for power and efficiency over the complete range of mixture strength at 0·8, 0·6 and 0·4 of full load, and at piston speeds of 1200 and 2000 ft. per minute, at varying compression ratios, and with constant heat to the carburettor.

Series III.—Tests over the complete range of mixture strength at a piston speed of 2000 ft. per minute, with compression ratio 5 : 1, constant heat to the carburettor, and with water jacket outlet temperatures at 30° C. and 90° C.

Series IV.—Tests over the complete range of mixture strength with a compression ratio of 5 : 1, a piston speed of 2000

ft. per minute, and the heat input to the carburettor nil, and at 2000 watts.

It may not be found absolutely necessary to take the full range of tests at varying compression ratios, and as a matter of fact the tests will only be made with two ratios to commence with, namely, 3·8 and 7 : 1. From the results thereby obtained, calculations will be made for an assumed case with a compression ratio of 5 : 1, and a check test with a compression ratio of 5 : 1 will then be made to ascertain how closely the practical result approaches the calculated result. If further tests are required, they will be made. In connection with Series II., it is also the intention of the Committeee, if possible, to make a comparative series of tests with petrol fuel.

4.—Solid Fuels.

The study of fuels should not be limited to liquids, but all fuels, both for internal and external combustion, should be investigated. From time to time solid fuels have been proposed as alternatives to be used in internal combustion engines. For instance, naphthalene has been suggested as a solid substitute for petrol, but the fallacy of this line of thought has now been definitely proved by Mr. L. S. Palmer,* who has shown that the advantage is only proportional to the increased heat value of the percentage of solid fuel added. This is very slight, and is only obtained at the expense of more serious disadvantages and troubles.

The commercial possibilities of gas producers for heavier forms of internal combustion engines should not be overlooked, and the systems of Col. D. J. Smith and others should be investigated scientifically, and the results obtained thereby in comparison with those from various solid fuels should be tabulated and recorded. Among these fuels a strict investigation should be made into the claims of charcoal produced from vegetable waste and brushwood, as advocated by Mr. John Wells,† whose work in this direction should be carefully considered, and, if found desirable, further experiments should be conducted, as there can be no doubt that in certain countries, notably Egypt, Uganda, and similar places where there is an abundance of raw material for the production of charcoal, this fuel opens up possibilities of transport hitherto unconsidered.

* See p. 195, *ante*.

† See Proceedings, Imperial Motor Transport Conference, 1920.

In all the foregoing experiments the problem of what constitutes an effective means of starting combustion, whether electrically or otherwise, should be concurrently investigated. If electrical ignition should be proved most effective, the investigation should include a determination as to whether a small light magneto could be made as effective as a large one, or, alternatively, whether either of these is as good as a battery and coil system of ignition when correctly synchronised. Coupled with these would be an investigation into the most desirable and effective ignition plug to meet various conditions. In this connection collaboration with the British Electrical and Allied Industries Association will be both necessary and advisable.

With regard to fuels for external combustion, a thorough investigation is long overdue. Steam propulsion on our roads has not received the encouragement which it deserves, and the author feels that had one-twentieth of the sum already expended on the development of petrol vehicles been devoted to the development of steam propelled machines, the steamer, popular as it now is in many branches of transport, would occupy a much more important position. To such persistent experimenters as Mr. T. Clarkson, Mr. Foden, and others, great credit is due for upholding the claims of steam transport, claims, by the way, which are not generally treated by automobile engineers with the seriousness they deserve.

5.—Pistons, Piston Rings and Cylinders.

The author has already referred to the danger of leakage of paraffin past the piston rings, and its effect upon the lubrication of big-end bearings. Another aspect of the piston question is the friction arising from the forces transmitted by the connecting rod and by the pressure of the rings to the cylinder walls. It is the author's experience that most of the usual piston troubles disappear when the design of the engine includes a cross-head, but, unfortunately, this type of piston introduces troubles of its own. The investigations should be directed to the discovery of a construction of piston which, among other virtues, includes the following points:—

(a) Absence of leakage of gas, liquid fuels, or lubricating oil.
(b) Reduction of wall friction.
(c) Sturdiness of construction so that it may be handled without damage to the rings.
(d) Freedom from distortion.

The examination of pistons should include aluminium alloy pistons, and the various types made of such alloys which have been so prominently before the automobile world during the past six years. The author's experience of certain types has been extremely satisfactory, whilst with others he has had disappointing results. On the whole, however, he is convinced that the field is worthy of further exploration, particularly with regard to:—

(e) An examination into the most suitable alloys, and the advantages attending the use of aluminium alloy pistons.

(f) Ascertaining data relating to design, such as clearance, etc.

This subject has been dealt with in numerous articles which have appeared in the "Automobile Engineer," chiefly in connection with aero engines, and no doubt much valuable information exists on this subject in the archives of the Air Board, which is the only authoritative body which so far has issued specifications for aluminium alloy pistons. If the foregoing points are not included in the programmes of the British Non-Ferrous Metals Research Association and the British Cast Iron Research Association, those bodies should be approached with a view to their inclusion, or their co-operation in collective research.

Concurrently, investigations should also be made into the use of aluminium alloy cylinders, their design, specification of material, heat-treatment, and probable advantages and life.

With regard to engine cylinders, if any judgment can be formed from the diversity of dimensions of bore and stroke, there is a lamentable lack of reliable information on the subject, and a thorough examination of the factors governing the design of cylinder castings is urgently needed. Heavy cast-iron cylinders have often failed through inherent weakness, and, on the other hand, we find extremely light cast-iron cylinders which, under the most severe conditions of service, have shown no signs of weakness. Variations in foundry practice may account for many of these discrepancies, but the blame, it is feared, rests at the door of the designer. We must know more about this subject if we are to make satisfactory progress in the development of internal combustion engines.

6.—Valves, Valve Materials and Valve Mechanism.

Valve failures and valve steels have been well covered by researches undertaken by the Air Board in relation to aero engines. These investigations were referred to at length by Dr. Aitchison

in a paper* which is one of the most valuable recent contributions to our store of knowledge. An extremely useful supplement to this work should be the specification of aero valve steels which is due for publication by the B.E.S.A.

The valve problem is not one which the metallurgist alone can settle, but the designer must also take a hand. As Mr. Remington truly said in the course of the discussion on Dr. Aitchison's paper, nearly all valve troubles arise from the fact that the valves get hot. This condition can be minimized to a great extent by giving the valves a sufficient mass of material and contact surface, which will absorb and transmit the heat to the water cooled walls. Such a remedy, however, introduces equally serious troubles due to weight and momentum. Dr. Aitchison's researches, whilst extremely valuable, all deal with aero engines, in which the conditions of service are entirely different from those prevailing in automobile engines, and steps should be taken to ascertain whether his conclusions can be applied to valves in general.

In contrast to aero engines, we might consider the conditions in engines fitted to Tanks. In these it was found that valves made of a case-hardened 3 per cent nickel steel gave by far the most satisfactory results, and at a cost of less than one-third that of valves made of tungsten steel, which is the material favoured by Dr. Aitchison. Under automobile conditions tungsten may possibly, from the design and metallurgical standpoints, prove to be the best, but if the life of such a valve is not at least $2\frac{1}{2}$ times that of a case-hardened nickel steel valve, or about four times that of a plain carbon steel valve, the author submits that it is commercially inadmissible. Cost of maintenance is the true test of commercial efficiency, and in all scientific tests this truth should be kept in mind, or the value of the results to the industry will be seriously discounted.

The conditions of use, and the desired characteristics are widely different in the head and the stem, a fact which suggests welding, or otherwise fixing, a head of one metal to a stem of another, as for instance, a cast-iron head on a case-hardened steel stem. The idea is not new, and a large number of transport wagons fitted with such valves gave satisfactory results in service during the war. These researches should also include an examination into the claims of Monel metal, which is an alloy containing, the author

* See Proc. I.A.E., Vol. XIV., p. 31.

believes, over 60 per cent of nickel. Other materials may be suggested, and all which give promise of being desirable should be examined. Among other factors, inquiry should be made into the following points, as submitted in Dr. Aitchison's paper:—

(a) The strength of the material at working temperatures.
(b) The resistance of the material to corrosion.
(c) The resistance of the material to distortion.
(d) Freedom from scaling.
(e) Freedom from surface cracks.
(f) Ability to take a high polish on the stem, and resistance to wear in the guides.
(g) Suitability of the material of the stem for hardening.
(h) Ease of forging.
(i) Ease of machining.

All the foregoing properties would be present in the ideal valve material, but it is doubtful whether such a material exists. For all materials which the investigations prove to be suitable, the correct heat-treatment and complete physical and chemical specifications should be given.

The design of the valve itself should also be considered, together with that of the means for locating the spring collar. There is a great diversity of opinion with regard to the last-named item, and reliable data should be established to show the relative merits of attaching the collar by:—

(k) Screw threads.
(l) Split cone wedges.
(m) Cotters.

7.—Cooling Systems.

Most radiators are unnecessarily large, and consequently their first cost and weight are unduly high, and the user has to pay for carrying the extra weight all the time. A thorough investigation of all the factors might establish data which would enable the size, weight and cost per horse-power to be considerably reduced. It would first be necessary to ascertain the effect and limitations in regard to temperature differences with variations in engine speeds. With the kinds of fuel now in use, engines run best when the jacket temperature is fairly high. It would be desirable, in the course of this research, to consider the practical value of controlling the temperature of the water as is done on some American agricultural tractors by means of a simple thermostatic

valve, through which, when the temperature falls to a pre-determined point, the radiator is short-circuited, or by regulating the amount of air passing through the radiator by a venetian blind type of shutter.

This investigation might also include a determination of the limiting factors governing the adoption of the thermo-syphon, or natural, circulation, as compared with pump circulation. The water pump should also be considered, particularly as to:—

(a) The most desirable type and capacity per horse-power developed by an engine.
(b) The determination of the efficiency of various types.
(c) The maximum and minimum running speeds.
(d) The most suitable materials for spindles, vanes, etc.
(e) The best means of lubrication.
(f) The most suitable gland-packing material.

The water jackets of the cylinders themselves should also be taken into consideration, particularly with a view to deciding upon the best positions for the inlet and outlet, and also whether, in the case of separate cylinder heads, it is desirable to have a pipe connection between the head and the cylinder jacket, or if the perforated gasket can be relied upon to make a joint both gas-tight and water-tight.

In connection with the question of the air cooling of cylinders, the paper by Mr. Lanchester, relating to both air and water cooling,* and that by Dr. A. H. Gibson,† on air cooling, should be studied by the investigators, and any further tests necessary should be undertaken, although the author doubts whether air cooling can be usefully applied on automobiles, except in the smaller sizes. The limitations, whatever they are, should be determined.

8.—Silencers.

The design of silencers does not appear to have received the slightest scientific consideration. Designers need some definite guidance as to the actual conditions which have to be met, and this will necessitate an examination into the pulsating effect of the exhaust discharge at various engine speeds, and the desirable length of exhaust pipe and silencer to give the best results. The

* See Proc. I. A. E., Vol. X., p. 59.
† See Proc. I. A. E., Vol. XIV., p. 243.

author believes that length is of far more importance than most of the other factors, and he has often found that by shortening or lengthening the combined overall length of the exhaust pipe, silencer and discharge nozzle, the exhaust discharge can be more effectively silenced without any increase in back pressure. Some guidance is also required as to the strength necessary in the silencer for engines of various capacities and speeds.

9.—Lubrication and the Need thereof.

Next in sequence we may take lubricants and lubrication, not only of the engine, but of all working parts of the automobile. Investigations should be made upon various lubricating oils and greases with a view to:—

(a) The provision of specifications of desirable oils and greases, and tests for various purposes.

(b) Making comparative tests of their freedom from carbonisation, oxidation, and gumming.

(c) Ascertaining the economic values of recovered and cleansed oil for further use.

The papers read by Mr. R. K. Morcom,* Dr. W. R. Ormandy and Mr. J. H. Lester,† and Captain G. W. A. Brown,‡ should be studied by the investigators, since, although those papers contain much valuable information, the subject is by no means exhausted. Experiments should be particularly directed, so far as engine lubrication is concerned, not only to finding out the best system or systems of application, but to ascertaining how far oil is used directly for lubrication purposes, and to what extent it is consumed as a supplement to the normal fuel used. There is much to be said for the method of precision feed of oil as used by Dr. Blackwood Murray on the Albion engines, and a definite pronouncement might be given as to its value relative to other systems for the guidance of engine designers.

With regard to item "c," the author has heard a well-known chemist declare that oil which has been used in an engine is not necessarily waste, but that if it is suitably cleansed it is actually better as a lubricant for engines than when used fresh.

At the same time, opportunity should be taken to review the

* See Proc. I. A. E., Vol. IV., p. 351.

† See Proc. I. A. E., Vol. VIII., p. 371.

‡ See Proc. I. A. E., Vol. XIII., p. 299.

present state of our knowledge concerning the coefficients of friction of all known bearing metals, together with an investigation of the relative values of different grades, and their behaviour when used in conjunction with various alloy and straight carbon steels in both the hardened and the normalised condition. With some alloy steels, great difficulty is sometimes experienced in finding a bearing metal which will give consistently good results, and a careful study of this subject, taken in conjunction with the composition and treatment of the bearing metals, as dealt with in Section 11 of this paper, will be appreciated by every engine builder. The use of "Oilless" bearings should also be considered, and an authoritative opinion given of their practical value.

10.—Shafts.

The state of our knowledge of the distribution of stresses in shafts of irregular shape and section leaves much to be desired. Professor E. G. Coker's paper on "Photo-Elasticity,"* is an interesting study, and might usefully be supplemented by further work on the same, or comparable, lines. Mr. C. E. Larrard's experiments† with soft castellated shafts might form a starting point for further experiments with heat-treated shafts.

It is common practice to use expensive alloy steels for shafts, particularly crankshafts, in order to get the necessary resistance to fatigue, but as there is evidence that many of our troubles in this direction are, to a large extent, due to vibration and whipping, it is probable that, by making the shafts of carbon steel and of more generous proportions, more satisfactory results may be obtained at a lower cost. Resistance to wear is one of the essential factors, and this certainly can be obtained with a heat-treated carbon steel.

The investigations under this heading should include:—

(a) Crankshafts.
(b) Hollow shafts.
(c) Castellated shafts.

In connection with the torsional problem, it might be of interest to study the results of experiments made by Messrs. Griffiths and Taylor, who read a paper on the "Use of Soap Films on Solving Torsion Problems,"‡ in which they describe a method of investigating irregular sections, carried out at the Royal Aircraft Factory.

* See Proc. I. A. E., Vol. XII., p. 21.
† See Proc. I. A. E., Vol. V., p. 133.
‡ See Proc. I. Mech. E., December, 1917.

11.—Bearing Metals and Journals.

Reference has already been made to the need of a wider knowledge of the anti-friction properties of bearing metals, and this should also include an enquiry into the causes of wear in bearings, particularly in connection with:—

(a) The most suitable steels for crankshafts from the journal surface point of view.

(b) The most desirable hardness of crankshafts.

(c) The preparation of specifications for white metals and similar alloys for various purposes.

We require more definite information concerning the relative anti-friction value of oil hardened, as compared with case-hardened, journals when running in bearings of various alloys, and the determination of the most desirable properties for bearing metals. For instance, it is said that remarkable results are being obtained with a bearing metal containing about 95 per cent of lead in combination with 5 per cent of other minerals; the material somewhat resembles a white hard brass, takes a fine polish, and is stated to have remarkably long life.

Amongst other characteristics all bearing metals should:—

(d) Be free from any tendency to spew.

(e) Be perfectly homogeneous.

(f) Have a low coefficient of friction.

(g) Possess good wearing qualities, and not be liable to abrasion or to abrade the journal.

(h) Have the property of ready adhesion to the housing or shell.

(i) Readily conduct and transmit any heat generated.

(k) Be relatively inexpensive.

(l) Not be unduly sensitive to reasonable variations in the temperature at which they are cast or run.

The last-named property is most desirable from the maintenance point of view, especially as repairs are usually carried out by comparatively unskilled men, in garages lacking the necessary equipment for ensuring exact temperatures. For these reasons it would appear that the most desirable bearing metal would be one which can be produced cheaply, and preferably be die-cast. A bearing might then be supplied cheaply by the maker, and replaced as a whole, rather than have a new lining run into it in a poorly equipped repair shop. No doubt the proceedings

of the Institute of Metals could furnish much valuable data on this subject, and the co-operation of the Non-Ferrous Metals Research Association be secured.

12.—Clutches and Clutch Couplings.

Clutch troubles are now by no means as frequent as they were some ten years ago, but there still remains the need for careful enquiry into both the question of design and the co-efficient of friction of the various materials employed as linings. Designers are now showing a preference for the single plate type, and in the author's opinion there is no other type which can compare favourably with it in point of smoothness of action, ease of manipulation and space occupied. The troubles which are experienced with this type arise chiefly through lack of ventilation leading to overheating, and consequent buckling of the driven disk. An examination needs to be made into the actual cause of this buckling with a view to the discovery of means to prevent it. Slitting the plates will often eliminate the trouble, but it would be far better if we could eliminate the cause.

The coefficient of friction of the various fabric linings on the market should also be investigated, as, although the makers put forward values for them, they cannot be relied upon to give the advertised results under ordinary working conditions. The makers' figures, it is feared, are the values for ideal conditions, but such conditions only exist temporarily, if at all, on automobiles. What is wanted is a series of coefficients which can be relied upon to give satisfactory results under ordinary conditions.

The clutch spigot bearing, and the clutch actuating mechanism, frequently give trouble on account of ineffective lubrication. Many of the parts which have only occasional relative movement are difficult to lubricate, and, consequently, are generally neglected. In this connection the properties of "self-lubricating" bearings, such as those made of compressed graphite or grooved bronze filled with graphite, should be especially examined. Many other clutch points will suggest themselves to the investigators, such as the most reliable and effective couplings between the clutch and the gear-box, in which the desirable points are:—

(a) Simplicity of design.
(b) Ease of assembling and dismantling.
(c) Good wearing qualities.

(d) Quietness of operation.

(e) Ease of lubrication or elimination of the need thereof.

The last-named item suggests an enquiry into the behaviour of flexible couplings, such as fabric, spring steel strips and disks, etc. Such couplings have given excellent service under severe conditions, and it is difficult to understand why some automobile builders continue to fit elaborate couplings which invariably become rattle traps after a few months of running.

13.—Gears.

Probably no part of an automobile gives rise to so much anxiety as the gears, no matter whether they be timing gears, transmission gears, change-speed gears or back-axle gears. Unless the greatest possible care is taken in their manufacture and location they are noisy and chattery. Noise cannot be produced without an expenditure of energy, and it is obvious that there is a very wide field open for the Automobile Research Association in the preparation of data and information which will be of real value to the automobile builder. Why is it that in a low-priced American car the gears are invariably quieter than in a British-made car costing from two to four times as much? The silent running of American gearing cannot entirely be attributable to the use of stub tooth gears, and either floating bearings or flexible roller bearings for the layshaft and axle shaft, both of which types of bearings have the effect of damping out sounds which, were ball bearings fitted, would develop into a distinct ring. During the past few years, the size of gear-boxes has been steadily reduced, and, the author thinks, with advantage. The shorter the shafts are made the less whip and vibration is set up in them, and the quieter the box will run, but how far we may actually go in the narrowing of gear teeth has not been decided. On the other hand, attempts should also be made to ascertain how wide the gear teeth may be made, and still have the full width effective when very heavy loads are to be taken. It is the author's experience that if gear teeth are made unduly wide the load is sometimes carried on one corner only of the teeth, due to the distortion of the gear either during the heat-treatment or while actually running. It would be useful to know the minimum and maximum limits of width to which gear teeth could be made and relied upon to carry a fair share of load uniformly distributed along the width of the tooth.

In general engineering the Lewis formula is the standard one, and gives every satisfaction, but it does not appear to fill the bill so far as automobile engineering is concerned. What is badly wanted by automobile designers is a set of data tables built up to meet the conditions actually met with in automobile practice. The Lewis "Strength Factor" is based upon all the load being taken on the point of one tooth, whereas in automobile work it has been found permissible, in fact even necessary, to assume that the load is applied to one tooth at the pitch line. In connection with the velocity factor, it is doubtful if automobile conditions are so severe as those taken by Lewis. Actual experiments should be made to determine these factors as applied to automobile conditions. At present, in calculating gears for automobile work the profile must be drawn individually to a large scale in order to determine the strength factor, and then we have to guess the velocity factor.

Investigations should also be made into the questions relating to friction of the wheel teeth and the proper distribution of losses between the gears and bearings. The losses in chain wheels, bevel wheels, spiral bevel and worm gear should also be included, although the tests which have been carried out at the National Physical Laboratory have probably gone as far as it is possible to go in the matter of worm drive. All the information which has been obtained at the National Physical Laboratory, however, should be embodied in the records of the Research Association.

14.—Wheels, Tyres and Roads.

The wheel and the road has been a popular title for papers for some years past, but unfortunately none of these papers seem to have led us into definite channels. Some years ago a Committee was appointed, and has been more or less working on the problem ever since, making investigations into the cause of wear of tyres and some means of increasing their life. The investigations have dealt with:—

(a) The design of vehicles;

(b) The design and construction of tyres.

To these two lines of enquiry should naturally be added a third, namely,

(c) The road surface.

Probably the work will now be carried to a satisfactory conclusion

by the Research Association of British Rubber and Tyre Manufacturers.

These problems ought to be taken concurrently with the investigation into vehicle springs, which is dealt with in a separate section, as the design and disposition of springs will naturally have some effect upon tyre wear. Enquiries under this heading may usefully include questions relating to the strength of commercial vehicle wheels of various types, with a view to the production of a lighter wheel without sacrificing anything in the way of strength or reliability; for instance, the claims of cast aluminium-alloy wheels should be investigated. The investigation might even go further and include all kinds of road vehicle wheels, especially the disk type which is now being very largely employed, with comparisons as to strength and weight for a given capacity.

The comparative merits of pneumatic tyres and solid tyres for heavy motor vehicles also need to be examined. Such tests could readily be made with an electrically-driven wagon, and the "input" of the motor recorded for the various experiments made. Comparative tests should be made with the same load and at the same speed over the same surface and under the same weather conditions. Concurrently comparative shocks suffered by the vehicle should be recorded under conditions:—

(a) When mounted on solid rubber tyres.

(b) When mounted on pneumatic tyres of the right size to replace the solids, and inflated as usually specified by the makers.

(c) The same as "b," except that the tyres should be inflated to lesser pressures than those specified by the makers.

Tyre makers have always been insistent in advising users to pump pneumatic tyres hard, and the author for one has been unable to see the value of this advice in all cases. The great value of pneumatic tyres is that they absorb blows in a horizontal direction, and thus supplement the springing which practically only caters for vertical movement; this suggests that the use of rubber tyres on heavy vehicles might possibly relieve the clutch and driving mechanism of much of the usual stress, because of the easier starting which the pneumatic tyres may be expected to allow. Tests, however, would quickly prove whether any gain is to be effected in this way.

15.—Spring Suspension.

Possibly the most important problem which automobile engineers have to face is that of the spring suspension of the vehicle. This problem needs to be solved from several points of view, and requires, among other things, a considerable amount of mathematical work. The investigation should be directed with a view to establishing:—

(a) The relative values of various conditions affecting the comfort of passengers.

(b) The effect of unsprung weight.

(c) The desirable relationship between sprung and unsprung weight.

(d) Means for reducing the spring periodicity, and for better absorbing the shock of road obstructions without increasing amplitude.

(e) The inter-relation between front and rear springs.

There is a wide difference in the methods of suspension on various cars, and the majority of automobile engineers appear to be very hazy as to the principles involved. Sometimes a very slight change in one of the factors will make an enormous difference to the comfort of the passengers. The principal factors affecting the riding properties of a motor vehicle appear to be spring deflection or amplitude, periodicity or number of vibrations per second, proportion of the sprung to the unsprung weight, wheel base, wheel track, the height of the centre of gravity of the vehicle above the road surface, and the distribution of load on the front and rear springs.

Before commencing their enquiries, the investigators of this subject should study all the available written matter concerning it, among which the author would mention a paper by Mr. G. H. Baillie.* Various articles have also appeared from time to time in the "Automobile Engineer," notably those in the issues for October and November, 1919. Also "A Theory of Plate Springs" and "Springs and Spring Suspension," which have been published in Vols. IV., V. and VI. of the Journal of the Society of Automotive Engineers of America. The work in this direction which has already been done in America should be most carefully studied, and the results tabulated and made available for English engineers. In the matter of spring suspension, possibly more

* See Proc. I. A. E., Vol. VII., p. 451.

than in any other, we have much to learn from American designers, as it is probably true that the percentage of spring failures on American built vehicles is considerably lower than that experienced on vehicles built in British factories.

With regard to springs themselves, taking the laminated semi-elliptic type as an example, every automobile engineer who had experience of vehicles in France can speak of innumerable cases of breakage, but unfortunately the breakages were not of such a consistent nature as to lead to any definite line of thought. In some cases it would be the top leaf which would go, and in other cases it would be the second, third, or even the shortest leaf. A study of the designs leaves us without any solution, and it seems highly probable that the difference in position of fracture may arise from the divergence in the practice of spring makers with regard to the variation in camber of the different plates, and the consequent nipping stresses. No study or consideration appears to have been given to these nipping stresses, and the author cannot find that they have been treated, either mathematically or otherwise, by any writer except Professor Perry in his "Applied Mechanics." A certain amount of nip appears to be both desirable and necessary, but if the nip between any two plates is excessive, very severe localised stresses are introduced in the plates. The desirable amount of nip to be allowed needs, therefore, to be ascertained.

It appears to be very desirable, however, that a careful consideration of the various methods of springing should be undertaken in addition to an investigation of the mathematical points affecting the design of the springs themselves. We need a suspension system which will not only take account of vertical displacement, but which will assume some of the duties of the pneumatic tyre and minimise longitudinal displacement. It is not so much the distance vertically or horizontally through which the body is moved as the rate at which it is moved which vitally affects the comfort of riding. Vibrations and shocks caused by road inequalities should be absorbed by the springs to such an extent, that if upward or horizontal motion must be given to the body, that motion should be given at the slowest possible speed.

With regard to unsprung weight, it is interesting to note that a series of experiments are being conducted by the Bureau of Public Roads (United States Department of Agriculture), at the Arlington experimental farm, near Washington, to determine the

effect of the impact of wheels on the road.* The results of tests so far made should be collected and, if necessary, further tests made on comparable lines.

The use of cantilever springs has developed to a considerable extent in recent years, and there can be no gainsaying the advantages which this type of spring offers for many purposes. It is a step forward in the evolution of the spring suspension question, but the author very much doubts whether it is the final solution.

16.—Brakes and Brake Mechanism.

The brakes fitted to motor vehicles have never been entirely satisfactory, and it is probably no exaggeration to say that, were the law enforced to the letter, very few vehicles would be permitted to use the roads. In the old days of slow speed motor vehicles the question of brakes was not of so much importance as it is now. The facilities for retardation of a motor car have not made equivalent progress with the development of means of acceleration, and the whole matter, from the design point of view, requires to be most carefully looked into. For instance, the relative values should be determined of:—

(a) Expanding shoes operating within a drum.
(b) A contracting band acting on the outside of the drum.
(c) Independent blocks or shoes acting on the outside of the drum.
(d) Hydraulic systems of braking.
(e) Pneumatic systems of braking.
(f) Braking on two wheels only on the same axle.
(g) Braking on two wheels but on different axles.
(h) Braking on all four wheels.
(i) Metal to metal, or metal to leather, fabric or other friction material.

The correct area of braking surface desirable for a given load for each of the systems named should be determined with relation to speeds at, and from, which retardation commences. The balancing of brakes should also be looked into, and the fullest possible information collected with regard to suitable designs of adjusting and actuating devices. The incorrect location of brake levers and rods is probably the most prolific cause of trouble with brake systems, because if the centre of location is not correctly

* See Proc. S. A. E., Vol. V., Part III., p. 210.

placed with regard to the movement of the axle each deflection of the spring produces a proportionate oscillating movement in the brake cams or other actuating mechanism, and this more or less continuous chatter leads to rapid wear of fulcrum and link pins.

17.—Steering Gear.

Steering gear failures are by no means so frequent as might be expected from an examination of the various arrangements fitted on some of the cars exhibited at Olympia and other recent Shows, but the probable cause of this immunity is sheer luck. It would be totally unsafe to rely upon such luck continuing.

Enquiries regarding steering gears should first be directed to the setting out of the conditions which have to be met. This will involve a certain amount of mathematical and draughtsman's work. If investigation should prove that no existing type is completely satisfactory, guidance should be given as to the lines upon which development could most usefully follow.

18.—Small Steel Castings.

The author feels that the more he has to do with British makers of steel castings the more doubtful he becomes of ever being able to rely upon home productions. This particularly applies to *small* castings. The alternative of using a stamping cannot always be adopted, sometimes because the quantities required do not justify the expense of cutting dies, and sometimes because the design does not permit of steel castings being used. Efforts should be made to determine suitable mixtures which will give uniform results as regards physical properties of small steel castings, and the steel founders should be asked to formulate their definite requirements as to the elements of design. The demand for small steel castings is becoming increasingly insistent, and the present unsatisfactory state of supply and quality cannot be allowed to remain. British founders can, and do sometimes, supply excellent castings, but the results sent out from some foundries are not always consistent, even from the same patterns. The reasons should be sought out in a spirit of co-operation.

19.—Transmission Efficiency.

The author has already made mention, under Section 13, of tests for ascertaining the power lost in the friction of gear teeth,

but this investigation requires to be extended throughout the whole of the chassis, including the friction absorbed by bearings, couplings and other links in the transmission mechanism. The overall transmission efficiency of complete vehicles, with all possible combinations of gearing, tyres and spring loading, should be tested out and the results tabulated for future information. Part of the investigation under this heading should include the making of fresh tests of the tractive resistance of various classes of road surface, so that this information, used in conjunction with that obtained from the data arrived at from the tests on transmission gearing, may be of assistance to designers in determining the engine power required to meet any condition of load, speed and service. Dr. Riedler has done quite a lot of useful work in this direction, but since the completion of his investigations there have been advances in gear production and design generally, and his work should be reviewed and, where necessary, brought up to date.

20.—Miscellaneous.

It would require no very great effort to enlarge the scope of this programme to any extent, but the author feels he has already outlined a programme wide enough for present consideration. He wishes, however, to call attention to the desirability of some standardised procedure with regard to makers' tests, so that declared horse-power, speeds, carrying capacity and other data which the purchaser requires should be given on a uniform basis. The need for this kind of thing is particularly marked with regard to agricultural motors.

Proposed Procedure.

Having now outlined a programme for research, a programme which is admittedly open to criticism on the dual score that it contains both too much and too little, the author will now attempt to outline a proposed scheme of procedure by which the whole programme, or such parts as may be approved, shall be carried into practical effect. In the first place, it might be advisable to split up any approved programme into divisions, each division to include a number of sub-sections which are allied in nature or in the desired methods of treatment. Each division might then be delegated to a sub-committee, the members of which should be the administrative representatives of the Automobile Research Association for all research work connected with that division.

It has often been said that the most effective sub-committee is one consisting of three members with two permanent absentees, and this assertion, the author fears, contains more than an element of truth in many cases. He would, however, like to see a number of small sub-committees appointed, each consisting of men who are not only keenly interested, but who are prepared to devote the necessary amount of time and energy to the achievement of the desired results. This is probably "the fly in the ointment," and the author fully expects to be asked to find the men. He recognises that it is always a matter of extreme difficulty to get together a body of men who really can spare the time necessary to do the job thoroughly; it must be clearly understood, however, that he would not expect the members of the suggested sub-committees to undertake investigations personally, but rather that their duties should be more in the nature of those of courts of enquiry, to consider and report to the Council of the Research Association upon all lines of research suggested by the technical officer, paid investigators, or others, and to act as courts of reference to which the actual investigators may look for guidance when serious difficulties arise. This procedure, the author believes, would save both time and expense in arriving at definite results.

At this late stage in the paper, any attempt at a detailed description of the methods and apparatus which it might be desirable to adopt for the purpose of the investigations outlined is out of the question. The author, therefore, proposes to confine his few further remarks to a brief indication of how and where some of the branches of research could be undertaken.

TABLE I.

1. Low-grade Fuels.
2. Petrol and Mixtures thereof.
3. Alcohol, Ether, Benzole and Mixtures thereof.
4. Solid Fuels.
5. Piston, Piston Rings and Cylinders.
6. Valves, Valve Materials and Valve Mechanism.
7. Cooling Systems.
8. Silencers.

With regard to the experiments set out in Table I., the bulk of the necessary apparatus and facilities might be available at the London College of Science and other Technical Colleges and

Universities, but whilst the author does not desire to minimise the value and importance of research work as carried out in our Universities and Colleges, he feels that in these Institutions the importance of commercial efficiency is not always kept in mind. He would, therefore, prefer to have the work done, as far as possible, in commercial laboratories, or under conditions which leave no need for the assumption of any of the important factors. In the first place, for the purpose of the experiments in Sections 1 to 6, it will be necessary to have access to an engine in which it will be possible to vary widely both compression ratio and piston speed within prescribed limits. So far as he is aware, only one such engine exists, and that is the property of the Asiatic Petroleum Company. The owners have kindly given permission for this engine to be used for carrying out the series of tests with alcohol and alcohol mixtures mentioned earlier in this paper; no doubt similar facilities could be given for any work desired to be undertaken by or on behalf of the Research Association.

TABLE II.

9. Lubrication and the need thereof.
10. Shafts.
11. Bearing Metals and Journals.
12. Clutches and Clutch Couplings.
13. Gears.
14. Wheels, Tyres and Roads.

With regard to the items in Table II., practically the whole of this work could be undertaken at the National Physical Laboratory; at any rate, it is known that all the necessary apparatus exists in that laboratory to enable Sections 9, 11, 13 and 14 to be undertaken without any serious delay. Whilst the ultimate establishment of a laboratory specially equipped for automobile research is desirable, it does not seem advisable to ignore existing facilities.

TABLE III.

15. Spring Suspension.
16. Brakes and Brake Mechanism.
17. Steering Gear.

The author is of opinion that the items in Table III. should preferably be taken together and carried out in one laboratory, and in view of the great importance and special nature of these researches, he considers it would be necessary to lay down special apparatus and put it under the charge of highly skilled investi-

gators. This apparatus might reasonably consist of an endless band travelling over a flat platform, and so arranged that the band could be driven by an electric motor, or other means, at any required speed. This platform and the travelling band should be mounted on a frame in such a manner that it may be tilted to represent uphill, downhill and side angle conditions. Any desired projection or projections could be attached to the band to represent road inequalities or obstructions. The rolling resistance on the band of various classes of tyres should first be ascertained and the value recorded, so as to afford the means of comparing the results of subsequent tests with various classes of road surfacing, for which similar rolling resistance would have been ascertained under Section 14. Resting on the travelling endless band would be a skeleton chassis embodying the apparatus to be tested, namely, springs, brakes, steering gear, etc., each of which could be tested separately or collectively, as desired. The chassis should be anchored on the frame by means of a recording draw-bar dynamometer, and be further provided with instruments recording periodicity and amplitude of the springs, sideways deflection of the steering wheels as they strike obstructions on the band, and apparatus for measuring brake movements caused by spring deflection. With this apparatus the retarding effect due to brake application within the limits of adhesion could be obtained for various classes of braking material, and different arrangements of brake operation and valuable comparative data could be obtained in this way.

Section 18 (Small Steel Castings) can be dealt with more by the co-operation of interested parties rather than by independent experiments, whilst, the author believes, the necessary apparatus to enable the making of tests under Section 19 (Transmission Efficiency) exists in the laboratory of Halley's Industrial Motors, Ltd., and possibly also in others.

In conclusion, the author wishes to express his appreciation of the invitation extended to him to prepare this paper, and to acknowledge the assistance he has received from Mr. Remington and other members. He regrets that the material offered for discussion is necessarily sketchy, but to have made it otherwise would have produced too bulky a paper for treatment in one evening. He hopes, however, that his efforts to present a broad basis for discussion have not been in vain, and that members will not hesitate to express their views on this important question of Automobile Research.

THE DISCUSSION.

Dr. Leslie Aitchison, in opening the discussion, said: The author has referred to the paper which I read before the Institution last year* on the subject of valves and valve materials, and I would like to try to impress upon you that the conclusions which I reached in that paper applied just as much to automobile work as to aeronautical work, and I submit that the list of properties which I considered that the ideal valve steel should possess and the causes from which valves do fail, apply just as completely to automobile engines as to aeroplane engines. I am glad to see that the author supports me in this and repeats the list of properties in full on page 522. I did not, however, in my paper press for the use of a tungsten steel, except for valves which are to work at a temperature of 800° C. and higher, and I submit again that if in any automobile engine the exhaust valves have to work at that temperature, tungsten steel will be found to be the most satisfactory. With regard to Monel metal, I rather suspect that the author has recommended this as a useful subject of research in consequence of several articles which have been published setting out good results obtained with this material in America. From some reports which I have obtained recently, however, I gather that the original reports did not represent all that happened, and that, when carried further, Monel metal did not stand up particularly well. That is what we might expect, because Monel metal is not a particularly strong metal in any case. On the other hand, Monel metal is by no means a cheap metal and, therefore, would immediately come under the suspicion of the author.

A second point to which the author has referred is the question of piston rings and piston ring material, and I think this subject should receive a considerable amount of attention in any scheme for research in connection with automobiles. In that connection I would mention one interesting fact which we found in connection with the standardisation of piston rings. A great deal of impor-

* See Proc. I.A.E., Vol. XIV., p. 31.

(Dr. Aitchison.)
tance has been attached by automobile engineers in the past to the use of rings which will give a very definite and quite narrow range of pressures on the cylinder walls. The requirements put forward by designers have been distinctly stringent, but we have found from careful tests that the ordinary tolerances within the limits of manufacture which were allowed on the majority of drawings gave a variation of some 300 per cent in the pressure.

I would suggest that the subject of the most satisfactory material for gears is a particularly important one. There is the controversy in regard to the use of air hardened steel or case-hardened steel for this purpose, but I think it is desirable to get down to the basis underlying that discussion and to find out whether we require a steel which is very hard on the surface, or a steel which is not very hard on the surface, or even a steel which is comparatively soft. I agree with the author that steel castings are most unsatisfactory at the present time. Yet he seems to wish to use steel castings to a greater extent than at present, but I am not at all sure that to do so would not be a distinctly retrograde step, if they are to be used for parts which are going to be of some importance and which are stressed in some way or another. It has been definitely proved recently that castings have a strength under fatigue of about one-half that which is possessed by the same material after it has been forged to a reasonable degree. I think it is time that the automobile industry initiated experiments to determine the effect of such things as keyways, and serrations, or screw threads, running circumferentially or longitudinally in a shaft, upon the resistance of that shaft under fatigue. The papers which have been referred to by the author all relate to static stresses, which in crank shafts, I should imagine, are of comparatively little importance, and what we want to know is whether the same degree of concentration of stresses, which is found for static loading, applies when the loading is dynamic, i.e., when the part is being stressed under fatigue.

Dr. W. R. Ormandy: The title of the paper is "A Suggested Programme for Automobile Research." What the author has actually given us is rather a descriptive and complete account of the major difficulties which are met with by the motor engineer in the manufacture of a motor car, and the amount of material put forward is so enormous that if the money available had been a hundred times as much only the beginning of the programme

could have been embarked upon. It seems to me that there is going to be very little done because too much is being attempted. Very good service would be done if the motor industry would let the steel makers know the ideal properties that they require in the material which they want and leave it to the research organisation of the steel makers to make these steels and present the results to the automobile engineer for him to say which is the best. We do not want to spend our particular money in that direction.

I am not entirely in agreement with the author's arrangement of material. An organised system of research should be laid out, not for this year, or next year, but with a view to its continuation over a considerable period of time. It should take cognisance of the possibility of a change in economic factors; it should be flexible, and I would suggest that on broad lines the first division might be (a) the power unit, (b) the chassis, and (c) the body. A research programme should be directed very largely by economic considerations, and to-day the most important economic consideration is to get the largest number of ton-miles per gallon of fuel, because fuel is the expensive item. That means that attention should be concentrated on the fuel portion of the scheme, i.e., on the power unit as a power generator, and on the chassis unit with a view to reducing friction losses and improving the transmission efficiency. Another point that strikes me is that there is not sufficient distinction made between research, invention, standardisation and ordinary testing for the formation of data sheets. It is stated in one place that certain troubles arise in certain types of clutches due to overheating and failure to carry the heat away properly. The trouble is described, the cause of it is given, and a method to cure it is given, and all that remains is a question of design. That is not a subject for research, particularly in view of the fact that the materials that are available for research are so enormous in number. On page 528 the author says that the effect of noise produced in the gear-box is evidence of loss of energy. Smoke abatement enthusiasts used to say in the north how many million tons of coal are wasted every twelve months judging from the smoke coming out of the chimneys. As a matter of fact, there is frequently more waste of fuel from a smokeless chimney than from a smoky one; and I hardly think that the amount of noise that comes from a gear-box is sufficient presumptive evidence of any considerable loss of energy. There is one sentence which inte-

(Dr. W. R. Ormandy.)
rested me very much on page 531: "The principal factors affecting the riding properties of a motor vehicle appear to be spring deflection, or amplitude, periodicity or number of vibrations per second, proportion of the sprung weight to the unsprung weight. wheel base, wheel track, the height of the centre of gravity of the vehicle above the road surface and the distribution of the load on the front and rear springs." These are about all the possible factors that there can be, and naturally they are the factors which do cause the end result. I mention this sentence because it seems to me that in spite of the very great value which undoubtedly exists in putting this information before us, it results in this, that the paper is less a suggested programme for research than a basis of material upon which a research programme must be founded. In one place it speaks of lubrication and the needs thereof. Yesterday I discussed with the chief chemist of one of the biggest oil companies the advisability of getting the oil companies to provide cash for the investigation of the fundamental principles of lubrication, which could not and would not be tackled by the individual manufacturer, and we came to the conclusion that unless we could get £25,000 a year it was not worth starting on. Yet that is given as one little item out of a number, and, therefore, it seems to me that the great difficulty of the research organisation is going to be to settle upon a programme of the problems upon which it shall spend its money. Research wrongly directed is a quagmire into which ideas, time, money and effort can be sunk, and it is essential that the programme should be laid out and thoroughly threshed out before it is started upon. I am in absolute agreement with the author as regards a system of having committees to deal in an advisory capacity with various aspects of the problem. I would recommend to those who have not read them already the articles dealing with the Ricardo research in the "Automobile Engineer," which give, in my opinion, details of the most perfect type of research work that has been done in the engineering world for many years, certainly that has ever been done on the internal combustion engine. The object of research done by an organisation ought to be to get at something which is fundamental. Ricardo started out with the desire to find out whether all fuels when burned with the theoretical amount of oxygen or air in the cylinder at a given compression, gave out the same amount of energy or whether they gave out different amounts of energy, and then, having found out that they all gave practically the same

amount of energy under a certain compression, he tried the effect of increasing the compression, and he found that so long as a fuel will stand a given compression it will give out as much energy as any other fuel, within one or two per cent at the outside. But the point at which detonation starts has to be thought of, and he tackled the problem of what the fundamental property of a fuel is which leads to detonation, and he has isolated and used various fuels and shown that within certain limitations paraffins detonate easily, naphthenes detonate less easily, and benzene and the aromatic bodies detonate with greater difficulty, and alcohols with the most difficulty. Consequently, the degrees of compression under which these fuels can be used vary. That leads to the possibility of taking certain fuels which cannot be used in high-compression engines, knowing exactly quantitatively how much of something else may be added to it to make a mixed fuel which will stand under conditions of high compression. That is fundamental research, because it has gone back to the chemical constitution of the fuel and the physical properties, and that is the sort of thing to be sought for in a research organisation of this type. On page 537 the author states that the work on the lubrication of shafts, etc. should be carried out at the National Physical Laboratory. I do not agree. It can only be carried out by co-operation with the aid of a perfectly fitted chemical laboratory, which must be working in conjunction with the great oil companies, because, unlike dealing with the fuels, petrol and benzenes, and toluenes, which are simple and can be isolated, with lubricating oils we are dealing with complex chemicals, things which cannot be made synthetically, and so all we can do is to find the basic line, that certain oils have given properties and that by certain methods of refining we can get certain things out of them. Any research work on lubricating oils must, in my opinion, be co-ordinated effort between those who do the mechanical testing and the chemist who does the chemical work, testing raw materials of known properties provided by the oil companies. I would like to ask, what is to be understood by a research on piston rings? I could understand what would be meant by an investigation of the material of which a piston ring is formed with a view to finding out the relationship, say, between its hardness and its life or between the clearances which were used and its life and its efficiency, but to simply stipulate a general line of research on piston rings is not sufficiently defining what is required. The paper is a very admirable statement of the diffi-

(Dr. W. R. Ormandy.)
culties and the ideal desires of the motor car manufacturer, but it is necessary to isolate from it so much of the fundamental information as is necessary to enable researches to be carried out with the very limited means at our disposal.

Mr. A. E. Parnacott: I would plead for the concentration of research in those fields which promise the greater gain. Why, for instance, try to get a greater efficiency out of the engine when commonly 40 to 60 per cent of the power is wasted between the engine and the tyre tread in practice? Far better, surely, for us to discuss matters of *urgent* moment, such as the reduction of weight and the suspension problem, which are far more promising fields for economies in road traffic. The greater proportion of driving to-day is done with partly closed throttle, and yet engine tests for efficiency are made with full open throttle, whereas an improved transmission alone can increase the miles per gallon far and away beyond that attainable by variation of compression pressure, carburation, vaporisers and even cycles of operation. In my opinion, the engine is good enough for years to come provided only that paraffins are not used and that the oil is not used over and over again. To supplement my contention that research is not for the present necessary for several of the objects the author mentions, I will make a few brief remarks thereon seriatim. Valve steels: R.A.F. and A.I.D. experience suffices. Valves that have stood up for ten or twelve years in high-speed engines are surely good enough, and I am inclined to think that a lot of difficulties occur through failure of some of the material. It is wiser, I think, to take material which has proved satisfactory and use it. Piston clearance: is not this a matter of cylinder distortion, curable by making them cylindrical in other respects than in the bore when cold? Iron and other castings: is it surprising that so many are scrap when the drawing office knows nothing practically of the subject, and when the pattern-maker and moulder are on fighting terms, and penuriousness and filth dominate the foundry? I am not surprised particularly when we know the power of molten metal to absorb gases resulting from the odd materials in both sand and loam. Cooling systems: there is ample knowledge and experience available for perfectly successful air cooled engines for cars, unless they are to run at very quick revolutions. The difficulty is not a matter of research but of public prejudice. Journal and bearing metals: there is enough published on this subject to enable satisfactory journals and bear-

ings to be designed. So far as the bearing metals are concerned, I would like the author to add to his list of requirements for bearing metals, non-porosity. The reason why most metals fail so lamentably is because the porosity of the metal lets down the film of oil on which the function of a bearing depends.

In my opinion it is at least a five years' job for the Research Association of British Motor Manufacturers to collect, collate and index the sources of information which would be a colossal and invaluable service to the British automobile industry.

Baron G. STEINHEIL: I do not agree with the author as to the unsuitability of the Diesel type engine for automobile work. It is true that certain experiments carried out about eight or nine years ago in Germany and Russia with high-speed small power air injection Diesel engines were not quite satisfactory owing to the difficulties with air compressors and pulverisers. Since then, however, the solid-injection principle, in conjunction with high compression, has been introduced, such engines requiring neither air compressors nor air starting bottles, as they can be started with an electric motor in the usual automobile way. I think that this type of engine has a great chance of being adopted for automobile work. The normal compression of a solid-injection submarine engine is practically the same as the maximum pressure in an automobile four-cycle engine (about 360 to 380 lb. per sq. in.). The maximum working pressure of the solid-injection engine is about 550 to 600 lb. per sq. in., but by making the reciprocating parts as well as the engine cylinders and crankcase of alloy steels and special materials the weight can be brought down to the limits used in automobile practice. As far as the piston speed is concerned, I understand that experiments have already been made with a standard Vickers submarine engine of one cylinder which was run up to 1,500 ft. per minute, and this figure is within the limits of automobile practice. The author does not mention the two-cycle engine which, in my opinion, possesses too many advantages to be ignored by automobile constructors. I am convinced that quite successful engines of this type could be produced if a certain amount of experimental work were done on them.

Major A. D. OWEN: The author calls attention to the matter of maintenance as follows: "Cost of maintenance is the true test of commercial efficiency, and in all scientific tests this truth should be kept in mind"—in other words, a commercial aspect is im-

(Major A. D. Owen.)

perative when compiling the research programme. No one can take exception to this, for it is indeed the crux of the matter.

Accepting the definition of "research" as "careful investigation," there are several matters on which all concerned in the compilation of a programme should be in agreement and which they should constantly keep in mind, such as the type of "research," the object that "research" is being made for, the period of time the "research" is to cover, whether a result is wanted soon, and the money available.

Until these points are decided, no practical programme of research can be compiled, or even suggested.

Mr. Letts, on the occasion of the last summer visit at Manchester, said: "We must all pull together," i.e., the purely commercial side and the engineering side of the industry. Again, at the Annual Dinner, Mr. Mayes-Smith (S.M.M.T.) said: "We want the help of the automobile engineer; there must be co-operation." When a cry goes out from one branch of the industry such as this, surely the direction in which the end lies is indicated. Is it not that increased sales are wanted? Automobile engineers cannot ignore the commercial, i.e., the sales side of the industry. Unless there is a market to take vehicles, few vehicles will be constructed; and unless an industry is financially successful it cannot find funds to pay for research.

But what have we been asked to make research into? Apparently, nothing has been definitely put, but is it not how to produce vehicles that are desirable in the opinion of the purchasers? It may be possible for the designer, by his products, to mould the opinion—this point is worth a certain amount of research. It is not, however, safe to assume, without investigation, that he can mould opinion to the extent of maintaining the necessary quantity of purchases.

The state of the market is not the concern of the sales department only. Indeed it is, first and last, an engineer's problem. If a certain market sends in a complaint or request, it is for the engineer to investigate the reason, incidence and genuineness of the complaint or request. If the market prefers something other than that which is being given, either as a whole or in part, the reason why should be ascertained, and what it is the purchasers in that market prefer. It may be that other makers' vehicles are more reliable; or that when repairs become necessary, better facilities for getting special spares exist; or that repairs as a whole

can be carried out quicker or cheaper. These are engineers' problems, and just as much matters of moment and matters for research, as for example, details of engine design—nay, at the present these commercial matters are of greater moment. The maintenance engineer should, therefore, have a big word to say as to the line of action to be taken in research; it should not be left wholly to the designer and the scientist.

General Bagnall-Wild: I propose to confine my remarks to two suggestions for the consideration of the author. The first one is with regard to pumps. I suggest to him that it is quite useless to carry out research on the capacity and reliability of the pump unless that research is coupled with the complete unit. The pump may function perfectly when tested as a pump, but when it is coupled up to the engine and to the radiator, conditions are materially altered.

The other point I wish to make is in connection with lubricating oil. The average car is subjected by its owner to periods of rest sometimes even as long as two or three months, and the oil is allowed to remain in the engine. There are certain oils, especially those which contain oil from a vegetable base, which are liable to develop as much as 4 per cent of acid after but two or three months in store. The effect of the acid is disastrous, especially to ball bearings. I consider that research into lubricating oil should include the question of storage, or rather, the period of rest. Dr. Aitchison suggested that research was desirable to ascertain the effect of sharp corners, machine marks, etc. on crank shafts. We have a superabundance of evidence that a fracture is caused by even light scratches. We have evidence that sharp corners at the bottom of the key-way cause fracture, and I suggest that research is unnecessary in this direction, although quite a separate problem presents itself with regard to the key-way.

Mr. A. E. Berriman: I should like to express agreement with what Dr. Ormandy has said with reference to this paper being a complete summary of the difficulties confronting the automobile engineer, and I also agree with him in his view that the solutions of many of them lie in the realms of design and experiment rather than in the field of research proper. It is this fact that increases the difficulties confronting the Automobile Research Association, and it is also in some degree the underlying cause of this paper, for the Council of the Research Association suggested to the

(Mr. A. E. Berriman.)
Council of this Institution that a paper on this subject might be of great assistance.

With these facts in mind I would venture to urge upon the present Committee the need of concentrating on a few of the more important problems that offer a reasonable chance of solution by research. Definite problems of this character have not been separated from the general review presented by the author, and it therefore remains for the members present to focus the more immediate urgent issues in a few concrete suggestions.

The question that should be asked about any proposed line of research is, "What exactly do we want to know on the subject?" The case of the Steel Research is an admirable illustration of the application of this general method of approach for research. The Steel Research confines itself strictly to ascertaining the numerical values of the maximum stress, yield stress, Brinell hardness, Izod impact, etc. This constitutes answers to definite questions which can be ascertained by known processes. It is not by any means necessary to employ a known process, but it is essential to be able to formulate a definite question before any active research could be made intelligently.

On one point in the paper I should like to make some remarks that may save misunderstanding. The author emphasises the desirability of making use of existing research establishments, such as those at the National Physical Laboratory and the various Universities throughout the country. This probably was accepted by the Council of the Automobile Research Association, but when it came to the direct issue of getting research carried out it was found that all these Institutions were already overwhelmed with work. In the circumstances, therefore, the Automobile Research Association has had to provide itself with a building that would serve the purpose of a laboratory when suitably equipped.

Mr. Max R. Lawrence: I think the matters that have been touched upon in the paper under discussion as being suitable for investigation could be grouped under three heads:—

1. Research to find out new and physical facts, such as re-establishing the value of coefficients of friction or the properties of new fuels or further properties of known fuels, etc.

2. Methods of application, such as the size of gear wheels, the use of particular materials for particular parts, piston clearances, etc.

3. Commercial tests, such as the life of particular types of sparking plugs or magnetos, etc.

No. 1 could probably be best dealt with by the Research Association, the second by organising such tests in manufacturers' laboratories, or generally persuading them to publish data which they had already got, and section 3 would be best left to the manufacturers of proprietary articles, getting them to conform to certain specified tests, and having an observer to see that they were carried out impartially.

In this way more work could be undertaken, and the cost of the experiments would be reduced to a minimum.

There is no doubt that if the Committee were to keep in touch with the interested manufacturers, they would, or might, be induced to undertake research in certain directions, and allow their results to be issued to the public.

Mr. O. D. North: I think one profitable subject of research would be a continuation of the work that Mr. Ricardo is so eminently fitted to undertake, and that is the investigation of the extent to which it is possible to improve the efficiency of the engine at small throttle openings. I disagree entirely with Mr. Parnacott when he says that the engine is perfect. The big economy in fuel consumption possible in some vehicles is often simply due to the fact that the gear-ratio is too high. It has nothing to do with losses in the transmission at all. Mr. Parnacott's statement that 60 per cent of the power is lost in transmission does not agree with my own experience. I would also like to emphasise the value of collecting information, more particularly from the American technical papers. This information should be collected and edited and put before the members of the industry in such a way that they will absorb it without feeling they are undertaking an arduous task. People ought to be encouraged to gain all the benefit they can from what has been done before by other people.

Mr. A. F. Evans: I am going to accept the suggestion made by Mr. Berriman to propose a definite line of research. We design the automobile engine for full load and then we run it at anything from 25 per cent to 30 per cent of the load, and when we are running with the throttle nearly closed we are working at a very low compression, under which conditions the engine is not so economical as it should be.

Baron Steinheil suggested that the two-stroke cycle might be

(Mr. A. F. Evans.)
of use. Many years back I produced a two-cycle engined car; it weighed 35 cwt. had the same horse-power as a Ford and it travelled at the same speed as a Ford, but the consumption of that car, over a 700 miles test in the winter, was 32 miles to the gallon.

Dr. W. R. Ormandy: Mr. Berriman, the only representative of the Research Association here, has asked for concrete proposals. I would like to propose, as a subject for research, an investigation into the action that takes place in the inlet manifold of four- or six-cylinder engines. Pinking occurs in one cylinder because that cylinder gets a detonating mixture, and at the other end the mixture may be very weak in one cylinder, which leads to misfiring. There is a good deal to be learnt yet as to what goes on in the intake manifold, although the American Bureau of Standards have done some really good work on the subject.

Mr. A. J. Rowledge: I have been trying to think of researches that could usefully be included in a simple programme. One such subject I have in mind is bearings. We used to use the factor $PV = \text{constant}$, but in the Michel bearing $P/V = \text{constant}$, and it is difficult to reconcile the two formulæ. Bearings at the present time, therefore, seem to me one of the most profitable fields for investigation. There may be something very useful to be investigated in connection with radiators, but I do not agree that usually radiators are too large to meet all the conditions of their service.

The President: We have had a very interesting discussion, although it has not been on the lines the author hoped. Nevertheless, I think he has given us a very great deal to think over, and it is very rarely that we get such a comprehensive view of a subject as he has given us, and it is for the industry to say definitely what it is we require to be done in this matter of research. That is the point that the author presses home on us in the paper, and it is the duty of each one of us to think the matter over and deal with it as fully as lies within our capacity.

Mr. Geo. Watson, in replying on the discussion, said: Owing to the very short time available, I will reply only briefly now and reply more fully in writing. In the first place, I should like to express my disappointment with the character of the discussion. Up to the present, only one constructive proposal has been made, and that is by Dr. Ormandy. All the others have been more or less suggestions for lines of research additional to a list which I

was charged with making too long to begin with. I was hoping that this discussion would take the line of suggesting what should be eliminated, which items in the suggested programme should be threshed out thoroughly at an early date, and which might be put off for some future time, and that we should be able to get down to a group of four, five or six headings out of the whole list, and say: "These are the most important ones," and that these are the items on which we should be justified in recommending that money should be spent.

I was glad to know that Dr. Aitchison agrees with me that tungsten steel is not absolutely necessary for automobile valves, though he advocated it so strongly in his paper. My remarks about Monel metal were written long before any of the writings to which Dr. Aitchison refers. The original copy of my paper was prepared nearly a year ago.

Dr. Ormandy says that routine work must be avoided; let us get at a few of the most important problems and direct the researches carefully, and it is for that reason that I advocate that there should be small sub-committees which would investigate the lines of research and lay down the detailed programme. My programme is a broad outline of the possible subjects for research. Whether they are all taken or not is a matter for the sub-committees to decide. Dr. Ormandy has outlined certain tests suggested by the oil firms, and that is just the sort of suggestion I wished to obtain.

Baron Steinheil spoke of the solid injection Diesel engine. I have not dealt with it because a separate Research Association is dealing with that particular subject, and the same thing applies to many of the other subjects. The question is, how far are the other Research Associations going into the matter, and I suggest that the Research Association of the Automobile Industry should make inquiries of the various other Associations and find out what subjects they are investigating, and, in that way, avoid any overlapping. At the same time, I ask the investigators, before doing any actual research work whatever, to find out what other work has been done, to tabulate and collate it and put it in such a form that it is available for use. Probably, if all the information which is available throughout the world were collected, we should find that many of the subjects do not need any further research, but we do not know where to look for the information, and the Research Association's task should be not only research into particular

(Mr. Geo. Watson.)

items of design, construction and material, but to find out all the information that exists.

General Bagnall-Wild spoke of the futility of testing the circulating pump without all the rest of the parts in series. I quite agree with him, and I certainly had such an idea in mind. I would ask him if he will use his influence to make available to the Research Association the vast amount of information which must be stored up in the cabinets of the Aeronautical Department.

Mr. Berriman quite rightly says that my paper is a summary of the subjects in which research is necessary, and we ought, as I have said before, to look into those items and agree on a limited number and go right into them, avoiding those which are already covered by other bodies. The same remark applies to Mr. North.

Mr. Rowledge said that no radiators are bigger than necessary. A little over a year ago I was in the very hilly parts of Italy, and I saw many cars there climbing far more severe and longer hills than we have in this country with very much smaller radiators than we fit for the same power of engine, and I never saw one of them boil.

COMMUNICATIONS.

Mr. JAMES WATT wrote: Dealing with gears in section 13 of the paper, the author suggests that, for the calculation of tooth strength, automobile engineers are badly in want of some formula other than the "Lewis." At the risk of appearing to labour the point, I desire to call attention to the curves I gave in the discussion on Mr. Worsley's paper, "Gear Cutting for Motor Cars."* The system or formula for which I coined the name "Autolew" is on the exact basis specified by the author, and I therefore suggest that, in order to prevent confusion, the title "Autolew" should be retained for any further formulæ or curves on the same basis.

Mr. D. P. MUIRHEAD wrote: With reference to the use of Monel metal for valves, the following may be of interest:—Monel metal is an alloy produced by direct smelting of copper nickel ore, mined in Sudbury, Canada. The alloy contains 67 per cent nickel, 28 per cent copper, 5 per cent iron and manganese. Table IV. gives the physical properties of this metal at high temperatures.

TABLE IV.

Monel Metal Tests at High Temperatures on $\frac{1}{2}$ in. dia. rod.

Temperature.	Yield Point.	Tensile Strength.	Elongation per cent.	Reduction of Area per cent.
Normal	21·3	41·4	51·	69·
320°C	17·0	38·5	48·	62·
410	15·6	33·8	49·5	63·
500	15·2	28·7	45·	56
630	10·4	16·2	33·	
700	7·9	10·3	27·	36·
800	5·0	6·0	25·	30·
905	1·6	2·8	27·	28·

* See Proc. I. A. E., Vol. XIV., pp. 439, 440, 441, 442.

(Mr. D. P. Muirhead.)

A set of valves made from Monel metal was tested in a 15 h.p. four-cylinder water cooled engine for a period of one month on the road, and at the end of that time there was no sign of pitting on the valve faces and the valves were in good condition. Valves have also been made with steel stems and cast-on Monel heads, but the results are not known. This metal has also been used experimentally for the inserted valve seats of the 240 h.p. six-cylinder Puma aero engine; the tests showed that this metal was superior to rolled manganese bronze or cast phosphor bronze, as specified for the engine. Monel metal does not oxidise as much at high temperatures as any of the steels in use, with the possible exception of high-speed and high chromium steels. It does not corrode under the action of acids produced by the burning of impure oils, and for this reason has found acceptance in Diesel engine construction. Monel metal can be readily forged and no subsequent heat-treatment is necessary as in the case of steels. This metal, however, cannot be hardened, which means that, in order to avoid excessive wear between the end of the valve and the tappet, the end of the valve must be fitted with a hardened steel thimble, unless a composite valve is used.

Mr. R. M. Maxwell wrote: From the point of view of the cheap light car it would appear to be important to obtain reliable information as to the effect of leaving out the differential, as regards the tyre wear, and the efficiency of the vehicle as a whole.

Mr. F. J. Woollard wrote: (7) *Cooling Systems.*—I think that consideration of the success of the Franklin car in the U.S.A. should modify the author's conclusions regarding air cooling.

(9) *Lubrication.*—Some authoritative statement regarding oilless bearings and their permissible loading would be most welcome.

(13) *Gears.*—There is yet a lot of work to be done with regard to the permissible load on gear teeth, particularly as regards oil toughened gears.

(14) *Wheels, Tyres and Roads.*—A comparison of the difference between the necessary factors of safety for commercial vehicles shod with pneumatic or solid tyres would be most useful. It would also be interesting to ascertain whether ball or roller bearings could be used satisfactorily on the hubs of heavy vehicles if these were mounted on pneumatic tyres.

(19) *Transmission Efficiency.*—This is most important. So far, the engine has had all the attention as far as efficiency is concerned; it is obvious that it is useless to make a highly perfected

engine if the remainder of the car is not in keeping. The investigation should cover everything from the fly wheel to the road, and include the tyres.

A further investigation that might usefully be made would be that of determining the factors of safety required over all parts of the chassis. This would help considerably those who are aiming to keep down the weight of the car.

In connection with the use of steel castings, I have lately been associated with a steel foundry, and I feel sure that the Institution could do useful work by disseminating information regarding the scope and particularly the limitations of steel castings. The founders would be pleased to give information as to what constitutes good design from the standpoint of the foundry.

Mr. Gerald Bunn wrote: The author mentions the disadvantage of paraffin as a fuel, especially the liability to leak past the piston and contaminate the lubricating oil. Does not the solution of this problem depend on (a) cooling, (b) piston design, (c) ignition?

(a) If an engine is too cool paraffin either does not vaporise sufficiently, or else condenses again on the cylinder walls. To enable the temperature to be raised, I suggest that oil should be used instead of water for cooling. Very few people seem to have heard of the 60 h.p. Hart Parr two-cylinder agricultural tractor, which has a horizontal engine and thermo-syphon *oil* cooling. These engines give good service in the American prairie lands.

(b) I suggest that the top ring in each piston should be designed as a scraper ring, similar to the oil scraper rings fitted at the bottom in many modern engines, but with the bevelled face downwards instead of upwards so that only the upper edge of the ring touches the cylinder wall. This would force condensed fuel back into the combustion chamber.

(c) Misfiring is a prolific cause of this contamination.

(6) *Valves and valve mechanism.*—I would suggest that the following additional points are worthy of further investigation:—

(a) Cam shape and periods of valve opening for (i) economy, (ii) power.

(b) Does the rate of revolution render an alteration of the periods of opening necessary or does not the increased velocity of the gas automatically govern the intake, assuming that the valve springs are strong enough to prevent lag and dancing?

(c) Variations of economy and power with (i) hot air supply

(Mr. Gerald Bunn.)
and cold induction pipe; (ii) cold air supply and hot induction pipe; (iii) The best combination of temperatures.

(d) Reliable data on air cooling by (i) induced draught, (ii) forced draught.

(13) *Gears.*—The first thing which should be established is: "Is the shape of the tooth satisfactory?" or "which of the many forms is satisfactory?" alternatively, "could not a better form be devised?" Apart from such problems, what is the comparative efficiency of ordinary generated gears and ground gears?

(15) *Spring Suspension.*—Is not the cause of the high percentage of spring failures on British cars in comparison with others largely due to the use of plain carbon steel? The irregularities in hardening and tempering would account for the variety of the breakages. A special research into the most suitable metals for laminated springs would be well worth the cost.

Mr. H. S. Rowell (Director of Research of the Research Association of British Motor and Allied Manufacturers) wrote: I have never regarded the paper as a programme of research so much as a catalogue of car features, and I had hoped that it would be of value in eliciting from automobile engineers their opinions as to which problems were most in need of investigation and most likely to prove valuable if solved.

I feel very strongly in agreement with almost all that Dr. Ormandy said regarding the unpractical scope of the paper as a programme of research. My Council have decided on four important problems, and these will be sufficient to absorb all the energy and funds of the Research Association for some considerable time.

I am deeply impressed by the serious disabilities under which the automobile industry labours, disabilities that affect seriously the work of the Research Association. No branch of engineering is so much the creature of taste and fashion as the automobile industry. No industry of anything like the same dimensions is so much dependent on the views and caprices of the layman. No industry, except, perhaps, that concerned with shipbuilding, involves so many trades, handicrafts, and other industries. It should be obvious to all reasonable people that the automobile industry, even though it could afford the money, has not the necessary knowledge or resources to conduct research on every problem that arises in automobile design. Research on cast iron

is the province, if not the duty, of the iron founders, who have formed a research association for that purpose, and so with other industries. It would appear to be the duty of the automobile engineer to state clearly his requirements to each of the industries that supply him, and to help them as far as he can in their efforts to meet his wants. The Research Association of British Motor and Allied Manufacturers is pursuing this policy by maintaining close touch with the other Research Associations, and in this co-operation the Department of Scientific and Industrial Research gives all assistance possible. At the same time the Research Association of British Motor and Allied Manufacturers is attacking with all energy possible problems which are not only urgent but which are automobile problems and which are not specifically the province of any other industry.

Mr. C. E. G. House wrote: I think it is very necessary in the first place to clear the ground by having a definite idea of what automobile research really is. Perhaps this is best done by stating what it is *not*.

(1) It is not standardisation, either of design or of material.

(2) It is not the testing of proprietary articles, such as brake linings, as suggested by the author.

(3) It is not, and can never be, a substitute for the ordinary road test for springs, brakes, etc., however interesting and ingenious the testing mechanism may be.

(4) It is not the testing of alternative designs, such as split-pin versus cotter, or two-cycle versus four-cycle, or worm drive versus helical bevel. These are problems for the designer in the first place, and the road tester in the second, not the research investigator.

My programme for research is as follows:—

Firstly, it is necessary to start collecting data of all the work already done that is of interest to automobile engineers. A good deal of this information is already in the possession of the Institution and only wants indexing. It may be that this alone would take up the bulk of the available funds. That would be a pity, but it is nevertheless the only possible first step. If the research funds may not be applied to this purpose, I would suggest that part of the S.M.M.T. grant be made available.

Secondly, standardisation must be left to the British Engineering Standards Association, tests of proprietary articles to the R.A.C., variation in design to the individual firms and those who invent and patent their inventions, commercial tests to the National

(Mr. C. E. G. House.)
Physical Laboratory, anti-dazzle tests to the Ministry of Transport, and so on.

Thirdly, co-operation should be sought with those who are engaged elsewhere upon research of interest to automobile engineers, not only to obtain the full benefit of such research but to assist in the direction of such research, so that it is carried on with the fullest advantage to our industry. We should co-operate with the metallurgists with regard to valve metal, bearing metal, foundry mixtures and the like. The fuel question, though a vital one for us, is just as vital for the nation in naval affairs, and we shall probably get our research done for us in the laboratories which the Admiralty will presently be obliged to set up. A large amount of useful work has been done on Gaseous Explosions under the ægis of a Committee of the British Association, of which the author seems to be unaware. The subject is a fascinating one to the student of pure physics, and is, perhaps, best left in his hands. It has already produced practical results for the designer, but would produce more if the average designer was capable of appreciating the subject. Co-ordination would probably be very beneficial to the designer of all types of internal combustion engines. The question of ignition apparatus and other electrical equipment would likewise best be met by co-ordination with the electrical people. Similarly with rubber tyres and roads.

It will be seen that we have nearly disposed of the author's programme without having started any definite research ourselves. Our field has shrunk, fortunately for us, seeing the small amount of money we possess, to very small dimensions.

The fourth step, then, is to select from what remains the best subject for our own research. This, I think, should be chosen on the following principles: (1) The subject should be as widely applicable as possible within the industry, and (2) should promise early beneficial results. With these principles in mind, I suggest that the first research should deal with the engine, which is common to the motor car and the motor cycle, and I think that the points to be investigated might well be the conditions governing the use of aluminium, and the behaviour of lubricants of various types under various conditions.

Lt.-Col. T. M. Hutchinson wrote: I consider that as the funds for research are limited, the experiences of users should be considered, and a large proportion of the funds available should be allotted to investigating and improving those parts of the automobile design which have given and continue to give trouble to

those responsible for the upkeep and maintenance of vehicles. At the present time there does not appear to be that close co-operation between the user and the maker in this matter which is desirable. The A.B.M.M. Research Association is limited to manufacturers only, general engineers and users not being admitted. Further, the results of the research work undertaken by this body are to be kept secret to the members of the Association. I am doubtful if this is the way to secure the best co-operation of the general engineer, the buyer, and of the user, who, after all, is the important person concerned. It is obviously desirable to avoid duplication of experimental and research work, yet this may happen if any body or firm adopt the policy of closed doors.

Mr. L. A. Legros wrote: My personal views are that in a subject covering so large a field and containing so many variables, it is desirable that a selective programme should be drawn up arranging the various items in order of importance, and that the greatest amount of assistance financially and technically should be devoted to those subjects which appear to promise the best return for the outlay.

The fuel question is of primary importance, and it is essential that it should be approached from two sides: (a) the provision of substitutes for petrol, and (b) the economical use of the fuel obtainable. The latter raises the carburettor question, and I am of opinion that the amount of waste of fuel due to badly designed, ill-made or wrongly adjusted carburettors may easily account for fully 50 per cent of the petrol used by private consumers. The cost of tuning-up most of the well-known carburettors is very heavy, in some instances it may mean running the car specially for 40 to 100 miles on the road. The adjustments, when made, are easily disarranged, and I remember a case of a car tuned-up to give 50 ton-miles per gallon which was altered by a chauffeur within a week of delivery, and the result secured at great expense was thrown away, the consumption of fuel being increased four-fold. The ideal carburettor would appear to be one in which one or two adjustments could be made by means of a screwdriver or spanner, and accessible lock-nuts on parts which are not subject to wear or necessity for removal for cleaning. It should not be put out of action by a minute drop of water, and it should be able to give reasonable economy within a range of, say, 20 per cent for the various fuels commercially obtainable and likely to be used.

(Mr. L. A. Legros.)

The second point which I think is of immediate importance is that of the brakes and brake arrangements of commercial vehicles. Cases that I have investigated have shown that there are a large number of vehicles on the road that are not only unsafe and a danger to themselves, to other vehicles and to the public, but that in some instances the defects present in the design were made worse by the use of poor material and aggravated by wear and careless adjustments. Vehicles I have examined could not have passed the requirements of the Light Locomotives Act even when new and properly adjusted, but the purchaser of a vehicle only too often assumes that the maker has fitted a brake that is, or should be, satisfactory, and will accept any excuse, such as that the surfaces are new and not worn to a full bearing, or that they are slightly greasy, in the rare instances in which a purchaser himself subjects the brakes of his vehicle to thorough test.

Mr. P. A. E. Armstrong wrote: I am interested particularly in valve steel, as this material has probably received less attention than the structural steels that go into the manufacture of an automobile; also the requirements of a valve steel are not all those of a structural variety. The properties called for in a good valve are distinctly peculiar. I believe that the author's desirable factors under the headings *a* to *i* state the whole case. I note that he suggests that Monel metal should be tried for this purpose, and I hasten to assure him that the results obtained from this metal for this particular job will not be particularly good, as, although it is a very good material for some classes of work, its use for automobile valves is not one of its strong points.

It is not a difficult thing to produce a material which will withstand hot oxidation, at a temperature, say, not exceeding 1650° F., which can be produced in an automobile engine when running on a brake test with a very thin mixture. I have seen valves operating under such conditions, and they stood this abnormal treatment for some 150 hours without the valves deteriorating. These tests, in my opinion, are in the nature of frequent tests, and do not represent any more than about one-hundredth of one per cent of the conditions that may arise, even in truck operation.

I believe that the demand for a better valve steel is right at hand, as in the endeavour to produce a more compact engine the bridge wall between each valve has been reduced, and it is no longer water jacketed. The valve seat is therefore water jacketed

only on one side, and the cast iron does not conduct away the heat from the valve quickly enough on the non-cooled side. This calls for a very non-corrosive steel at high temperatures, and also for a steel of which the coefficient is comparatively low, and I believe it is fair to think, if not say, that a grade of cast iron will have to be developed that has a low coefficient of expansion.

From my observations with engines of this character, the non-cooled bridge of the valve seat has a tendency to lift, and no matter how good the valve is, it cannot possibly seat under these conditions. The greater the angle of the seat, the less will be these difficulties.

A cylindrically ground valve would not be troubled so much with the valve being lifted out of position by the expansion of the valve seat at this thin bridge, but such a condition is purely imaginary. A valve made in this manner and a valve seat machined to take a cylindrically fitting valve would be out of the question, but the nearer the angle is to the cylindrical form, the less would be the difficulties. Freedom from surface cracks can be best met by a valve material which has a low coefficient of expansion and fairly good heat conductivity so as to prevent surface heating of the valve, thereby setting up surface strains resulting in cracks.

The ability of the steel to take a high polish calls for a material which will mechanically harden, in the nature of manganese steel, and even with this feature it would not solve the difficulty unless the scale which may form on the surface of the valve is not readily detached. Furthermore, the scale that is detached should not be of a highly abrasive nature.

I have seen valves which have a very adhering scale, but nevertheless it has popped off and got down between the guide and the stem, resulting in wear in the guides to the extent of 1/32 in. or more, merely because the scale which came off of a valve was of a very highly abrasive nature.

I do not think that it is necessary to harden the valve except at the tappet end, as any hardening that may be given to the valve is removed during the process of work. It might be an advantage to harden the valve stem to prevent wear, but if the steel naturally gets hard by mechanical work or polishing, then hardening by heat-treatment is quite unnecessary.

The valve guides, particularly if they protrude above the cast iron housing, should be of equally good material as the valve, as

(Mr. A. E. Armstrong.)
the heat on the top of the guide will set up a number of difficulties and troubles on the valve stem which would not arise if the guide was of a more suitable material. These two essentials to an excellent operating valve should receive equal investigation.

The valve materials which are produced by my company meet in a most satisfactory manner all the desirable factors outlined by the author.

In further reply to the discussion and in reply to the written communications, Mr. WATSON wrote: Dr. Aitchison apparently dismisses Monel metal because of conflicting reports of tests which have been made in America. I think, however, before we dismiss this point altogether, that we should definitely ascertain whether or no this metal has any economical advantages, because if it is proved to have long life, even if it is costly in the first place, its use might be justified. I admit that a valve made of this material might be weak in the stem, but possibly a combination of a hardened steel stem with a Monel metal head might give good results. Dr. Aitchison's reference to piston rings only emphasises my remarks on the subject. Piston friction is by far the most serious source of mechanical loss in an engine, and this is largely a question of heavy pressures of the rings on the cylinder walls to ensure anything like a gas-tight seal between the cylinders and the crankcase. It is a subject upon which we have a little information but demand much.

I fear that Dr. Aitchison has misunderstood me in the matter of small steel castings. I do not advocate them in any case where steel stampings or forgings may be used, but only to meet those cases where the quantity required is so small that the cutting of dies might not be justified, or for parts which are not suitable for forging by hand. Personally, I should strictly limit the use of castings for the very reasons given by Dr. Aitchison. In any tests on gears or shafts consideration should of course be given to the important question of dynamic loads and fatigue of material.

I particularly welcomed Dr. Ormandy's remarks. He stated, however, that: "There is going to be so little done because so much is being attempted." If my programme is to be attempted *en bloc*, then I agree that *nothing* would be done, no matter how much money we spend. Yet if the idea which I have outlined—that of a limited programme—could be agreed upon, then I see no difficulty in the way of securing much valuable information.

Dr. Ormandy knows from personal experience that I strongly advocate the principle of conducting researches step by step, limiting the programme to begin with, and extending it as may be indicated desirable by early experiments. These are the lines upon which the Empire Motor Fuels Committee is progressing. I do not agree with Dr. Ormandy that I have mixed up research, invention and standardisation. I have merely stated that research often stimulates invention, and that it is here that standardisation should be borne in mind, and referred to the B.E.S.A. In supporting his allegation, Dr. Ormandy refers to my remarks *re* the warping of clutch plates, and states that I gave the cause, together with a cure, and that the rest was merely a matter of design. That is not so. What I gave was the *effect* and its cure from the design point of view, but I did not give the cause for the simple reason that I do not know it, neither have I met anyone else who does profess to know it. The trouble is apparently due to internal stresses in the clutch plates, but we want to know what those stresses are, and how they are caused.

In answer to Dr. Ormandy's question as to what is meant by research on piston rings, I would refer him to items "a," "b," "c" and "d" under Section 5. If researches are conducted under these four heads, the points he refers to would be covered.

It is rather unusual to be told that engines, as made to-day, are good enough for many years to come, and that no more money should be spent upon researches connected therewith, which appears to be the opinion of Mr. Parnacott. He also asserts that from 40 to 60 per cent of the engine power is wasted between the clutch and the tyre. I cannot agree with him on either of these points.

Major Owen's remarks took the form of a strong appeal for co-operation in all technical matters between the commercial (sales) department and the technical men and scientists of the industry. Such co-operation is undoubtedly essential, and, although there are now encouraging signs that such co-operation is being effected, I should like to see further developments in that direction. The technical man, both collectively and individually, is generally short of funds, and it is only by the maintenance of close co-operation between him and the commercial side that his work can be carried into practical effect. If our commercial colleagues have any definite views upon research, or subjects for research, it will be well if they will bring them forward and provide the means to undertake them.

(Mr. Watson.)

Mr. Berriman, I believe, was the only member of the Research Association present at the reading of the paper, and his remarks were therefore of considerable interest, and I was glad that he asked members to focus their immediate attention upon a few concrete suggestions, a remark which at once called forth further observations from Dr. Ormandy. He proposed an immediate investigation into gas distribution in a multi-cylinder engine. All the apparatus necessary for carrying out such research is in existence, and it certainly would appear that this is one of the most important subjects to be dealt with in connection with automobiles, and, more particularly, with commercial vehicle engines. Not only is distribution the decisive factor controlling the efficiency, and, to a lesser extent, the power output, of such an engine, but it is also the subject about which least is known. It is now possible to design a single-cylinder engine to give any required performance, and to be able to state in advance, to within very fine limits, exactly what we can get from it, but when turning to multi-cylinder engines all our calculations are upset by losses in the distribution system. In some engines the distribution is quite good, yet if we follow the same design on an engine of a different size, the results are not necessarily satisfactory. The efficiency of any distribution can usually be gauged by the amount of heat applied to the carburettor and the induction system, and the deductions from theoretical considerations, backed up by practical experiments, have proved that every unit of heat added to the working fluid before its entry to the cylinder represents a dead loss to power output by reducing the density of the charge, and increasing the tendency to detonate. The probability is that it would not be possible to find a system which would enable all four or six cylinders to be fed from one carburettor, but even if two jet chambers have to be used, it would not be a serious disadvantage. I agree with Dr. Ormandy that the subject is one of such importance as to justify the making of complete researches, but, if attempted, they should be carried out under conditions which will ensure extreme accuracy and absolute consistency. They should begin with a thorough investigation into the physical properties of the various fuels, and means of ascertaining accurately both the mean mixture strength of the working fluid applied to the induction system generally, and to each system individually. I should be pleased to meet a small Committee of the Research Association and outline a detailed programme and procedure for

this particular line of research if there is any reasonable chance of their being able to provide the funds necessary to carry it into effect.

I agree with Mr. North that one of the duties of the Research Association should be to collect and circulate among its members the vast amount of technical information which may be obtained from the foreign Press, and that we should thereby benefit by what has already been done by other people.

Baron Steinheil and Mr. Evans both mention the desirability of an inquiry into two-cycle engines, and, whilst I believe that this is desirable, I am inclined to think that other matters mentioned are more urgently necessary.

I greatly appreciated the President's expression of opinion that it is for the members of the industry to say definitely what they require to be done in the matter of research, and to this I would add, that having come to this conclusion, the industry should find the money for the work to be carried into effect, and, although it is regrettable that at present the general state of the industry is not such as to encourage expenditure, I do submit that, no matter how hard up we are at the moment, we must safeguard the future by bringing our industry into a leading position instead of being at the tail end.

In answer to Mr. Gerald Bunn's written communication, I may say that all the points he raised are obvious ones, and would be dealt with in any investigations conducted on the subjects.

Mr. House put forward a programme which is substantially in agreement with mine. He then refers to the work of the Gaseous Explosives Committee, of which work I am fully informed. I submit, however, that much of the work of such Committees is wasted, unless it is put forward in such a form that designers can make use of it. The "student of pure physics" may find the work fascinating, but unless he occasionally comes down from his cloud of theories and puts his deductions into concrete form, he must not feel hurt if designers are too busy to appreciate him at his own value. No designer more fully appreciates the work of our scientific investigators than myself, but in the past much of their work has been carried out with a certain amount of disregard of practical conditions. I agree with Mr. House that the first series of researches should deal with the engine, which is now, and is likely to remain, by far the most important unit in any automobile.

(Mr. Watson.)

I fully sympathise with Lt.-Col. Hutchinson's complaint that the A.B.M.M. are pledged to secrecy. I also am of opinion that if any attempt is made to keep secret the results of any research work of importance to an industry, other investigators will retaliate by keeping their own results secret. Perfectly free intercourse of ideas and discoveries is the only safe line of progress.

In conclusion, I thank all the members who have taken part in the discussion or have sent in their observations in writing, and whilst I still have a feeling of disappointment at the results, I have hopes that the matter will not be allowed to rest without some serious effort being made to restore to the British motor industry the prestige which it has previously enjoyed.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

April, 1921.

The Eleventh Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1, on Wednesday, April 27th, 1921, at 7 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"The Ball Bearing: In the Making, under Test and on Service," by H. L. Heathcote.

There were present 76 Members and visitors.

THE BALL BEARING:

In the Making, under Test and on Service.

By HENRY L. HEATHCOTE, M.Sc., F.I.C.

Introduction.

Since Mr. Barrett* and Professor Goodman† presented their papers on the subject of ball bearings to this Institution, events have happened which necessitated a great increase in the production of such bearings in this country. Of the firms asked by the Ministry of Munitions to undertake the manufacture of ball bearings, that to which the author is attached was one. To us it was not an altogether new industry, for one of our Works Managers well remembers making steel balls for the Rudge's bearing in 1885, but, needless to add, the methods of production employed now are very different from those in vogue then. In modern factories, bristling at every turn with scientific methods and imbued with the scientific spirit, refinements are possible which would have been scoffed at years ago. Our response to the invitation of the Ministry was to erect on a site of forty acres of land at Sparkhill on the outskirts of Birmingham, a factory designed, built and equipped solely for the manufacture of steel balls and ball bearings. Here, the author is happy to say, scientific knowledge and methods are not confined to a laboratory in a corner of the factory, but irradiate the work of every department.

This paper had its origin in an attempt to understand what went on in a ball bearing when in use and under test, and a desire to devise tests which would be useful and promote the production of a perfect bearing. Much of the paper is mathematical, but an attempt has been made to present this part of the subject in a manner which will be readily followed by those who prefer simple geometrical and algebraical methods. The subject of "point"

* See Proc. I.A.E., Vol. VI., p. 195.

† See Proc. I.A.E., Vol. VIII., p. 107.

contact is and will remain a rather difficult one, but quite a useful conception of the stresses and strains can be arrived at without having to manipulate the transcendental functions developed by Hertz and used by Heerwagen, Goodman, Palmgren and others. For everyday use such functions are too complicated, and there is a real need for a simpler presentation of the subject, and one which will bring it within the scope of those who have to make, test and apply ball bearings to various uses. The author's attempts in this direction have been much aided by the contributions of others, but particularly so by the kind help of Dr. W. E. Fisher, Principal of Wolverhampton Technical Institute, and of Professor G. N. Watson, of the University of Birmingham, who have helped him through many difficulties and provided some of the solutions.

In presenting an account of the manufacture of some article, it is usual to follow it in chronological order from the raw material to the packing room, ending up, perhaps, with a photograph of the loaded lorry and an account of one or two marvellous and more or less mythical performances in use. The author proposes to follow an exactly opposite course, and to consider first the fitted bearing and the stresses and strains it experiences when on service. Then we will pass on to the bearing under test, i.e., under conditions somewhat similar to, or more severe than, those to which it is subjected when on service, and end up with an account of its manufacture.

If at first this order appears inappropriate, the author would say that surely it is the experiences a bearing has to go through which determine the qualities it has to possess, and if we could acquaint ourselves with those experiences, we could prescribe the qualities, materials and treatments to be sought. The part of the life-history of a bearing which is of the greatest interest is that which begins when it is wedded to the machine in which it is intended to run for the rest of its life. Its earlier history—pre-natal history it might be called—is, of course, a matter of great interest to its manufacturers, but the supreme moment in its life is when it is fitted into the housing prepared for it with so much care.

The Fitted Bearing.

Fit of Ball Bearings—The "Housing" Problem.—Considerable difference of opinion exists as to the proper fit, and in view of the importance of avoiding initial load on a ball bearing and the fact that, hitherto, neither the British Engineering Standards Asso-

ciation nor the Society of Automotive Engineers in America has issued any recommendations, the following information as to the dimensions of housing and shaft preferred by various users of ball bearings will doubtless be of interest.

American Practice.—According to the statistical number of Automotive Industries (January 15th, 1920) a "sucking fit" is the favourite for the outer ring when the *shaft* revolves. When the outer ring revolves a closer fit is recommended. In the United States the Society of Automotive Engineers has fixed limits for the bore of the inner ring, the outside diameter of the outer ring, and the width and eccentricity of the assembled bearing, but not all the manufacturers of ball bearings are, as yet, working to those limits. Opinions there, as to what dimensions shafts and housings for outer rings should have, vary so greatly that standardisation does not appear to be possible.

British Practice.—The fit employed by some of the users of ball bearings does not correspond with the recommendations of the bearing manufacturers. The practice of some is to fit to shafts and housings by selective assembly, the kind of fit being by no means the same for various firms.

In Table I. are tabulated some figures culled either from recommendations made by the various authorities or kindly communicated by letter. They show the maximum and minimum clearances and interferences between the outer ring and the housing, and the shaft and the inner ring that are possible and probable. The size assumed for the outer diameter of the outer ring is about 3 in. To obviate any misunderstanding, the following illustrates the method of working out.

Data:—

Limiting dimensions for diameter of outer ring ... 2·9992—2·9987
Limiting dimensions for bore of housing ..... 3·0000—2·9995

Results:—

Fits varying from 0·0013 in. clearance to 0·0003 in. clearance are possible.

According to the theory of probabilities (see "Engineering," 21-3-19, page 361), not more than 5 per cent of the fits will range from 0·0013 in. clearance to 0·0011 in. clearance, and a further 5 per cent from 0·0003 in. clearance to 0·0005 in. clearance. These figures, however, do not quite represent the facts, because of the known tendency of the operator to produce shafts nearer the upper than the lower limit, and holes and bores nearer the lower than the

upper limit. Consequently, the percentage of fits over the range 0·0013 in. to 0·0011 in. clearance will be slightly less and the percentage over the range 0·0003 in. to 0·0005 in. sensibly greater than that predicted, and the normal or mean fit will be between 0·0003 in. and 0·0008 in. clearance. In Table I., C means Clearance, and I, Interference. Both I and C are differences of diameter.

TABLE I.

Housing stationary—outer ring, nominal diam. 3 in. in housing.

Authority.	Description of fit.	Possible max.	Possible min.	Tendency towards a normal between.
		in.	in.	in.
Hoffmann ..	"Good push fit." (When extremes meet selection is recommended.)	0.0013 C.	0·0003 C.	0·0008 C. & 0·0003 C.
Skefko.....		0·0013 C.	0·0003 C.	0·0008 C. & 0·0003 C.
Gurney.....		0·0020 C.	0·0004 C.	0·0012 C. & 0·0004 C.
Fafnir	Push fit.	0·0024 C.	0·0006 C.	0·0015 C. & 0·0006 C.
Strom.....	"Sucking fit."	0·0022 C.	0·0009 C.	0·0015 C. & 0·0009 C.
Rudge-Whitworth.	Push fit.	0·0008 C.	0·0002 I.	0·0003 C. & 0.0002 I.
Rolls-Royce.	By selection the same kind of fit is ensured for all bearings.	0·0005 C.	0·0005 I.	nil. & 0·0005 I.
White and Poppe.		0·0010 C.	nil.	nil. & 0·0005 C.
Armstrong, Siddeley.	Free fit without shake.			
Lanchester	Ring clamped.	0·0023 C.	0·0005 C.	0·0005 C. & 0·0014 C.
	Ring not clamped.	0·0017 C.	0·0005 C.	0·0005 C. & 0·0011 C.

Housing revolving—outer ring, nominal diam. 3 in. in housing.

Hoffmann ..	Tight press fit.	0·0007 C.	0·0003 I.	0·0002 C. & 0·0003 I.
Skefko	Tapping fit.	0·0016 I.	0·0002 I.	0·0009 I. & 0·0016 I.
Gurney		0·0015 C.	0·0001 I.	0·0007 C. & 0·0001 I.
Strom.....		0·0016 C.	0·0002 C.	0·0009 C. & 0·0002 C.
Rudge-Whitworth.	Press fit.	0·0002 C.	0·0008 I.	0·0003 I. & ·0008 I.
Rolls-Royce.	Press fit—by selection the same kind of fit is ensured for all bearings.	0·0005 C.	0·0005 I.	nil. & 0·0005 I.
Wolseley	"Tight push fit" selective assembly found necessary.			

Shaft stationary—inner ring, nominal diam. about $1\frac{1}{2}$ in. on shaft.

Authority.	Description of fit.	Possible max.	Possible min.	Tendency towards a normal between.
		in.	in.	in.
Hoffmann ..		0·0013 C.	0·0003 C.	0·0008 C. & 0·0003 C.
Skefko.....		0·0017 C.	0·0005 C.	0·0011 C. & 0·0005 C.
Gurney ...	(Race clamped.)	0·0004 I.	0·0008 C.	0·0004 I. & 0·0002 C.
	(Race loose.)	0·0006 I.	0·0006 C.	0·0006 I. & nil.
Rudge-Whitworth.	Push fit.	0·0007 C.	0·0002 I.	0·0003 C. & 0·0002 I.
Rolls-Royce.	"Push fit"—by selection the same kind of fit is ensured for all bearings.	0·0005 I.	0·0005 C.	0·0005 I. & nil.
White and Poppe.		0·0010 C.	nil.	0·0005 C. & nil.
Armstrong, Siddeley.	"Free fit," but without shake.			

Shaft revolving—inner ring, nominal diam. about $1\frac{1}{2}$ in. on shaft.

Hoffmann ..	Tight press fit (when extremes meet selection is recommended).	0·0010 I.	nil.	0·0010 I. & 0·0005 I.
Skefko.....	Tapping fit.	0·0010 I.	nil.	0·0010 I. & 0·0005 I.
Gurney ...	(Race clamped.)	0·0010 I.	0·0002 C.	0·0010 I. & 0·0004 I.
	(Race loose.)	0·0013 I.	0·0001 I.	0·0013 I. & 0·0007 I.
Fafnir.....	Drive fit.	0·0014 I.	nil.	0·0014 I. & 0·0007 I.
Strom				
Standard Roller Bearing.	"Press fit."	0·0014 I.	0·0002 I.	0·0014 I. & 0·0008 I.
Rudge-Whitworth.	Press fit.	0·0007 I.	0·0002 C.	0·0002 I. & 0·0007 I.
Rolls-Royce.	(Race not clamped) —by selection the same kind of fit is ensured for all bearings.	0·0005 C.	0·0005 I.	0·0005 I. & nil.
Lanchester	Ring clamped.	0·0010 C.	nil.	nil. & 0·0005 C.
	Ring not clamped.	0·0008 I.	0·0002 C.	0·0003 C. & 0·0003 I.

The figures in the foregoing Table show how little real uniformity there is in the recommendations and practice adopted with regard to the fit of ball bearings. They show also that the users of ball bearings are inclined to choose the limiting dimensions of housings and shafts so that the possible clearances and inter-

ferences are not so great as the recommendations of the manufacturers would permit. That this is a step in the right direction can hardly be doubted, but the author is not sure that it is realised how important it is, to the bearing as usually made, that the amount of the interference shall not exceed a few tenths of a thousandth of an inch.

Usually, bearings are made so that they have a *just* perceptible amount of end play. Some customers, indeed, object to any end play whatever. If the balls in a bearing which has just perceptible end play be replaced by others 0·00004 in. greater or less in diameter, the bearing will become too tight or too loose—too tight because the larger balls will have a slight initial load which must detract somewhat from the longevity of the bearing; too loose if end play is objectionable and persists when the bearing is assembled.

Be that as it may, generally speaking, to satisfy a customer, balls and outer and inner tracks have to be produced and assembled of such dimensions so that the sum of their clearances does not exceed 0·00008 in., and is generally less. It is, therefore, worth inquiring what amount of interference is compatible with a like alteration of inner or outer track diameter.

In one set of experiments we have carried out, the apparatus consisted of a solid cylindrical plug about 1 in. dia., the length of the cylindrical part being about $1\frac{1}{4}$ in., which fitted a collar having an inside diameter about 1 in., external diameter about 1·5 in. and a length of 1·1 in. This collar fitted another collar about 1·5 in. internal, 2 in. external diameter, length 1·1 in. All the cylindrical surfaces were ground and lapped, and when first produced there was a small interference between collar and collar, and a considerable interference between collar and plug. The force required to move the plug within the inner collar and the outer collar on the inner was measured when the plug was inside the inner collar; 0·00005 in. was then removed from the plug diameter and the force measurements repeated, until so much had been removed from the plug that no force was required to push it into the inner collar. It was expected that the force required to push the outer on the inner collar would decrease as the plug decreased in size, and that the point at which this force ceased to decrease would correspond with the point at which the plug was of just the same size as the internal diameter of the inner collar—the difference between this dimension of the plug and its earlier dimensions

being the amount of interference between the plug and inner collar at the various stages.

If this seems a circuitous route for ascertaining the internal diameter of the inner collar, it may be urged that it has the merit of avoiding any doubt or convention as to the size of a plug which just fits a hole. Some maintain that such a plug is slightly smaller than the hole—estimates varying from 0·0001 in. to 0·0005 in.

The results of the tests with the plug and the collars show that a decrease in the diameter of the plug of 0·0001 in. is associated with a definite decrease of the force required to move one collar on the other, and a decrease to zero of the force required to move

TABLE II.

Axial Pressure.	Amount of Interference.	Sideplay.
lb.	in.	in.
Bearing (1) 0	0	0·006—0·008
Bearing (2) 0	0	0·006—0·008
Bearing (1) 225 — 450	0·0002—0·00035	0·006—0·007
(Bore of bearing slightly taper.)		
Bearing (2) 810	0·0004	0·005—0·0065
Bearing (1) 1350—1750	0·0009—0·0011	0·001—0·0015
1760	0·0010—0·0012	0·001
Bearing (2) 1760—1900	0·0011	0·001—0·0015
Bearing (1) 2300	0·0021—0·0023	nil (binding)
Bearing (2) 2420	0·0023	nil (binding)

the plug in the inner collar. The inference is that an interference of 0·0001 in. or perhaps less is sufficient to increase the outer diameter of the inner collar. A further inference is that an interference of 0·0001 in. between a shaft and the inner ring of a ball bearing will perceptibly increase the diameter of the track of the inner ring. Furthermore, an interference of less than 0·0001 in. between the outer ring and its housing will decrease the outer track diameter by an amount at least as great as the increase of the inner track just referred to.

Effect of Interference between Shaft and Bore of Type 125 *Bearing on Sideplay of Inner and Outer Races.*—Two Type 125 bearings were taken and assembled with 9/32 in. balls, the smallest

diameter of the smallest measuring N-0·00004 in. and the greatest diameter of the largest N-0·00002 in. Sideplay was measured. Plugs of different diameters were then forced through the bore of the bearing, the pressure required and the amount of interference and sideplay being as stated in Table II. The size of the bore was taken as being the same as that of a bore gauge which was supported by the inner ring. After removing 0·00005 in. from this gauge the bore would not support it.

Stresses on Automobile Wheel Bearings.

We will first consider the forces acting at the tyre contacts, and postpone the question as to how these loads are distributed among the bearings in the hub.

The loads which it will be well to take into account are:—

(1) Those due to the weight of the car and passengers (a) when running straight, (b) when rounding a corner.

(2) Those due to the effects of shock when the wheel drops into a hole or meets an obstruction.

(3) Those due to vertical undulations in the road surface.

(4) Those due to the resistance to propulsion and the acceleration and braking of the car.

To limit the number of symbols we will consider the case in which

W	= weight of car + passengers	= 3000 lb.
	width of track	= 4·5 ft.
	height of centre of gravity above ground level	= 2·0 ft.
	wheel diameter	= 3·0 ft.
$W_F + W_{F1}$	= weight on front wheels	= 1000 lb.
$W_R + W_{R1}$	= weight on rear wheels	= 2000 lb.
μ	= coefficient of friction between tyres and road	= 0·80

Some drawing offices are said to employ 0·45, others 0·6, 0·7 and 0·8 for this coefficient. In view of the experiments which J. V. Pugh carried out with a toy motor car, illustrated in the "Autocar" of April 9th, 1910, it appears probable that the coefficient is really unity, and that the earlier low values referred to above were based on studded tyres, which were more used formerly than they are now.

(1) (a). Loads, designated by W, due to the weight of the car and passengers when running straight.

$$W_F = W_{F1} = \tfrac{1}{2} \times 1000 = 500 \text{ lb.}$$
$$W_R = W_{R1} = \tfrac{1}{2} \times 2000 = 1000 \text{ lb.}$$

(1) (b). Loads, designated by L, due to the weight of the car and passengers when rounding a corner.

In the case when the inner wheels are on the point of lifting off the ground $L_F + L_R = 3000$ lb., $L_F = 1000$, $L_R = 2000$ lb., $L_{F1} = 0$, $L_{R1} = 0$.

Side thrust T is given by

$$T \times 2 = 3000 \times 2{\cdot}25,$$

whence $T = 3375$ lb., which could only be the case if a rut, bank or other resistance were added to that of the friction between the tyres of the outside wheels and the ground.

In such a case $T_F = 1125$, $T_R = 2250$ lb.

This, of course, is a very extreme case and could not, in the nature of things, happen very often in the life of a car.

Another rarely occurring case is that in which the corner is rounded at sufficient speed to skid all the wheels.

then $T = 0{\cdot}8 \times 3000 = 2400$ lb., and the couple called into play is

$w \times 4{\cdot}5 = 2 \times 2400$ ft.-lb., where w is the load added to the outside and taken off the inside wheels,

whence $w = 1066$ lb.

and

for front outside $L_F = 500 + 355 = 855$ lb.
,, ,, inside $L_{F1} = 500 - 355 = 145$ lb.
,, rear outside $L_R = 1000 + 711 = 1711$ lb.
,, ,, inside $L_{R1} = 1000 - 711 = 289$ lb.

Consider a more common case, e.g., when the vehicle is travelling at 10 miles per hour and rounding a corner of, say, 30 ft. radius.

Then $T = Mw^2R$ poundals.

$$= \frac{W}{32{\cdot}2} \times \frac{V2}{R} \text{ pounds.}$$

$$= \frac{3000}{32{\cdot}2} \times \left(\frac{88}{6}\right)^2 \times \frac{1}{30} = 666 \text{ approximately.}$$

and $T_R + T_{R1} = 444$ lb. $T_F + T_{F1} = 222$ lb.

To find the load at each hub we proceed as before, and equate couples.

For front wheels $w \times 4{\cdot}5 = 222 \times 2$,
therefore $w = 100$ lb. approximately,
and for rear wheels $w = 200$ lb. approximately,
whence $L_F = 500 + 100 = 600$ lb.
$L_{F1} = 500 - 100 = 400$ lb.
$L_R = 1000 + 200 = 1200$ lb.
$L_{R1} = 1000 - 200 = 800$ lb.

A consideration of Fig. 1 shows that the side thrusts at A and A_1 on the ground are proportional to the loads thereat.

(2) Shock load (SW) (SL).—According to the D. W. F. bearing catalogue, the effect of shock is to increase the load at the hub of

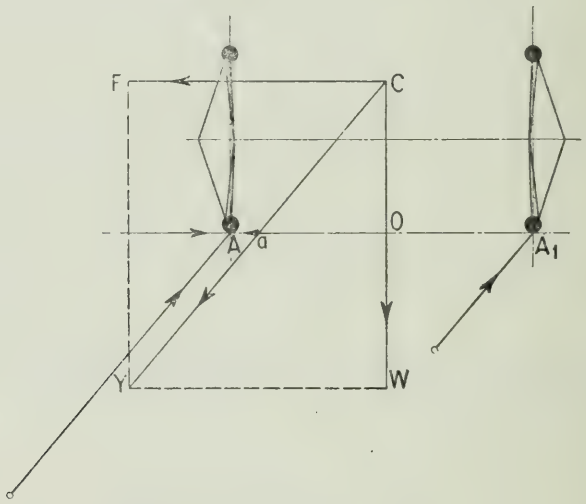


Fig. 1.

a wheel fitted with a pneumatic tyre to 1·6 times its original value. This may be a fair estimate for some speeds, but at high speeds it is too low.

Consider the case of a wheel having a radius of 1·5 ft. striking an obstacle with the result that the hub is raised relatively to its normal running position by 3 in. while the car is travelling at 40 miles per hour.

We will assume that during the displacement the pneumatic tyre exerts a uniform force vertically upward, that the mass lifted by the bearing is 50 lb., also that the change of kinetic energy $\frac{1}{2}Mv^2$ is brought about by a uniform force F, acting in the direction in which the velocity is changed.

Then, in Fig. 2,

$F \times \overline{NC} = \frac{1}{2}M \times (\text{velocity added along NC})^2$

$NC = 0{\cdot}25$ ft., and mean velocity along $NC = \frac{CN}{AN}\frac{40}{60} \times 88$ ft. per sec.

By geometry $AN = \sqrt{CN\ (2r - CN)}$

$= \sqrt{0{\cdot}25 \times 2{\cdot}75} = 0{\cdot}83$ approximately

and final velocity $= 35{\cdot}4$ ft. per sec. approximately.

Substituting, we get:—

$F = 2/32{\cdot}2 \times 50 \times (35{\cdot}4)^2$ lb. $= 4000$ lb. approximately due to the shock, to which we must add at least the original load on the bearing (more accurately, the force exerted by the spring when

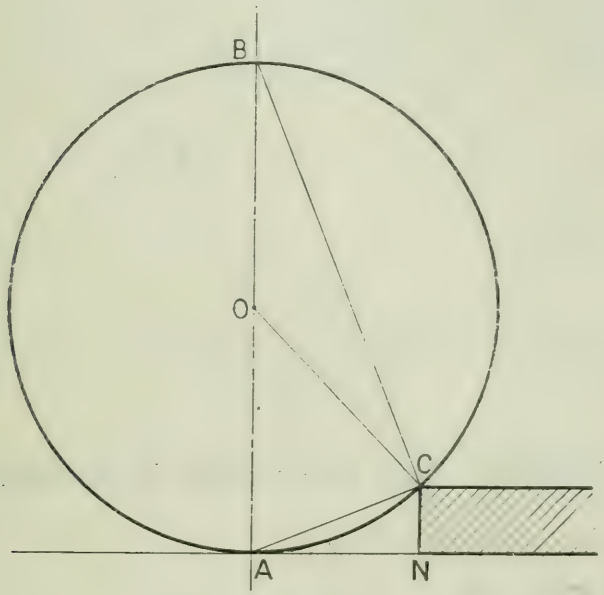


Fig. 2.

compressed during the shock), making a total of about 4500 for a front wheel, when running straight, and more when rounding a corner. For the rear wheel, the bearings of which lift, say, 100 lb. 3 in., the total load during shock works out at about 9000 lb. This is a load of some magnitude, and much greater than 1·6 times the load on the wheel.

The assumption with regard to the force F is equivalent to assuming that the path traversed by the hub is a parabola, having its vertex at O. It should be possible to obtain a record of the path actually traversed by making a small rotating centre or scriber attached at the hub mark its course on a disk attached, say,

to the mudguard in a plane parallel to that of the wheel. If a similar but eccentric scriber were made to trace its path at the same time, an idea of the path in space pursued by the hub could be obtained. The only observation the author has made is that when travelling at about 20 miles per hour, the studs in the near front tyre struck the mudguard stay 3 in. above it when standing loaded on level ground.

If the displacement of the axle follows a sine curve, the estimate of the forces required is even greater. For instance, postulating a 3 in. obstacle of which 1 in. is absorbed by the tyres and that the path traversed by the axle during displacement is the part of the sine curve shown solid: then from the form of the curve the axle, etc. will be having their velocity upwards accelerated during the first half of the time required to travel AC in Fig. 2a. The amplitude of the oscillation is 1 in. and $\lambda = 2AN$. If we assume

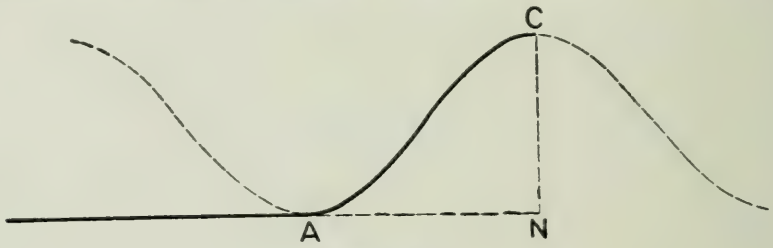


FIG. 2A.

that the indentation at the tyre reduces its effective radius to 17 in.

$$AN = \sqrt{3 \times 31} = 9{\cdot}64 \text{ in.}$$

and $\lambda = 19{\cdot}3$ in.

time of cycle = time to travel 19·3 in.

$$\text{at 40 m.p.h.} = \frac{19{\cdot}3}{12 \times 2/3 \times 88} = 0{\cdot}0274 \text{ sec.}$$

Substituting this in the general expression

$$T = \frac{2\pi}{\omega}$$

$$\text{we get } \omega = \frac{2\pi}{0{\cdot}0274} = 230$$

and maximum acceleration = $\omega^2 \times$ displacement

$$= \frac{(230)^2 \times 1}{12} = 4400 \text{ ft. per sec. per sec.}$$

$$\text{for a 50 lb. mass force} = \frac{4400 \times 50}{g} \text{ lb.} = 6800 \text{ lb. approx.}$$

This, of course, presupposes that the car as a whole is not lifted whilst the wheel is climbing the obstacle, which is virtually true owing to the period of free vibration of the car upon its springs being much greater than the time required to climb the obstacle.

(3) The forces on the wheel due to vertical undulations in the road surface.

Since the car carries the wheel hub forward nearly at its own speed, vertical undulations in the road surface must result in acceleration or deceleration of the angular velocity of the wheel. Consider the case of a car travelling at 24 miles per hour, a wheel of which mounts a short slope, say, 12 in. base and 2 in. high. The velocity of the wheel up this slope is 24·33 miles per hour, and is therefore accelerated 0·33 miles per hour or about 0·5 ft. per sec. in the period required for the car to travel 1 ft. (or less), i.e., in $3600/(24 \times 1760 \times 3) = 1/35$ sec. (or less).

Acceleration is therefore $\frac{0{\cdot}5}{\frac{1}{35}} = 17{\cdot}5$ ft. per sec. per sec. (or more). The angular velocity of the wheel also is accelerated and the force producing this acceleration resides in the ground where the tyre touches it. Its magnitude depends upon the inertia of the rotating masses that are accelerated. This is greater by far for the driving wheels, and is, the author believes, one of the main reasons why the tyres of driving wheels wear more than those of non-driving ones of the same size.

The point that concerns us at the moment is the fact that the force producing acceleration has the effect of applying a horizontal load to the bearing, equal to the magnitude of the force and acting in the same direction. Owing to the flexibility of the springs, the acceleration of the wheel will be a little less than that calculated. For a Rudge-Whitworth triple-spoked wire wheel 880 mm. × 120 mm. Type 62 fitted with a Dunlop tyre, the moment of inertia is 58·43 sq. ft.-lb., and the weight 52 lb. To ascertain the force F of the road on the tyre required to produce an angular acceleration corresponding to a linear acceleration of 17·5 ft. per sec. per sec. in such a wheel we have—Couple = Rate of change of angular momentum.

Substituting, we get

$$F \times 1{\cdot}5 = 58{\cdot}43 \times 17{\cdot}5/1{\cdot}5 \text{ ft.-poundals,}$$

therefore $F = 14{\cdot}1$ lb.

It is more difficult to state the actual force at the tyre which is called upon to reduce the angular velocity to its former magnitude as soon as the wheel tops the hillock, and calculated for accelerating the wheel up and down the hillock. At the bottom of a depression it will be of considerable magnitude and greater than that at the sides of the depression. The genesis of a pot-hole is readily imagined.

Assuming for a rear wheel that the moment of inertia of the parts accelerated is 200 sq. ft.-lb., and that the car is travelling at 48 miles per hour, the value of the frictional force will be—

$$F = \frac{200}{32{\cdot}2} \frac{17{\cdot}5 \times 4}{1{\cdot}5^2} = 193 \text{ lb. or more.}$$

When one wheel is jerked up or falls suddenly, the tilting of the axle applies a couple to the other wheel tending to twist it about an axis that passes through the hub, and is in the direction in which the car is moving.

A single bearing in the hub would, of course, be ill-adapted to withstand such a twist—the effect of which would be to incline the axes of the outer and inner rings on one another. A radial bearing would not suffer any disadvantage from such a twist, but, in the case in question, would be unsuitable on other grounds. Two bearings in the hub and connected to the tilting axle will oppose the couple. The load of each multiplied by the distance between them is equal to the applied couple, the maximum value of which will be $0{\cdot}8 \times$ load on wheel $\times$ radius of wheel, and the minimum the product of the moment of inertia of the wheel about its diameter and the rate of change of the angular velocity of the wheel about a horizontal diameter, due to tilting. This loading is as likely to come on the outer as on the inner bearing, and does not have the effect of putting side thrust on either bearing. In view of the magnitude of shock loads due to bad road surface and the greater likelihood on such roads of the axle tilting, it is of great importance that in providing for the bearings the effects of such shocks should be taken fully into consideration. On a rough road tilting a rear wheel under shock load equal to 2950 lb. may lead to loading w on each bearing, 6 in. apart, given by $w \times 0{\cdot}5 = 1{\cdot}5 \times 0{\cdot}8 \times 2950$; whence $w = 7100$ lb. If the bearings are symmetrically

placed and take an equal share of the 2950 lb. load, the total load on the inner or the outer bearing may be about 8600 lb.

(4) The forces required to overcome the resistance to propulsion and developed during the acceleration and braking of the car.

The values quoted for resistance to traction at low speeds vary from about 40 to 60 lb. per ton. Some of this force is doubtless due to the vertical undulations already discussed, part to the friction due to rolling and part to the resistance to rolling due to the elasticity of the rubber of the tyre being slightly less in the rear half of the contact surfaces than it is in the front half (*vide* Journ. Royal Soc. Arts, 7th April, 1911, page 531).

The force opposing propulsion requires effort applied at the tyre contact, which by means of the driving couple is transferred to the hub, and the effect of which at the hub is an equal force applied there. Much greater forces similarly located but in the opposite direction arise when the car is accelerated and in the opposite direction again when it is braked.

In either case the maximum value of the force will be equal to $0{\cdot}8 \times$ load. Using the letters A and B to designate horizontal forces due to accelerating and braking, then for the case when the tyres skid—

$$AW_R = AW_{R1} = 0{\cdot}8 \times 1000 = 800 \text{ lb.}$$

and

$$AL_R = 0{\cdot}8 \times 1200 = 960 \text{ lb.}$$

$$AL_{R1} = 0{\cdot}8 \times 800 = 640 \text{ lb.}$$

These forces have the same effect on the loading of the bearings as equal forces applied *at* the hubs.

The forces due to skidding during acceleration will not be affected much by unevenness of the road. It is, however, possible for skidding during braking to occur on a rough road; then the values, assuming only 60 per cent increase due to shock load, may be—

$$SL_R = 1200 \times 1{\cdot}6 = 1920 \text{ lb.}$$

$$BSL_R = 0{\cdot}8 \times 1920 = 1540 \text{ lb.}$$

Load on Rear Wheel Bearings.—The sum of the resultant loads on the bearings in the outside rear wheel when rounding a corner of a radius of 30 ft. at 10 miles per hour may therefore be—

$$R_R = \sqrt{(1540)^2 + (1920)^2} = 2460 \text{ lb. approximately.}$$

$$T_R = 265 \text{ lb.}$$

We will now enquire how this load and thrust are distributed between the bearings in the hub. For a rear wheel we will assume

that the centre line of the tyre is midway between the two bearings, and that the distance between them is 6 in. In this case each bearing will take $\frac{1}{2}R_R = 1230$ lb. The effect of the couple $T \times 1{\cdot}5$ in the case of the outer wheel is to increase the load on the inner

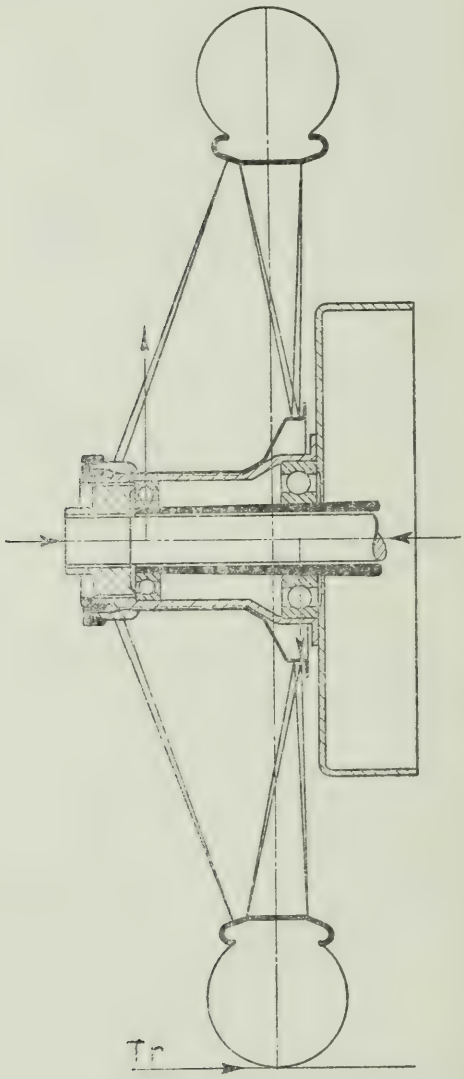


FIG. 3.

bearing and decrease that on the outer by w (see Fig. 3). Equating couples, we have—

$$T_R \times 1{\cdot}5 = w \times 0{\cdot}5.$$

whence $w = 800$ lb. (about). The resulting load on the bearings due to $\frac{1}{2}R_R$ and w is, for the inner bearing of the outer wheel,

1920 lb. For the outer bearing of the same wheel the resultant is 785 lb.

From this it is clear that the inner bearing should, wherever possible, be relieved of side thrust and provision made whereby the outer bearing takes all the thrust. Furthermore, the two bearings should have about the same load-carrying capacity. It is true that in the case of the inner wheel the torque due to side thrust will increase the load on the outer bearing, but in this case the load is less than that on the outer wheel, and if the two bearings are of about the same load-carrying capacity the outer one will be quite equal to taking all that it gets.

A factor of safety of 2 to $2\frac{1}{2}$ will provide safely for all unconsidered contingencies, such, for instance, as a resultant radial force at the brake drum, due to the pressure of the brake shoes being unbalanced, or the effect of the braking forces on the brake shoe pivot and viâ the axle on to the bearing. It is worth noting in passing that such forces may either increase or decrease the load on the inner bearing, which should be placed at the middle of the brake drum in order to reduce to a minimum any possible increase of loading due to such forces.

In the case of the front wheel, the centre line of the tyre is generally nearer to the inner than the outer bearing. Taking the case when the centre line of the outer bearing is 4 in. from the centre line of the tyre, and, for the inner bearing, $1\frac{3}{4}$ in. when running straight,

$$W_F = W_{FI} = W_{FO} + W_{FI} = 500.$$

Taking moments—the share of the load W_{FO} borne by the outer bearing is given by—

$$W_{FO} \times \frac{4}{12} = W_{FI} \frac{1{\cdot}75}{12}$$

whence $$\frac{W_{FI}}{W_{FO}} = 2{\cdot}28.$$

Substituting in $W_{FO} + W_{FI} = 500$

we get $$W_{FI} = 500 - \frac{W_{FI}}{2{\cdot}28} = 350 \text{ lb. nearly}$$

and $$W_{FO} = 150 \text{ lb. nearly.}$$

As before, we find the loading effect due to the force of the ground on the wheel sideways by equating—

$$T_F \times 1{\cdot}5 = w \times 5{\cdot}75/12$$

whence $w = (18 \times 133)/5{\cdot}75 = 416$ lb.

The resultant load on the inner bearing will be—

$W_{F1} + 416 = 766$ lb., and on the outer -266 lb.

The negative sign indicates that it acts in a direction opposite to the resultant load on the inner bearing. In the case of the front

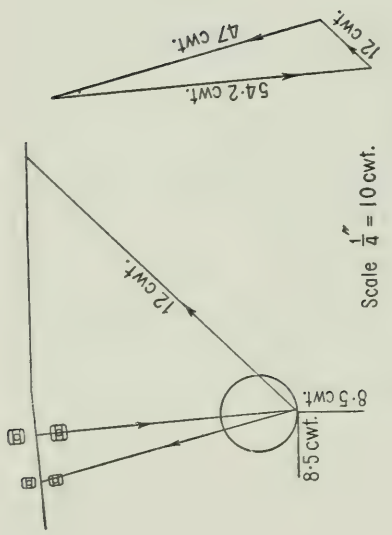


FIG. 3A.—Front wheel.

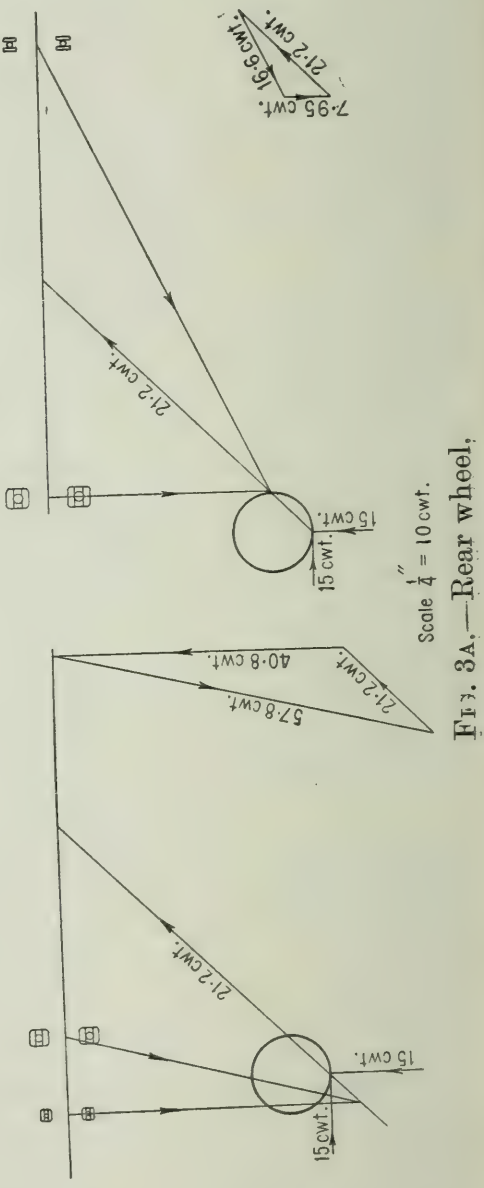


FIG. 3A.—Rear wheel.

wheel as with the rear, the outer bearing has the easier time. The inner bearing is not subjected to such heavy loads as that of the rear wheel; it seems advisable, however, to relieve it of side thrust.

As Fig. 3a shows, the forces on the outer and inner bearings attain considerable magnitude when a lorry wheel skids (a coefficient of 1·0 is assumed). By providing a thrust washer near the differential casing the forces on the wheel bearings are greatly reduced.

Lubrication and Protection of Ball Bearings.—This very important aspect of the subject has been fully treated by Mr. G. F. Barrett. Without doubt, inadequate lubrication and protection still account for very many failures and much disappointment. The provision which can be made is, for the most part, a matter which the user of ball bearings has to decide.

With regard to fit, it is to be recommended that the revolving ring of the bearing is about 0·0005 in. larger than that of its

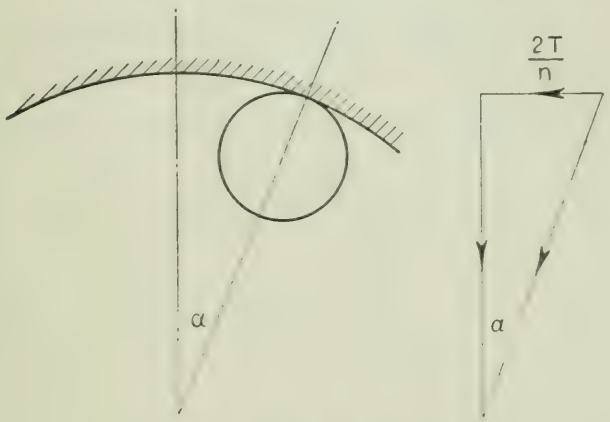


Fig 4.

housing, both housings and bearings being selected to maintain this amount of interference approximately constant. The non-revolving element should be a push fit, and should be made of the same size as the shaft it is intended to fit, uniformity being ensured by selecting both bearings and shafts.

Ability of Journal Bearings to withstand Side Thrust.

It is too often stated that journal bearings will not carry end thrust. Such statements are misleading and detract from the usefulness of journal bearings. What, of course, is perfectly true is that journal bearings must be fitted so that they are not subjected to initial end thrust.

The results of the mathematical investigation which follow are

confirmed by the results of actual tests, and show that the side thrust to which automobile wheel bearings are subjected during use can be taken by journal bearings without over-stressing them. It will make the case of the single row bearing easier to follow if we first consider the double row radial type, Fig. 4, in which the outer raceway is part of a spherical surface and r_1 the radius of curvature in the direction of rolling is equal to r_2 the radius of curvature in a plane containing the axis of the bearing. If T be the thrust load in lb., assuming no initial compression on the balls, the thrust will be taken up by the balls of one row only and the thrust per ball will be $T/(n/2) = 2T/n$ where n is the number of balls in the bearing. The actual force between raceway and ball has a direction which passes through the geometrical centre of the bearing. From the diagram it is clear that its magnitude will be $2T/n.\text{cosec } \alpha$ and its component in a radial direction will be $2T/n \cot \alpha$. A radial component of this magnitude acts upon each ball in this row, and such forces, being symmetrically arranged, have no resultant assisting in carrying the journal load.

We can now calculate the journal load which, if acting alone, would produce on the loaded ball the same load as in the present case.

According to Stribeck, the maximum radial load per ball due to a journal load L will be $5L/n$ (approximately). To find L we put $5L/n = 2T/n.\cot \alpha$, whence $L = 2/5\ T \cot \alpha$ or a thrust load T, in such a bearing, produces ball loads equal to the greatest with a journal load $2/5.T \cot \alpha$; to find the equivalent radial load due to radial and axial loads combined, we must add to the actual radial load $2/5.T \cot \alpha$ where T is the axial load.

For a bearing of this type (345) α is approximately 10 deg. 21 min. and cot 10 deg. 21 min. = 5·5 nearly.

In this case, therefore, the equivalent radial load (i.e., equivalent in the sense that such a load will produce an equal maximum load stress at the contact surfaces) $= 2/5 \times 5{\cdot}5\ T = 2{\cdot}2\ T$. When the bearing is subjected to radial load R and axial load T, if P be the limiting safe load, then the selection must be made so that $P = R + 2{\cdot}2\ T$ or $> R + 2{\cdot}2\ T$. We must, however, remember that the relation derived by Stribeck is one which applies to load stress only. In the case we are considering under combined journal and side thrust, one row of balls is much more loaded than the other, with the result that the angular velocity of the more loaded row round the axis of the bearing will be less than that of the less

loaded row. The difference will be small but it must exist, and the effect when the bearing revolves will be that the more loaded row will retard the cage and through the cage the other row of balls, with the result that the less loaded balls will be made to skid or to slip and the more loaded ones to get hotter than they otherwise would. In severe cases the cage also may suffer.

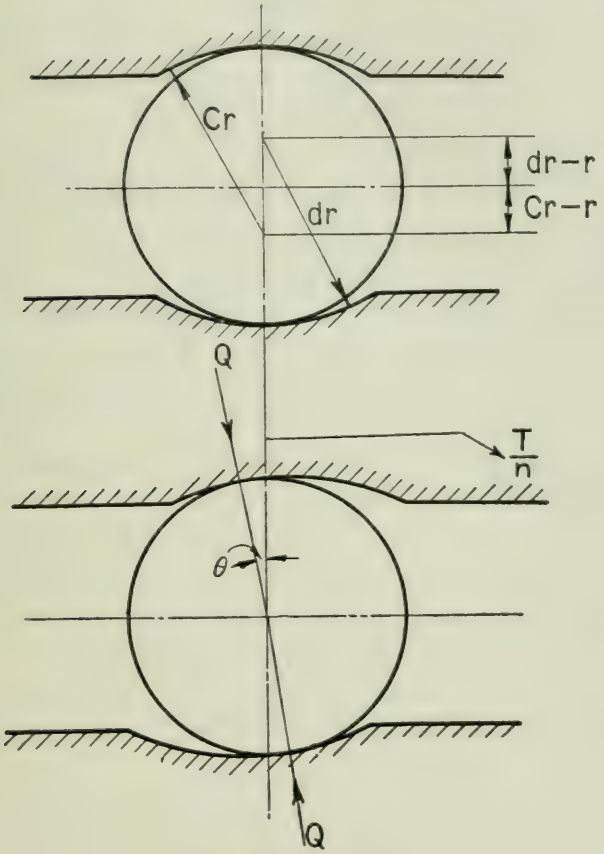


FIG. 5.

Furthermore, since the balls spin about an axis through the centres of their contact surfaces as well as roll, the material of the ball and raceway near the contact surface will be the seat of shearing stress as well as of load stress. Consequently Stribeck's expression is not applicable without a correction, the sense of which will be to increase the factor 2·2.

Various formulæ have been put forward from time to time for the relation between the limiting safe load P and the axial and radial loads for this type of bearing, e.g.—

$P = 4T + R$ or P must exceed $4T + R$.
$P = 7T + R$ „ P „ $7T + R$.
$P = 6T + R$ „ P „ $6T + R$.

Single Row Journal Bearing.—A glance at any diagram (see Fig. 5) of a section of a journal ball bearing gives the impression that side thrust must develop an enormous bursting load. Were the position of the ball and races to remain as shown in the diagram, the bursting stress would, of course, be nil. The idea that the bursting stress must be very considerable probably arises from the mental picture of the balls compressing ever so slightly. If the compression were sufficient to allow the ball to take up a position in which the line joining the contacts made an angle of 10 deg. 21 min. with the median plane of the bearing, clearly such a bearing would be able to withstand at least as much side thrust as the double row radial bearing we have been discussing. In fact it would probably function better because it has but one row of balls, so that one row will not tend to drag the cage and the other row along after it. In Fig. 5, which represents the case when there is no axial load, let r be the radius of the ball and r_2 for the outer ring be cr and r_2 for the inner ring be dr.

Then the distance between the centres of the curves radii cr and dr is $cr - r + dr - r = r(c + d - 2)$. If the bearing be now subjected to an axial load T lb., relative movement of the races will occur and the loci of the centres of these curves will be lines parallel to the axis of the bearing and passing through the positions occupied by the centres in the unloaded condition.

If the load on a ball be Q, then $(T/n)/Q = \sin \theta$ where n is the number of balls.

The distance between the race curve centres increases to

$$\frac{r(c + d - 2)}{\cos \theta}.$$

∴ the sum of indentations measured radially when the line joining their centres is swung through θ

$$= \frac{r(c + d - 2)}{\cos \theta} - r(c + d - 2)$$

$$= r(c + d - 2)\,\frac{1 - \cos \theta}{\cos \theta}.$$

If we can ascertain the radial compression we can equate it to this expression for $\cos \theta$ and thus find θ for any value of the radial compression, and by substituting then for $\sin \theta$ in the former expression we can, knowing T, find Q, and then, by apply-

ing Stribeck's equation, find the journal load which will produce as great a load on any ball.

We will suppose that T is such that each ball is subjected to its listed limiting safe load. For a Type 345 bearing containing ten $\frac{5}{8}$ in. balls this is 1590/(10·5) = 795 lb. when running at 1400 revs. per minute = say, 800 lb.—when the bearing is under pure radial load. Assuming that this is also the limiting safe load when the balls are subjected to side thrust, then, according to Heerwagen (see Goodman Inst. C. Engrs. 1911—12)

$$\text{the approach } \delta = \frac{1}{32000} \times \sqrt[3]{\frac{800^2}{0{\cdot}625}}$$

for two flat plates with a 0·625 in. ball between them and less than this for hollow grooves.

To find the value of the coefficient for reducing the approach with flat plates to that with ball races, we can extrapolate the graph obtained by plotting the values for δ with different curvatures of ball and raceway. In this way we find 0·6 for the case when the radius of curvature of tracks (r_2) is 1·05 to 1·1 times that of the ball. Then:—

$$\frac{1-\cos\theta}{\cos\theta}(c+d-2)(0{\cdot}3125) = \frac{0{\cdot}6}{32000}\sqrt[3]{\frac{800^2}{0{\cdot}625}}$$

Assuming that $c = d = 1{\cdot}075$, then

$$\frac{1-\cos\theta}{\cos\theta} = \frac{0{\cdot}6}{32000} \times \sqrt[3]{\frac{800^2}{0{\cdot}625}} \times \frac{1}{0{\cdot}3125} \times \frac{1}{0{\cdot}15}$$

or

$$\frac{1}{\cos\theta} = 1 + 0{\cdot}04032$$

and

$$\theta = 16 \text{ degrees.}$$

We can now find the corresponding side thrust from the equation

$$T/(n.800) = \sin 16 \text{ degrees} = 0{\cdot}2756$$

$$\therefore T = 10 \times 800 \times 0{\cdot}2756$$

$$= 2200 \text{ lb. about.}$$

Experimental evidence as to the superiority of single row journal bearings for withstanding side thrust is given in the part of the paper dealing with the ball bearing under test.

The Ball Bearing "On Service."

We will now approach the ball bearing more closely, and consider the stresses and strains it is subjected to whilst on service.

It will be convenient to classify the stresses according to their kind and the manner of their application as follows:—

Load stresses.
Friction stresses.
Heat stresses.
Centrifugal stresses.

Needless to say these stresses are greatest at and in the region of the "points" of contact between the balls and the raceways.

If a body having a smooth convex surface is laid upon another

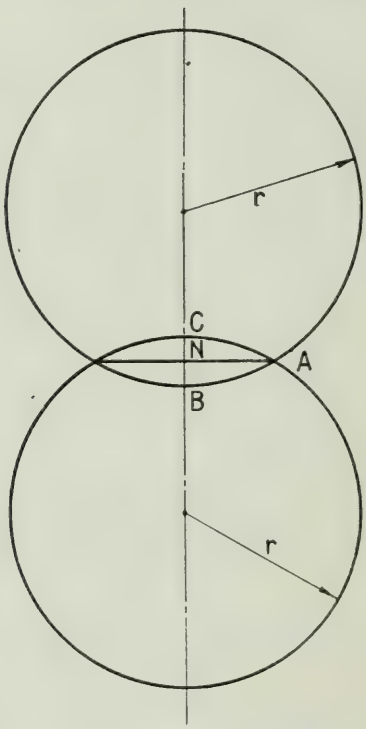


Fig. 6.

with a smooth surface the curvature of which is, in any plane through the "point" of contact, less than that of the other body, it is obvious that since the "point" of contact must be momentarily a mathematical point (or as nearly so as the molecular constitution of matter permits), however small the body may be, the pressure per sq. in. must be nearly infinitely great. The duration of such a state of stress will be exceedingly short, for pressure at a point deforms the surface adjacent to the point into a curved surface such as would be formed by the revolution of an hyperbola about the line of application of the pressure, and as both of the

surfaces in contact must be assumed to be so deformed as long as the contact is a mathematical point, it is unthinkable that such a state of strain can persist longer than an instant. Since all bodies deform under load and possess a finite elasticity, it follows that all contacts with which we have to deal are contacts in which a finite area of each surface is in contact with the other.

Form of the Contact Surface.—This will be completely defined if we can state its curvature in all directions and the form of its periphery. When two homogeneous balls (Fig. 6) of equal diameter and of equal elasticity are pressed together, it is unthinkable that the common contact surface can be other than flat and in a plane at right angles to the line joining the ball centres, and, by symmetry, circular whether the load sustained by the balls be great or small. Expressed in other terminology, the contact surface is, in this case, represented by a family of concentric circles the diameter of which is a function of the pressure. Still considering two balls of equal size and similar material, if we wish to express the curvature of the contact surface in terms of the curvatures of the contacting balls, we may say that it is the arithmetic mean between the curvature of their surfaces when not loaded. At any point in the contact surface, the material of one ball has been displaced by an amount which is obviously exactly equal to that of the material in the other ball. The author thinks it is obvious, also, that when the contact surface is small enough to be regarded as a mathematical point, the displacement of the material near each of the contacting surfaces will be equal; this will be so, of course, whatever the form of the contacting surfaces, and whatever the load—provided that the surface remains exceedingly small. Hertz (in 1881) showed that, assuming that the contacting bodies are isotropic, that all the strains are within the elastic range, that the reactions are at right angles to the contacting surfaces (i.e., that friction might be ignored) and that the dimensions of the contacting bodies are infinitely great compared with those of the contact surface (i.e., that the contact surface is a mathematical point, or very nearly so), the displacements of the materials of two contacting bodies at the contact surfact are equal in magnitude and opposite in direction. The consequences of this are important, for if the displacements are equal for each of the contacting surfaces, and the final curvature is the same for both, it follows (by arithmetic—from Newton's definition of curvature) that the final curvature is the arithmetic

mean between the initial curvatures, and this however we choose to represent them and however great the load, provided the assumptions already referred to hold good. If r and R be the radii of curvature of the contacting bodies in the plane in question, and ρ be the radius of curvature of their contact surface in the same plane, then

$$\frac{1}{\rho} = \tfrac{1}{2}\left(\frac{1}{r} + \frac{1}{\mathrm{R}}\right)$$

in which curvatures that have their centre on the same side of the contact surface have the same sign. ρ is approximately constant for all points along the arc of contact.

Flexure.—In the case of a ball (see Fig. 7), the curvature of the contact surface will always be less than that of the unloaded surface of the ball: it is flexed, we may say, in a negative direction. At the edge of the contact surface, however, the reverse is the

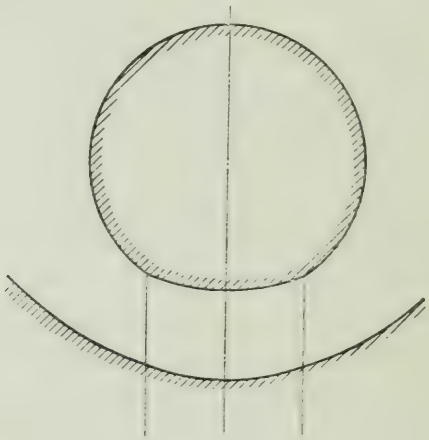


Fig. 7.

case, and if we consider a short length of the ball circumference, part of it in the contact surface and part beyond it, it is clear that the flexure of the surface is positive at the periphery of the contact surface.

In the case of the ball race, the curvature of the contact surface will generally be greater than that of the unloaded raceway. The curvature of the contact surface in a plane containing the axis of the bearing will always be greater than that of the unloaded raceway, and we will call its flexure negative since it is such as to induce a state of stress similar to that in the ball on the other side of the contact surface. At the periphery of the con-

tact surface the flexure of the surface of the raceway changes sign. The same holds good for the curvature and flexure of the contact surface between the ball and the outer raceway in the direction of rolling, also for the flexure of the inner raceway; the curvature of this contact surface may, however, be greater or less than that of the unloaded race according to the relative dimensions of the ball and the diameter of the race in the direction of rolling.

Magnitude of the Flexure.—Since the curvature of the contact surface is the arithmetic mean between the curvature of the two unloaded surfaces, it follows that the flexure of the ball in any

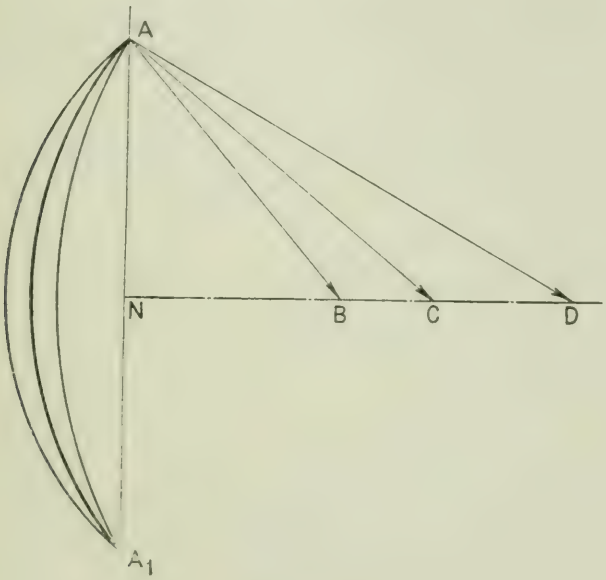


FIG. 8.

plane is equal to that of the raceway it touches in the same plane. Obviously, too, the flexure of the ball and the race will be the less the closer their original curvatures approach one another —presupposing, of course, that the one embraces the other. The flexure of the ball at its contact with the inner race in the direction of rolling will be greater than elsewhere and in any other direction.

Relation between the Flexure and the Radii of Curvature.—The flexure in a given plane at the periphery of the contact surface increases as the distance from the centre to the periphery is increased by increasing the load. In Fig. 8 the three curved lines between A and A^1 represent the three curved surfaces, the most

curved being that of the ball, the least curved that of the raceway, and the intermediate curve (shown thicker) the arithmetic mean between the other two, i.e., the curvature of the contact surface which ends—in the plane under consideration—at A and A^1. In Fig. 8—

AB represents the radius of the ball $= r$.
AD „ „ race $= R$.
AC „ „ contact surface $= \rho$

Then assuming that the ball and race are flexed only at their contact surfaces, the flexure of the ball in the plane in question is the angle BAC; that of the raceway being the angle CAD. We will call these angles θ. To find an expression for θ in terms of r and R, draw AN perpendicular to DB produced and let the angle NAB be designated by ϕ, and AN by α. Then since

$$\sin 2\theta = \sin (\overline{2\theta + \phi} - \phi)$$

$$\therefore \sin 2\theta = \sqrt{\frac{R^2 - \alpha^2}{R}}\,\frac{\alpha}{r} - \frac{\alpha}{R}\sqrt{\frac{r^2 - \alpha^2}{r}}$$

$$\text{or } \sin 2\theta = \sqrt{\frac{R^2\alpha^2}{R^2r^2} - \frac{\alpha^4}{R^2r^2}} - \sqrt{\frac{\alpha^2r^2}{R^2r^2} - \frac{\alpha^4}{R^2r^2}}$$

Since α is a very small magnitude and R and r are not small, we may consider that $\alpha^4/R^2r^2 = 0$. Whence $\sin 2\theta = \alpha\left(\frac{1}{r} - \frac{1}{R}\right)$ in which, as before, curvatures which have their centres on the same side of the contact surface have the same sign. This relation will be true only as a first approximation, since the bodies are flexed beyond the parts in contact, the effect of which is to make $AB > r$ and $AD < R$. For very small values of θ, $\sin 2\theta = 2\theta$. Approximately, therefore, the flexure θ at the periphery in a given plane is proportional to the distance from the centre of the contact surface to the periphery for constant values of the radii of the ball and the raceway. This is likely to be true in spite of the flexure extending beyond the surface of contact.

Relation between the Flexure and the Radial Compression.—A simple expression connects the flexure of the bodies in contact at the periphery of the contact surface with the radial compression at the centre of that surface. In Fig. 9, EF represents that part of the reduction in the distance apart of points in the bodies well away from the contact surfaces on a line passing through the centre of the ball and the centre of the contact surface which is due

to the deformation of the ball and raceway at the contact surface.

$$EF = EN - FN$$

$$= \frac{a^2}{2r} - \frac{a}{2R} \text{ approximately) by geometry}$$

$$= \frac{a^2}{2}\left(\frac{1}{r} - \frac{1}{R}\right).$$

Substituting in this the value of $\left(\frac{1}{r} - \frac{1}{R}\right)$ in terms of $\sin 2\theta$ we get $EF = \frac{a^2}{2}\frac{\sin 2\theta_\alpha}{a} = a\theta_\alpha$, approximately.

For two curved surfaces in contact under constant load EF is constant.

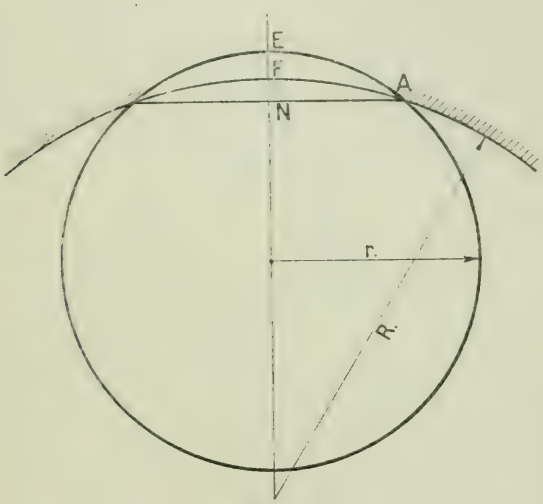


FIG. 9.

Under these conditions, therefore, $a\theta_\alpha$ is constant for all planes passing through the line through the centre of the contact surface and normal to it. This is only approximate, because the load flexes the surfaces of the ball and the race in the region of the periphery of the contact surface, the effect of which is to reduce the flexure *at* the periphery below that given by the expression and to a greater extent the greater the flexure calculated.

For a type MS 16 journal bearing containing twelve $\frac{3}{4}$ in. balls carrying a load of 7600 lb. (about three times the listed limiting safe load at 1400 revs. per minute), the flexure, calculated by the above formula, at the *end* of the contact surface on the inner race is 0 deg. 32 min. At the *side* of the same contact surface the flexure is 1 deg. 43 min. 30 sec.

For a type MS 7 journal bearing containing ten 9/32 in. balls carrying a load of 1370 lb. (about three and a-half times the rated load at 1400 revs. per minute), the corresponding flexures work out at 0 deg. 37 min. and 2 deg. 2 min. 30 sec.

For a Type W 3 thrust washer containing sixteen $\frac{1}{2}$ in. balls running at 1400 revs. per minute, and supporting a load of 1967 lb. (about double its rated load) the flexures are 0 deg. 3 min. at the end of the major axis and 0 deg. 55 min. at the end of the minor axis.

Frequency of the Flexure.—As a ball in a ball bearing rolls round the tracks, the flexure at any point on its surface changes sign twice as the point passes across a contact surface—once at (or near) each crossing of the periphery. If the same point continues to pass through the contact surface at both rings, the flexure will change its sign four times during each revolution of the ball, and as long as the ball revolves about the same axis, the flexure will change its sign four times for each revolution of the ball.

If R_o be the radius of the outer track
R_i „ „ inner „
R „ „ ball
and N be the revs. per minute of the outer ring, the inner being assumed stationary, then revs. per minute of ball =

$$N(R/(R_o + R_i))\,(R_i/R) \text{ approximately,}$$

and revs. per minute of cage $= N(R_o/(R_o + R_i))$.

For a Type 345 Rudge-Whitworth bearing, as tested under pure side thrust in the Research Laboratory—
$R_o = 1{\cdot}740$ in., $R_i = 1{\cdot}115$ in., $R = 0{\cdot}3125$ in., $N = 1400$ revs. per minute. Substituting, we get—

$$\text{Frequency of flexure of ball} = \frac{4 \times 1400 \times 1{\cdot}74 \times 1{\cdot}115}{2{\cdot}855 \times 0{\cdot}3125}$$
$$= \text{about } 12{,}000 \text{ per minute.}$$

As the bearing contains ten balls, the total number of flexure reversals on balls may be 120,000 per min., or about 7·2 millions per hour.

For the same bearing the frequency of flexure reversals at each point of the inner race is

$$\frac{2 \times 10 \times 1400 \times 1{\cdot}74}{2{\cdot}855} \text{ per minute.}$$
$$= \text{about } 17{,}000 \text{ per minute.}$$
$$= \text{about } 1{,}000{,}000 \text{ per hour.}$$

Consequently, in such a bearing run at such a speed there may be as many as 9·2 million reversals in all at points, one in each

raceway surface and one in each ball surface each hour that the bearing is run. Judging from the appearance of the balls taken from tested journal bearings they do not run on the same "circle" of the ball. Consequently the figure 7,200,000 is in some cases, and probably generally, too high for the flexure reversals at points in the balls.

If the bearing be run at the same number of revs. per minute, but with the outer ring stationary and the inner ring revolving, (as in our running tests under radial and combined radial load and side thrust load) the number of reversals is the same, but the speed of the cage and of the balls about the axis of the bearing is approximately $R_i/(R_o + R_i)$, which is less than before.

Dimensions of the Contact Surface.—When dealing with the form of the contact surface, it was noted that its form was completely defined by stating its curvature in all directions and the form of its periphery. By symmetry it follows that the periphery of the contact surface between two balls, or between a ball and a flat plate or hollow spherical shell must be a circle. Before loading, the locus of points on the ball and the surface it touches, which after compression will be on the periphery of the contact surface, is obviously a circle; it is not, however, quite correct to assume that in the case of a ball in a ball race, the locus of such points is the same after loading as it was before. Before loading, the locus of points for which the distance between the two surfaces is constant (or, more precisely, the locus of their projection on the plane tangential to the bodies at their points of contact) is an ellipse $z = Ax^2 + By^2$ where z is the constant distance between the points on the two bodies and A and B are constants which depend upon their radii of curvature. The effect of loading is to cause the distance z to decrease more at some points on this ellipse than at others, with the result that the periphery of the contact surface (or more precisely its projection on the tangential plane) is an ellipse of somewhat different form. We can compare these ellipses by considering the value of the ratio of the semi-axes for each. Let the semi-axes of the "unloaded" ellipse be α and β, and r the radius of the ball, r_1 that of the race in the plane containing β and r_2 that in the plane containing α.

Then by geometry $\frac{\alpha^2}{2}\left(\frac{1}{r} - \frac{1}{r_2}\right) = \frac{\beta^2}{2}\left(\frac{1}{r} - \frac{1}{r_1}\right)$,

for the case of a ball in the outer ring of a ball bearing. Whence

$$\frac{a^2}{\beta^2} = \frac{\frac{1}{r} - \frac{1}{r_1}}{\frac{1}{r} - \frac{1}{r_2}} \text{ and } \frac{a}{\beta} = \sqrt{\frac{\frac{1}{r} - \frac{1}{r_1}}{\frac{1}{r} - \frac{1}{r_2}}}$$

For the case under consideration Hertz's expression for cos τ reduces to—

$$\cos\tau = \frac{\frac{1}{r_1} - \frac{1}{r_2}}{\frac{2}{r} + \frac{1}{r_1} + \frac{1}{r_2}} \quad \therefore \frac{\cos\tau + 1}{1 - \cos\tau} = \frac{\frac{2}{r} + \frac{2}{r_1}}{\frac{2}{r} + \frac{2}{r_2}}$$

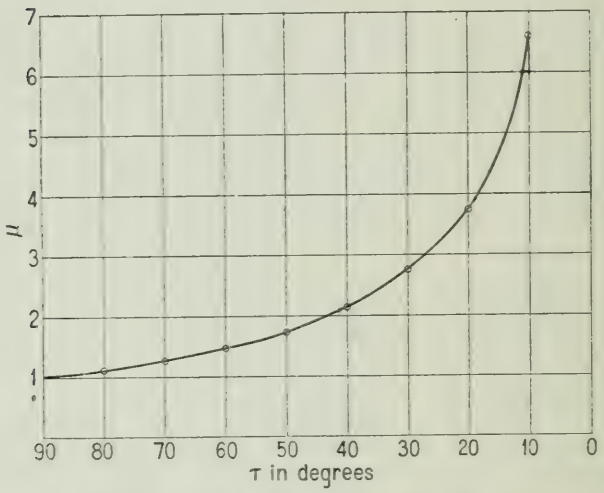


FIG. 10.—Graph of μ against τ.

whence, putting in the signs proper to the case in question—

$$\frac{a}{\beta} = \sqrt{\frac{\cos\tau + 1}{1 - \cos\tau}}$$

For the pressure ellipse, the semi-axes of which are a and b, according to Hertz (see also Palmgren, Engineering, 28/2/19, pp. 289—291)—

$$a = \mu q\,;\ b = \nu q$$

$$\text{where } q = 1{\cdot}40 \sqrt[3]{\frac{P}{E}\left(\frac{rr_1r_2}{rr_1 + rr_2 + 2r_1r_2}\right)}$$

and μ, ν, are functions of τ as shown in the two graphs (Figs. 10 and 11), P the load on the ball, and E the elastic constant. If P is expressed in lb., E in lb. per sq. in. (30×10^6) and

r, r_1 and r_2 in inches, q is found in inches. If P is in kg., E in kg. per sq. cm. (2,150,000), r, r_1 r_2 in cm., q is found in cm.

For a Rudge-Whitworth Type 345 bearing, under its test load of $3\frac{1}{2}$ times the listed load for the outer ring—

$a = 0{\cdot}111$ in. by calculation and $0{\cdot}107$ in. by measurement.

$b = 0{\cdot}0233$in. by calculation.

Ratio—$a/b = 4{\cdot}75$.

While $\cos\tau = 0{\cdot}83$, whence $\frac{a}{\beta} = \sqrt{\frac{1{\cdot}83}{{\cdot}17}} = 3{\cdot}28$.

From this it follows that the contact ellipse is relatively longer and flatter than the ellipse calculated without allowing for the fact that at some points on the ellipse $z = Ax^2 + By^2$ the approach is more than at others. The ratio of the axes of the contact ellipse, however, remains constant as the pressure increases.

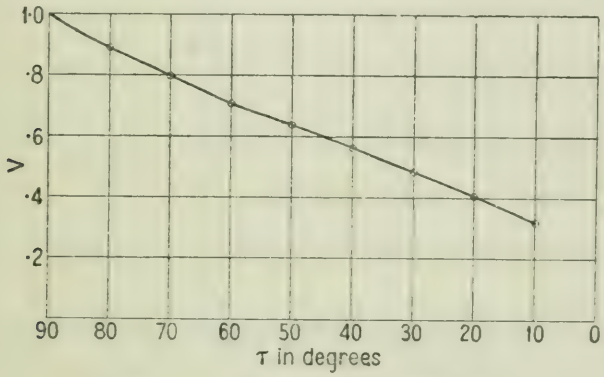


FIG. 11.—Graph of v against τ.

Relation between the Radial Compression and the Radii of Curvature, Load and Elasticity of the Contacting Bodies.

Assuming the curvatures of the bodies to alter only at the contact surface, the radial compression of two identical balls (see Fig. 6) radius r, or the approach of their centres, is approximately

$$\mathrm{BC} = 2\mathrm{NB} = \frac{2(\mathrm{AN})^2}{2r} = \frac{a^2}{r}$$

where a is the radius of the contact circle.

Hertz's expression for the approach for all cases where the contact surface has a circular periphery is

$$\delta = \frac{3p}{16}\,\frac{\Im_1 + \Im_2}{a}$$

When the contact surface is circular, Hertz's expression for the relation between the radius a of the contact surface and $p_1(\Im_1 + \Im_2)$ reduces to

$$\int_o^\infty \frac{du}{(a^2 + u)^2 \sqrt{u}} = \frac{A}{\Im_1 + \Im_2} \quad \frac{16\pi}{3p}$$

where A is $1/r$ if the contact is between two balls of radius r. By integrating the left side of this we can find a value for the right side which can be substituted in the expression for δ. Substitute $u = a^2x^2$.

Then $$\frac{2}{a^3}\int_o^\infty \frac{dx}{(1 + x^2)^2} = \frac{A}{\Im_1 + \Im_2} \quad \frac{16\pi}{3p}$$

Substitute $x = tan\ \downarrow$.

Then $$\frac{2}{a^3}\int_o^{\frac{\pi}{2}} \cos^2 \downarrow d\downarrow = \frac{A}{\Im_1 + \Im_2} \quad \frac{16\pi}{3p}$$

$$\therefore \frac{\pi}{2a^3} = \frac{A}{\Im_1 + \Im_2} \quad \frac{16\pi}{3p}.$$

Substituting this in the expression for δ we get

$$\delta = 2a^2A = \frac{2a^2}{r}$$

or just double the approach due to the deformation *at* the contact surface.

The same holds good when the contact is between a ball and a flat surface the approach due to deformation at the contact only being $a^2/2r$ and that calculated from Hertz's functions for the approach of distant points in the two bodies being $2a^2/2r$.

Friction and Heat at the Contact Surfaces between Balls and Raceways of Journal Bearings.

Friction at Points along the Major Axis of the Contact Surface.—In order to calculate the heat generated by friction forces where the ball and raceway touch, we need to know definitely for each point along the major axis of the contact surface the distance through which relative movement of the two surfaces occurs, also the load stress at each point and, if possible, the coefficient of friction. As already stated there is generally relative movement along the arc of contact. This must be the case for the contact surface is not flat and parallel to the axis of rotation of the bearing. Some or all of the points on the ball contact surface will slip

relatively to the race contact surface. The maximum *amount* of slip per revolution of the ball about the axis measured in the direction of rotation, will be $2\pi(R_c - R_a)$ where R_c is the distance between the centre of the contact surface and the centre of the ball and R_a is the like for either end of the contact surface. This is 2 π times the height of the centre of the contact surface above its projection.

If there is no slip at the centre, the slip at all points along the arc of contact is in the same direction. If there is no slip at the ends, the slip elsewhere along the arc of contact is in the same direction, but the common direction in this case is opposite to that in the other. Consider a bearing of which the cage is stationary, the rings revolving at uniform speed in opposite directions (Fig. 12). If there be no slip at the centre of the contact

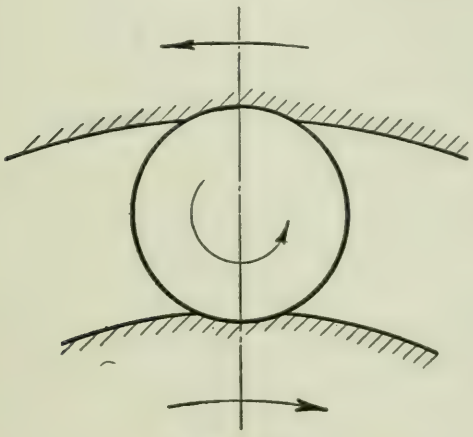


Fig. 12.

surface in the plane of the diagram, the forces at the contact surface in planes parallel to that of the diagram will tend to accelerate the ball.

If there be no slip at the ends of the arc of contact (above and below the plane of the diagram, Fig. 13), the friction in the planes between will be in a direction tending to retard the ball. But, by hypothesis, the ball has a constant angular velocity. It follows, therefore, that the resultant frictional couple is nil, and that the slip at each contact surface is such that the sum of the friction forces in the direction of rolling is (in vacuo) equal to the sum of the forces in the opposite direction, when the friction between the ball and the cage is zero. With regard to slip, the contact

surface will be divided into three zones by two lines. The outer two will be similar, and the slip at points in each will be in the same direction. The zone between them including the centre is the part where the load stresses, and therefore the friction, are greatest. All points in it will slip in the same direction and opposite to the former. Between the inner and the outer two zones there will be lines or bands, the width of which will depend upon the difference between the coefficient of sticking and sliding friction for the surfaces in question, and on the rigidity of the material of the ball and race. These bands the author proposes to call non-slip bands (with apologies to Messrs. Ewing and Rosenhain). They are quite discernible on journal bearings which

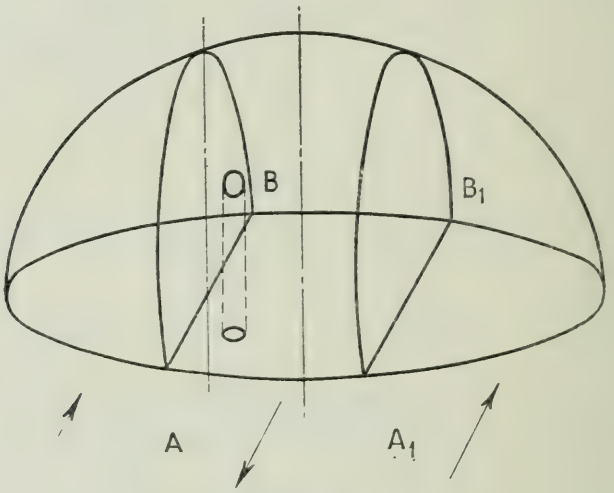


FIG. 13.

have been overloaded, and appear as narrow uncoloured parallel bands running between three wider brown or blue strips. As the heat developed at any point in the contact surface as it passes from one side to the other of that surface depends upon its position relative to the non-slip bands, it becomes necessary to determine their position on the major axis. To do this, what we have to find is the integral of the frictional forces multiplied by their respective arms (or the distance from the axis of revolution of the ball) for two equal outer zones which is equal to a similar integral for the inner zone. The mean arm for the forces between the non-slip bands is slightly greater than that for the outer zones, but since the contact surface is very small relative to the radius of the ball, we will assume that the mean arms are the same for both.

In Fig. 13, AB, A^1B^1 represent the projection of the non-slip bands traversing the projection of the contact surface. Consider a small element of the contact ellipse area $\triangle A$ over which the average pressure intensity is p. Then $p \triangle A$ is the pressure over the element and $\mu p \triangle A$ is the friction due to slip.

The condition that there shall be no resultant couple due to friction is (assuming the friction between the ball and the cage to be nil):—

$$\Sigma \mu p \triangle A = 0. \quad \therefore \mu \Sigma p \triangle A = 0.$$

Now numerically $\Sigma p \triangle A$ is the volume of the space bounded below by the contact ellipse and above by the surface, the ordinates of which represent the load stress at corresponding points on the contact ellipse. According to Hertz the upper surface is an ellipsoid.

The friction due to slip at any part of the contact surface is proportional to the volume of that section of the pressure ellipsoid which is bounded by lines drawn upwards from the periphery of the part of the contact surface in question to the surface of the pressure ellipsoid. What we have to find, therefore, is the volume of a section of the pressure ellipsoid between two planes parallel to, and equidistant from, the minor axis of the contact ellipse, which is one-half the total volume of the ellipsoid. The problem is simplified by the fact that the positions of the two planes will be the same as if the ellipsoid were the half of a sphere, for by projecting a circle we do not alter the relative areas of its parts.

For a hemisphere, radius R the volume of a thin slice distance x from centre and dx thick $= \frac{1}{2}\pi r^2 dx = \frac{\pi}{2}(R^2 - x^2)\, dx.$

Total volume of hemisphere

$$= 2\int_0^R \frac{\pi}{2}(R^2 - x^2)\, dx.$$

$$= \left[\pi R^2 x - \frac{\pi x^3}{3}\right]_0^R$$

$$= + \pi R^3 - \frac{\pi R^3}{3} = \frac{2}{3}\pi R^3$$

To find the position of the non-slip bands put

$$\left[\pi R^2 x - \frac{\pi x^3}{3}\right]_0^x = \frac{1}{3}\pi R^3$$

Then $x^3 - 3R^2 x + R^3 = 0.$

Whence $x = 0{\cdot}348$ R.

Non-slip bands are $\pm 0{\cdot}348\,a$ from the centre of the contact ellipse measured along the major axis, a being the semi-axis.*

Work done against friction due to Slip at Contact Surface.—Fig. 14 represents a ball rolling in a direction normal to the plane of the diagram.

Fig. 15 represents the arc of contact radius ρ_2

$AN = a$, the semi-axis.

$NB = 0{\cdot}348\,a$.

$OC = \rho_2$

D, D^1 are the non-slip bands.

The distance $CK = \dfrac{(0{\cdot}348a)^2}{2\rho_2}$ approximately.

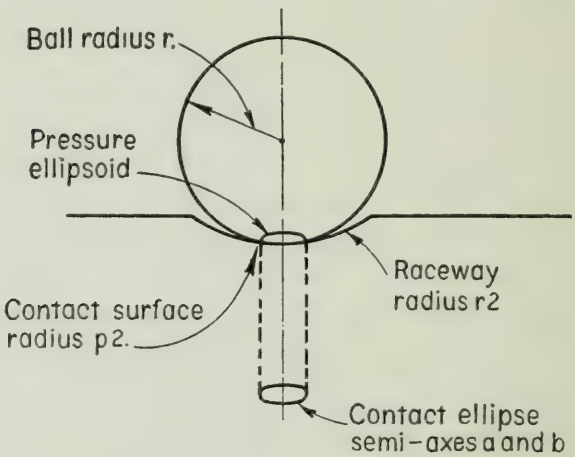


FIG. 14.

Therefore the slip at the centre C per 1 revolution of the ball

$$= \frac{2\pi(0{\cdot}348)^2}{2}\,\frac{a^2}{\rho_2}\dagger$$

$$= \frac{0{\cdot}1211\pi^2}{\rho_2}$$

We will take for our co-ordinate axes CO and the tangent at C. Consider a typical point (x, y) on the arc of contact. Its distance from DD^1 or DD^1 produced

$$= \frac{0{\cdot}1211a^2}{2\rho_2} - \frac{x^2}{2\rho_2}$$

* This has been independently proved by Palmgren (*Engineering*, February 28th, 1919); but as pointed out in *Engineering*, April 4th, 1919, p. 450, the effect of friction due to the cage is ignored.

† For simplicity the effect of the radius of curvature r_1 of the raceway is omitted, which amounts to a tacit assumption that it is infinity.

$\therefore$ slip at point $x, y, =$

$$\frac{2\pi}{2\rho_2}\ (0{\cdot}1211a^2 - x^2)\dagger$$

per 1 revolution of ball.

If the friction were constant during the passage of an element area $\triangle$ A across the contact surface, the work done against friction in crossing the contact surface would be proportional to

$$\mu p_x \triangle \mathrm{A}\ \frac{\pi}{\rho_2}\ (0{\cdot}1211a^2 - x^2)$$

where p_x is the pressure intensity over the element and $\Sigma p_x \triangle \mathrm{A} = p_x \Sigma \triangle \mathrm{A} = p_x \times$ width of contact surface at distance $x \times \delta x$, and would be represented geometrically by a slice of rectangular

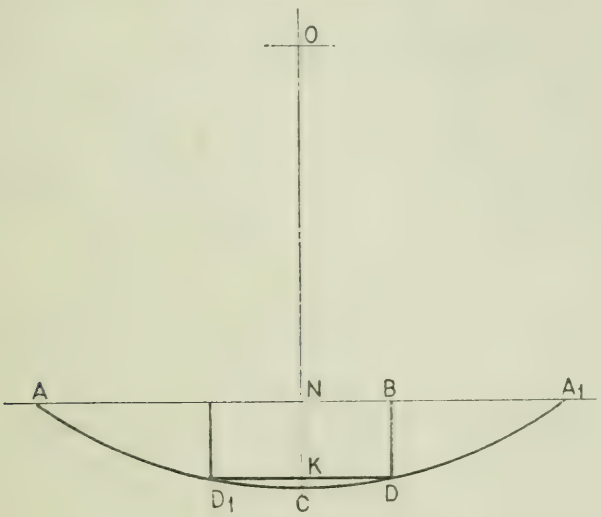


FIG. 15.

block of a thickness equal to δx and width equal to the width of the contact surface at distance x from its centre and height equal to p_x.*

During contact, however, the pressure at any point is not constant—even along the centre of the contact surface. It increases from zero, passes through a maximum and decreases to zero again

* If this be not obvious it will be apparent from the following :—

Work $\propto \mu p_x \triangle \mathrm{A} \times$ actual distance of slip.

$\therefore$,, $\propto \mu p_x \triangle \mathrm{A} \times$ rubbing velocity $\times$ duration of contact.

$\therefore$,, $\propto \mu p_x \triangle \mathrm{A} \times \frac{\pi}{\rho_2} (0{\cdot}1211\, a^2 - x^2) \times$ width of contact ellipse.

$\therefore$,, $\propto \mu p_x \times$ width $\times$ total slip per 1 ball rev. $\times \delta x$.

as the ball rolls on. This is true, of course, for points on both the ball and the race. The pressure intensity at any point in the contact surface is given by the height, above the point in question, of the pressure ellipsoid, and according to Hertz is represented by the equation—

$$p_{xy} = \frac{3P}{2\pi ab}\sqrt{1 - \frac{x^2}{a^2} - \frac{y^2}{b^2}}$$

where P is the load on the ball, and a and b are the semi-axes of the contact ellipse.

This is a surface the intersection of which with normal planes through the axes of the contact ellipse is an ellipse. The semi-axes of the ellipse formed by the intersection of the normal plane containing b are b and p_m $(= 3P/2\pi ab)$ the intensity of the pressure on the contact ellipse where it is greatest.

Planes parallel to that through b also cut the pressure ellipsoid in an ellipse (E) (or, more precisely, a semi-ellipse).

If the distance of the plane from the centre be $f\,a$ measured along the major axis of the contact ellipse, by substituting this value for x in the equation to the contact ellipse

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$$

we get the semi-axes of the semi-ellipse (E), namely—

$$b\sqrt{1 - f^2} \text{ and } p_m\sqrt{1 - f^2}$$

Now the area of this semi-ellipse is—

$$\frac{\pi}{2}\,bp_m(1 - f^2)$$

$$\text{and } \mu\,\frac{\pi}{2\rho_2}\,bp_m(1 - f^2)\,\delta x\,(0{\cdot}1211a^2 - x^2)$$

is proportional to the work done against friction forces in crossing the contact surface distance x from the centre. Leaving out factors which are independent of x and substituting $f = x/a$, work varies as

$$(a^2 - x^2)\,(0{\cdot}1211a^2 - x^2).$$

Therefore work $\propto 0{\cdot}1211a^4 - 1{\cdot}1211a^2x^2 + x^4$ for constant values of ρ_2 and δx. For finite values of a, work is zero when $\rho^2 = \infty$, i.e., when the contact surface is flat.

Differentiating this expression with respect to x, we find that it will be a maximum or minimum when $4x^3 - 2{\cdot}2422\,a^2x = 0$,

i.e., *when* $x = 0$, or if x is not $= 0$,

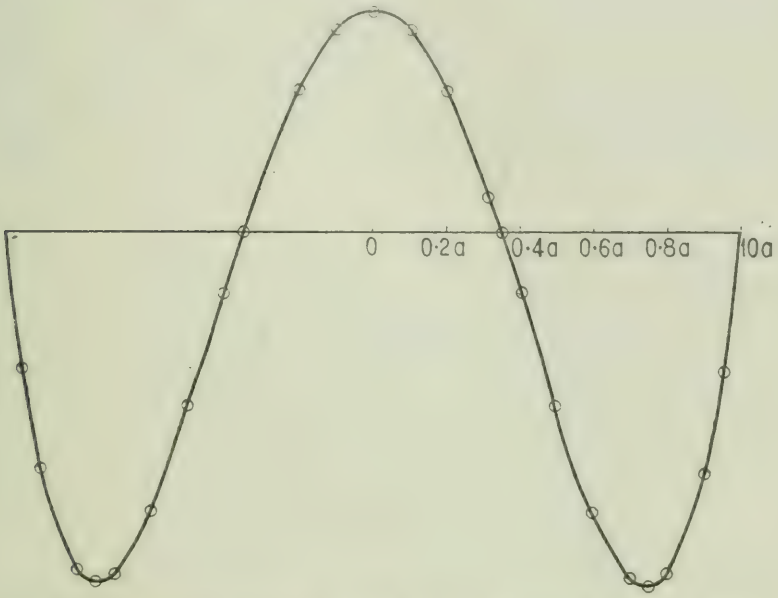
when $4x^2 = 2{\cdot}2422\,a^2$,

$\therefore$ when $x = \pm\,0{\cdot}7487\,a$,

$= \text{say } \pm\,0{\cdot}75\,a$.

Fig. 16 is a graph drawn by plotting the value of $y = 0{\cdot}1211\,a^4 - 1{\cdot}1211\,a^2x^2 + x^4$ for different values of x/a.

Experimental Results.—Measurement of the distance between the coloured bands on the raceways of Type 345 bearings tested under about $3\frac{1}{2}$ times the listed limiting safe load, shows a satisfactory agreement with that calculated from the above expression. By measurement (see Fig. 17)—



Curve showing relation between work done against frictional forces at various positions on the track (and ball) measured parallel to the axis of the bearing—a being the semi-axis of the ellipse bounding the contact surface (in the same direction).

FIG. 16.—$y = 0{\cdot}1211a^4 - 1{\cdot}1211r^2x^2 + x^4$.

$$\frac{DD^1}{FF^1} = \begin{matrix} 0{\cdot}327,\ 0{\cdot}312,\ 0{\cdot}348, \\ 0{\cdot}40,\ \ 0{\cdot}40,\ \ 0{\cdot}408, \end{matrix}$$

average 0·366, by calculation 0·348,

$$\text{and } \frac{EE^1}{FF^1} = 0{\cdot}76,\ 0{\cdot}725,\ 0{\cdot}80,\ 0{\cdot}72,\ 0{\cdot}815,\ 0{\cdot}76,$$

average 0·76, by calculation 0·75.

The assumption underlying the calculation of the amount of slip involves, in the case of the comparatively long arc of contact developed in this test, an error of about 2 per cent.

Position of Non-slip Bands is affected by Forces between Balls and Cage.—The agreement is sufficiently close to show that for the

bearings and tests in question, the couple tending to retard the rotation of the ball about its axis, due to friction at the surface of the ball where it touches the ball cage, is small compared with that due to the frictional forces at the contact surfaces. This, however, may not always be the case. If the balls in a bearing vary in size even ever so little, the larger ones will tend to complete their circuit round the axis of the bearing more quickly than the smaller ones. The faster-running ones will tend to accelerate the cage and the cage will tend to drag the slower-running ones after it. In such a case the couple due to friction forces at the ball cage contacts may be considerable. The effect of any such couple is to move the non-slip bands nearer together, for only in this way

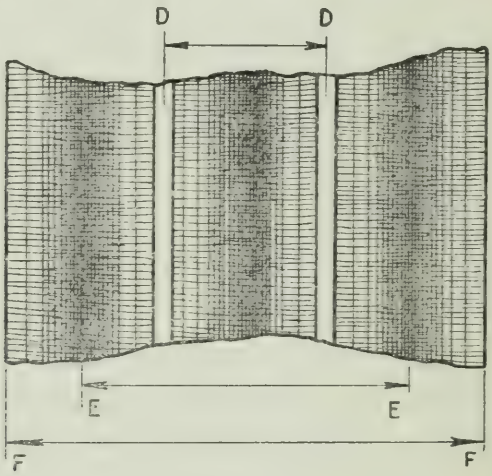


FIG. 17.

can a couple be provided which will maintain uniform velocity (see Fig. 12). A glance at the heat curve (Fig. 16) shows that the result will be a greater evolution of heat at the ends of the "arc of contact," and from the expression for the work done against friction forces it is clear that the total work done will be greater.* Hence the heat generated at each contact surface will be greater. Even this does not quite exhaust the question, for if one ball tend to accelerate the cage and another to retard it, in order to ensure the uniform angular velocity pre-supposed, it follows that both balls must be acted upon by friction forces, the sum of which for each ball is equal and opposite to the retarding or accelerating

* A proof of this is given at the end of this section.

force. For either ball these forces must reside in the contact surface, and they act in the same direction as one another. The result on the position of the non-slip bands will be to make them nearer together at the contact between a ball and one race and further apart at the contact between the same ball and the other race. No measurement of this effect can be made, because the widening on, say, the inner track at one ball contact is obliterated by the narrowing at the other ball contact. The nett result is to obscure the definition of the non-slip bands.

Since the slightly larger balls will develop more heat at their contact surfaces, the measurements of the non-slip and heat bands will give results more in agreement with the conditions affecting the larger than the smaller balls.

The area of the curve is proportional to the total heat due to friction developed as the contact surface goes past a point on the ball or the raceway. The temperature attained at different points in the contact surface does not necessarily follow the same curve for the rate at which the heat is generated will depend upon the position along the major axis. This will affect the two side zones more than the middle zone, and tend to make their temperature higher than the heat curve suggests.

Nature of the Coloured Film produced at Contact Surfaces.—That the temperatures attained at the contact surfaces of overloaded bearings must be considerable is, the author thinks, proved by the colouring of the surface, which becomes brown, blue, and sometimes a distinctly reddish colour such as is associated with the sequi oxide of iron Fe_2O_3. Corrosion has been noticed on parts subjected to alternating load, *vide* Eden, Rose and Cunningham, Proc. I. Mech. E., 1911, 4, page 875, who say with regard to the formation of what they call red rust, where specimens subjected to the Wöhler test were held: "There seems to be no doubt that this corrosion is due to the varying stress between the test specimen and its holder, and the authors believe that the rust often found between a wheel and its shaft, or a key and key-way, and the corrosion of ball bearings is usually due to a similar action and not only to the presence of moisture." . . . "Rust" was still produced when the authors' specimens were oiled before assembling.

Some experiments the author carried out some years ago (*vide* "The Engineer," January 8th, 1912, page 80) led him to believe that the coating which formed when dry steel surfaces were subjected to repeated impact was not rust but oxide of iron—either

6 FeO, Fe_2O_3, or a mixture of oxides having a composition intermediate between FeO and Fe_2O_3. Some particles that he obtained were non-magnetic and quite red, and most probably fairly pure Fe_2O_3.

The temperature at which these oxides form at the rate they do, on parts subjected at the same time to considerable pressure at their contacting surfaces, and to rapid and rapidly repeated relative movement of the contacting parts, must be considerably above normal—probably 125° C. They are not formed at ordinary temperatures, in fact, a prolonged red heat is said to be required to fully oxidise iron to Fe_3O_4 (*vide* Mosander, Pogg. Ann. 6, 35).

The explanation the author puts forward of their formation at the contact surfaces of ball and raceway, and elsewhere, is that under the conditions just stated, heat is developed there either by the friction due to slip or to the more or less adiabatic compression of the material near the contact surface or to both, and owing to the shortness of the interval of time during which the slip and compression occur at and near any point, the heat evolved can and does raise the surface layer of the steel to a high temperature before it is conducted away by the underlying layers. There is no need to pre-suppose a film of oxygen or air between the contacting parts. The author has shown elsewhere (*vide* Journal of Soc. of Chem. Industry, 31st August, 1907), that air is dissolved in steel and given off when steel is exposed to high vacua.

Internal Stresses due to Alternating Load.—Whatever be the correct explanation, we shall have no hesitation in accepting it as a fact that temperature rise, possibly of considerable magnitude, occurs at the contact surfaces of a ball and the cage and a ball and its raceway, and that the temperature at a point on either a ball or its raceway will fluctuate as it comes into, and passes out of, a contact surface.

As the bearing revolves, most if not all of its parts undergo a stress cycle. As a result, each element of volume will vary in temperature—even whilst the stresses remain within the elastic range. Some of the volume elements will be in highly stressed zones and in contact with a much greater bulk of relatively unstressed material. Conduction from one to the other will ensue, compression and recoveries will not be adiabatic, work will be done on the stressed parts and more power will be required to drive the bearing.

Consider the track of the revolving member. Each point of

the raceway is momentarily heavily stressed, and then, for a longer period, is less stressed or even oppositely stressed. Soon another ball will traverse the same point and the cycle of stress changes will be repeated, their magnitude, in the case of a journal bearing, being either greater or less according to whether the point is travelling towards or away from the position where the load stress is greatest. A temperature wave will start from such a point at each cycle and travel in all directions. While a bearing is revolving, therefore, the temperature at a point within and near the raceway is alternately higher and lower. Furthermore, as the point is moved radially away from the surface of either ball or raceway, it will pass across zones or shells of alternately higher and lower temperature. Such alternations of temperature will induce corresponding alternations of stress at every point on and near the surface, which must, as a first approximation, be added to the stresses due to load, flexure, slip against frictional forces, and the inertia of rotation in order to obtain the total stress at the point in question.

We can assume that the main effect of these thermally induced forces will be to shear the material parallel to the wave front, and that the shear stress will be proportional to the maximum difference of temperature between two adjacent waves, and inversely proportional to the wave-length.

Effect of Speed.—The number of wave fronts started in any time interval t is, obviously, proportional to the revs. per minute, N, but it does not follow that the wave length is inversely proportional to N. If the conduction of heat from one point to another were independent of the temperature distribution in the body in question, the wave length would be independent of the frequency N. Bearing in mind the fact that an increase in N results in there being more points in the body from which a given point may receive heat in a given time t, it becomes obvious that the rate at which the temperature at a point changes will be affected by the frequency, and in such a sense that the greater the frequency, the greater the rate at which the temperature, if relatively low, will increase. In other words, the forward velocity of a wave front will increase with the frequency.

According to Clerk Maxwell (Theory of Heat, page 265), the length of such waves is proportional to the square root of the periodic time,

or $\lambda = \frac{1}{\sqrt{N}}$ and the velocity of propagation will vary as $\sqrt{N}$.

Applied to the ball bearing, this means that the shear stress due to these heat waves is proportional to $\sqrt{N}$. If, therefore, a particular bearing is run at two different speeds, and if the shear stresses induced thermally are the controlling factor in determining the life of a bearing, steps must be taken to ensure that the said shear stresses are reduced as the speed increases. If the shear stresses are, ceteris paribus, proportional to the load, the shear stresses will be the same if

$$\frac{\text{Load } (n)}{\text{Load } (1)} = \sqrt{(N_1)/(N_n)}.$$

How the currently recommended limiting safe loads have been arrived at the author does not know, but it is interesting to note that for thrust bearings of the same size, it is usual to reduce the load precisely according to the above formula.

The limiting safe loads currently recommended for journal bearings of the same size are according to the formula

$$\frac{\text{Load } (n)}{\text{Load } (1)} = \sqrt[3]{(N_1)/(N_n)}.$$

In this case the recommendations suggest that the effect of speed on the safe load is considered to be less for a journal bearing than for a thrust bearing.

Other Effects of Heat Stresses.—There are two corollaries which seem to be worthy of consideration:—

1. Since increasing the frequency brings the wave fronts nearer to the surface of contact, the latter will receive more heat from the former, the faster the bearing revolves. Consequently the temperature attained at the surface when heat is generated by friction and elastic compression will be higher the greater the speed.
2. A minute flaw, cavity, impurity or slip band, the sides of which become rapidly stressed either by the fluctuating shear stresses thermally induced or by the adiabatic stress variations due to the fluctuating load, will become the seat of the evolution of heat.

Now if the conductivity of the particle in question be less than that of the surrounding material, the heat will not escape so rapidly, and the succeeding cycle will raise its temperature above that of the homogeneous part near to it. In short, it will rise in temperature for much the same reason that the contact surface does. This will be true wherever there is a double surface as at the contact surface itself, and at a slip band (the genuine thing, this time).

Moreover, we may expect the escape of heat to be very slow for particles that are exceedingly minute. It is not difficult to show that the *electric* resistance round a *point* is infinitely great.* Round a small spherical particle radius a, the resistance to flow of heat is approximately proportional to $\sigma . a$ where σ is the specific thermal resistivity of the steel surrounding the particle. Evolution of heat will occur at every fluctuation of the stress which does work on the particle, and it will be rapid. Its temperature will rise, and, owing to the rate at which it loses its heat being less than that at a correspondingly sized particle of sound steel, it will become higher than that of the surrounding homogeneous material. Such a particle, therefore, will become the seat of very high forces, tending to rupture its steel envelope. It is indeed conceivable that chemical combination may occur, e.g., between oxygen dissolved in the steel and the steel surface of such a particle, possibly with the result that the bursting forces are much increased, such as occurred at the contact surface between the test piece and holder in the experiments of Eden, Rose and Cunningham. Moreover, many kinds of particle will have the same experience, gaseous inclusions less so than liquid and solid inclusions. From this point of view, a ball becomes a medium in which heat is generated, principally at and near its outside surface, in such a manner as to flow inwards in alternating waves of temperature, producing alternations of stress of high frequency, also heat segregations and radial compressive stresses centreing round all flaws and other discontinuities in the medium. Its state is akin to that of the universe, traversed as it is by electro-magnetic vibrations of high frequency which result in the evolution of heat where they impinge on those discontinuities of the inter-stellar medium we know as matter.

Moreover, it seems probable that these accumulations of heat and the resulting internal stresses go far to explain the decay of elasticity observed by Lord Kelvin; also the effect of rest (during which time is allowed for conduction of heat away from hot nuclei),

* For a small area, e.g., a circle having a radius a on the surface of a larger sphere, the total resistance is $\tau/4a$, τ being the specific electric resistance of the material of the sphere. Since thermal conduction follows the same laws as electric conduction, the heat conducted away from a hotter part of the surface of a sphere is nil when the area of that part is infinitely small. Hence we may expect slow conduction from a small contact surface that is slipping or spinning.

in restoring elasticity and strength to specimens which have been subjected to alternating stresses.

It is time, however, to leave this corollary and return to earth.

The practical upshot of the matter is that heat is evolved at contact surfaces at a sufficient rate to raise the surface layers to a quite considerable temperature. It is conceivable, in fact probable, that bearings—particularly those that run at a high speed—fail, not because the load stress is unduly high, but because of the temperature the contacting surfaces attain, and the shear stresses that ensue.

Effect of varying the Number and Radii of Balls and Raceways on the Heat and Heat Stresses.—We will assume that the load and speed have to be maintained, and enquire into the result of—

(1) reducing the value of the expression $R_o/(R_o + R_i) \times R_i/R$.

(2) reducing the number of balls in the bearing.

(3) reducing the length of the major axis.

(1) Where space permits we can increase the radius R of the ball at the expense of the inner and outer track radii R_i and R_o respectively.

For R_o we will write $n\,R$

and for R_i „ $m\,R$

then $\dfrac{R_o}{R_o + R_i}\dfrac{R_i}{R} = \dfrac{nR}{R(n+m)}\dfrac{mR}{R} = \dfrac{nm}{(n+m)}$.

Assuming that the inner and outer track are encroached upon to the same extent, $n + m$ is constant, and the expression becomes proportional to nm, which is a maximum when $n = m$ and the balls have an infinitely small radius, and a minimum when the balls are very large. Increasing the ball radius will, of course, diminish the number of balls and so reduce the number of heat evolutions per revolution and the shear stress. On the other hand, unless the radius of curvature of the raceways is considerably increased, the "arc of contact" will be increased, and this will increase the heat evolved considerably.

(2) *Effect of Reducing the Number of Balls in the Bearing.*—To find the work done against friction forces in crossing the contact surface, and so obtain an expression proportional to the total heat evolved at all points which cross that surface, we integrate the expression $\mu\frac{\pi}{2\rho_2}bp_m\ (1-f^2)(x^2 - 0{\cdot}1211\,a^2)\,dx$ between the limits $x = +a$ and $x = -a$.

Substituting $f = \frac{x}{a}$ we have

Heat

$$\propto 2 \int_{x=0}^{x=a} \mu \frac{\pi}{2\rho_2} b p_m \frac{1}{a^2}(a^2 - x^2)(x^2 - 0{\cdot}1211a^2)dx$$

$$\therefore \propto -\frac{\mu\pi b p_m}{\rho_2} \int_o^a \frac{1}{a^2}(0{\cdot}1211a^4 - 1{\cdot}1211a^2x^2 + x^4)dx$$

$$\therefore \propto -\frac{\mu\pi b p_m}{a^2\rho_2}\left[0{\cdot}1211a^4x - \frac{1{\cdot}1211}{3}a^2x^3 + \frac{1}{5}x^5\right]_o^a$$

$$\therefore \text{Heat} \propto \frac{\pi b p_m a^3}{\rho_2}$$

or $a^2 \times$ area contact ellipse $\times$ maximum value of load stress $\div$ radius of ball

when r_2 for race is a constant times r for ball.

Since a^2/ρ_2 is proportional to the greatest distance between the contact surface and its projection, the heat evolved at a journal bearing contact is proportional to the load on the ball multiplied by the maximum distance of the contact surface from its projection.

Suppose, now, that the number of balls be halved, the load L and the revs. per minute remaining the same.

Then load per ball is doubled, consequently b and a are increased $\sqrt[3]{2}$ times.

$\therefore$ Heat, in the altered bearing, per contact, is—

$$2 \times \sqrt[3]{2^2} \text{ times as great.}$$

The number of contacts per minute is halved,

$$\therefore \text{Total Heat is } \sqrt[3]{4} \text{ times as great.}$$

If shear stress is proportional to heat per contact and to $\sqrt{\text{Frequency}}$

Then shear stress is $\sqrt{2}\ \sqrt[3]{4}$ times as great.

We infer, therefore, that reducing the number of balls in the bearing offers no advantages.

(3) *Effect of Reducing the Length of the Major Axis.*—This, of course, is to be effected by increasing the radius of curvature r_2 of the track.

$$\text{Heat per contact} \propto \frac{a^2}{\rho_2} \times \text{load on ball.}$$

In this case the load per ball will remain constant, and the effect

of reducing a to one-half its former value will be to reduce the heat per contact to less than one-quarter of its former value.

Consequently, when the heat generated at the contact surfaces of a journal bearing is the cause of failure, the remedy is to be sought by increasing the radii of curvature r_2 of the raceways.

Case of Journal Bearing.—To find the work done per second due to friction over the whole area of contact.

Consider the element, area $dydx$, load stress p_1

$$\text{friction} = \mu p_1 \, dy \, dx.$$

Work per second due to friction forces on this element

$$= \mu p_1 \, dy \, dx \times \text{relative velocity at } x.y.$$

When discussing the distribution of heat along the major axis and finding an expression proportional to the total heat evolved, it was tacitly assumed that the relative velocity at the point (xy) was independent of the velocity of the raceway. This assumption does not invalidate the results obtained, but it will not do if we want to calculate the actual heat evolved at a journal bearing contact.

In a contribution to the discussion which followed Mr. Barrett's paper,* Mr. J. V. Pugh included a Table showing the total amount of slip per ball revolution. For the journal bearing in question the total slip at the contact of the ball and inner ring was 0·01285, and at the contact with the outer ring (for an assumed equal length of arc of contact) 0·00676. That the slip per second varies inversely as the radius of the ball race in the plane of revolution follows from a consideration of Fig. 12.

For let ω be the angular velocity of the ball of radius r to the non-slip bands, and ω_o and ω_i the angular velocities of the outer and inner rings, radii r_o and r_i to the non-slip bands.

$$\text{then } \omega_o = \omega \frac{r}{r_o} \text{ and } \omega_i = \omega \frac{r}{r_i}$$

Velocity of ball at the centre of the contact surface, major axis a, radius of curvature ρ_2

$$= \omega \left(r + \frac{0{\cdot}1211a^2}{2\rho_2} \right)$$

Velocity of outer race at centre of contact surface

$$= \omega_o \left(r_o + \frac{0{\cdot}1211a^2}{2\rho_2} \right) = \omega \frac{r}{r_o} \left(r_o + \frac{0{\cdot}1211a^2}{2\rho_2} \right)$$

* See Proc. I.A.E., Vol. VI., p. 267.

$\therefore$ Relative velocity at the centre of the contact surface

$$= \omega r + \frac{\omega 0{\cdot}1211a^2}{2\rho_2} - \omega r - \frac{\omega r}{r_o}\frac{0{\cdot}1211a^2}{2\rho_2}$$

$$= \frac{\omega 0{\cdot}1211a^2}{2\rho_2}\left(1 - \frac{r}{r_o}\right)$$

For the Inner Ring.—At centre of contact surface

$$\text{velocity on ball} = \omega\left(r + \frac{0{\cdot}1211a^2}{2\rho_2}\right)$$

$$\text{,, ,, race} = \omega_i\left(r_i - \frac{0{\cdot}1211a^2}{2\rho_2}\right)$$

$$= \omega\frac{r}{r_i}\left(r_i - \frac{0{\cdot}1211a^2}{2\rho_2}\right)$$

$\therefore$ Relative velocity at centre of contact surface

$$= \omega r + \frac{\omega 0{\cdot}1211a^2}{2\rho_2} - \omega r + \frac{\omega r}{r_i}\frac{0{\cdot}1211a^2}{2\rho_2}$$

$$= \frac{\omega 0{\cdot}1211a^2}{2\rho_2}\left(1 + \frac{r}{r_i}\right)$$

Comparing the relative velocities at the centres of the outer and inner ring contacts, we have

$$\text{relative velocity}\ \frac{\text{inner}}{\text{outer}} = \frac{1 + \frac{r}{r_i}}{1 - \frac{r}{r_o}}$$

$$= \frac{r_o}{r_i}\frac{r_i + r}{r_o - r} = \frac{r_o}{r_i}$$

Consider now a typical point co-ordinates $(x.y)$—see Fig. 15—between D and A[1].

The radius to point xy on ball $= r + \frac{0{\cdot}1211a^2}{2\rho_2} - \frac{x^2}{2\rho_2}$

$\therefore$ Velocity at $xy = \left(r + \frac{0{\cdot}1211a^2}{2\rho_2} - \frac{x^2}{2\rho_2}\right)\omega$

For outer raceway velocity at $xy = \omega_o\left(r_o + \frac{0{\cdot}1211a^2}{2\rho_2} - \frac{x^2}{2\rho_2}\right)$

$\therefore$ Relative velocity at xy

$$= \omega r + \frac{\omega}{2\rho_2}(0{\cdot}1211a^2 - x^2) - \frac{\omega r}{r_o}\left(r_o + \frac{1}{2\rho_2}(0{\cdot}1211a^2 - x^2\right)$$

$$= \frac{\omega}{2\rho_2}(0{\cdot}1211a^2 - x^2)\left(1 - \frac{r}{r_o}\right)$$

Similarly, for inner ring, the relative velocity is

$$\frac{\omega}{2\rho_2}(0{\cdot}1211a^2 - x^2)\left(1 + \frac{r}{r}\right).$$

Consequently, the total slip per second at each race is inversely proportional to the track radius (in the plane of rolling) and is much greater at the inner than at the outer ring contact.

The rate of heat evolution for equal area of contact will also be, approximately, inversely proportional to the track radii, and much greater at the inner than at the outer ring contact.

In the case of bearings run under conditions in which there is a sensible evolution of heat—and probably also when the heat is but slight—it would be preferable to make the radius r_2 greater for the inner than for the outer ring and not less as is recommended by some ball bearing manufacturers.

The results of running tests on heavily loaded bearings in which r_2 was the same for both races, indicate that the raceway of the inner ring *is* more heated than that of the outer. In such a case

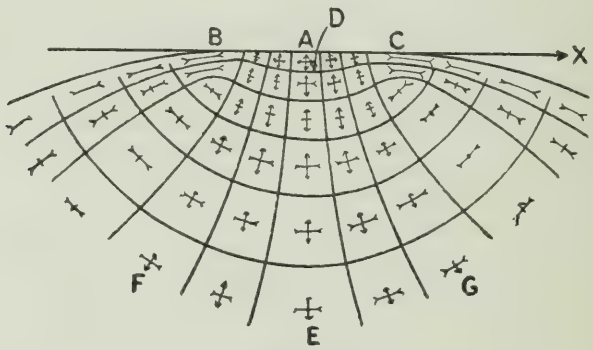


FIG. 18.

the effect of the heat will be to cause expansion and tend to put compression on the balls.

If by adjusting the radii of curvature more heat is evolved at the outer than at the inner ring, the balls will not become compressed owing to expansion, and such a bearing may be expected to run better under trying conditions.

Reverting to the expression: Work per second due to friction forces on element area $dy\,dx = \mu p_1\,dy\,dx \times$ relative velocity at xy.

We can now substitute relative velocity at a point distant x from the centre of the contact surface

$$\text{for outer race} = \frac{\omega}{2\rho_2}(0{\cdot}1211a^2 - x^2)\left(1 - \frac{r}{r_o}\right)$$

$$\text{and ,, inner ,,} = \frac{\omega}{2\rho_2}(0{\cdot}1211a^2 - x^2)\left(1 + \frac{r}{r_i}\right)$$

$$\therefore \text{ W}_x \text{ per sec.} = \frac{\pi\mu b 3\text{P}}{2\pi ab}\frac{1}{a^2}(a^2 - x^2)\frac{\omega}{2\rho_2}(0{\cdot}1211a^2 - x^2)\frac{r_i + r}{r_i}dx$$

(at x on inner race).

$$\text{W}_i \text{ per sec.} = \frac{3}{4}\frac{\mu\text{P}}{a^3\rho_2}\frac{\omega(r_i + r)}{r_i}\int_{x=-a}^{x=+a}(a^2 - x^2)(0{\cdot}1211a^2 - x^2)dx$$

(for whole contact on inner race).

$$= \frac{3}{2}\frac{\mu\text{P}}{a^3\rho_2}\frac{\omega(r_i + r)}{r_i}\int_{o}^{a}(a^2 - x^2)(0{\cdot}1211a^2 - x^2)dx$$

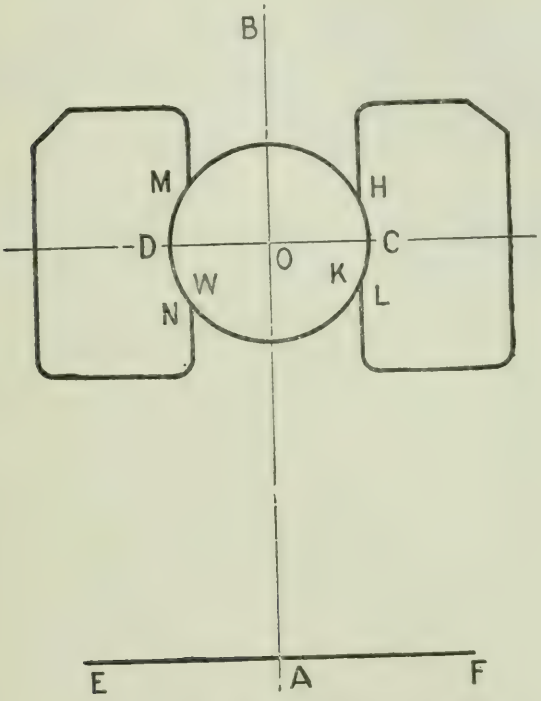


FIG. 19.

The form of the expression under the integral is shown in Fig. 16, and has positive and negative values according to the direction of the relative velocity. To find the numerical value of the total friction work done per sec. we must therefore integrate this expression from $x = 0$ to $x = 0{\cdot}348\,a$ and from $x = 0{\cdot}348\,a$ to $x = a$, and affix to each the sign which will ensure that value.

The result is W_i per sec. $= \frac{3}{2}\frac{\mu\text{P}}{a^3\rho_2}\frac{\omega(r_i + r)}{r_i}(0{\cdot}1075)a^5$

(for whole contact on inner race).

$$= 0{\cdot}1612\,\mu\text{P}\frac{\omega}{}\frac{(r_i + r)}{r_i}\frac{a^2}{\rho_2}$$

If for $0{\cdot}345\,a$ we write fa where f may have any value between o and unity, and after carrying out the integration we differentiate with respect to f, it is easy to see that the value of W_f is a minimum when $x = 0{\cdot}348\,a$ and that it is about one and a quarter times as great when $f = o$ and the non-slip bands are at the centre of the contact surface, and about five times as great when the non-slip bands are at the ends of the arc of contact.

Friction and Heat at the Contact Surfaces of Ball and Raceways of a Thrust Washer.

Consider the case when the two races revolve at equal speeds in opposite directions. The centre of the ball, Fig. 19, will then remain stationary, and the ball will revolve about an axis AB through its centre O and at right angles to the axis EF of the bearing.

Draw CD through the centres of the contact surfaces.

Let ω be the angular velocity of the ball, the effective radius OC of which $= r$, and let OA the axis about which the centre of the contact surface on the race revolves be R, referred to axes intersecting at the centre of the contact surface, x being measured parallel to the axis AB.

Then the velocity of any point xy in the contact surface of the ball centre C is $\omega y = \omega\left(r - \frac{x^2}{2\rho_2}\right)$ where ρ_2 is the radius of curvature of the contact surface in the plane of the diagram.

In order to ascertain the angular velocity of the raceway, we need to know at what point or points along the major axis of the contact surface there is no slip between the ball and the raceway.

A consideration of Fig. 19 shows that the ball and the raceway can only have an equal velocity at the centre of the major axis.

In this respect, therefore, the thrust washer differs from the journal bearing, in which there are usually two points along the major axis where the ball and the race can and do have the same velocity.

We arrive at this conclusion by showing that the assumed motion of the ball is inconsistent with any other position. For instance, we will assume that there is one non-slip band at K between C and L. Then if this raceway has a clockwise rotation seen from F, the race moves, relatively to the ball, downwards normal to the plane of the diagram for the region H to K. For the zone LK the

relative movement is in the reverse direction. Similarly, the relative movement of the raceway at the other contact is upwards along MW and downwards along WN. Bearing in mind the distribution of load stress over the two contact surfaces and taking moments about the axis AB, it is clear that there will be a resultant couple tending to increase the angular velocity of the ball. But the ball, by hypothesis, has a constant angular velocity, ∴ the non-slip band cannot be between C and L nor can it be between C and H. A similar result follows if we assume two non-slip bands.

It follows therefore that there must be one at C, provided there is no force, e.g., due to friction of the cage on the ball which has a couple tending to oppose the angular rotation of the ball. Such a couple will have the effect of displacing the non-slip band from C towards EF.

Consider now the velocity of a point in the raceway at an infinitely small distance from a point xy on the ball contact surface.

Since the velocity of a point in the raceway at $C = \omega r$, the

velocity at xy on raceway $= \omega r \dfrac{R \pm x}{R}$.

For all positive values of x, the velocity on the ball is less than at C and on the race greater than at C. Therefore for such values of x, the raceway will exert a force on the ball tending to turn it in a clockwise direction about CD, as seen from C.

At the other contact surface the force at a similarly placed point will be equal and act in the opposite direction.

For negative values of x

$$\text{velocity on ball} = \omega\left(r - \frac{x^2}{2\rho_2}\right) = \omega r\left(1 - \frac{x^2}{2r\rho_2}\right)$$

$$\text{velocity on race} = \omega r\left(1 - \frac{x}{R}\right).$$

From inspection of these expressions it is obvious that if $x/(2r\rho_2)$ is less than $1/R$ the velocity on the ball is greater than on the race.

Therefore, on this side of C also, the raceway exerts a force on the ball tending to turn it in a clockwise direction about CD, seen from C.

It is obvious that a similar but opposite force acts upon the ball near D.

Relative Velocity of Ball and Raceway at Point xy in the Contact Surface.

For positive values of x

$$\text{Relative velocity} = \omega r_1 \frac{R + x}{R} - \omega r \left(1 - \frac{x^2}{2r\rho_2}\right)$$

$$= \omega r \left(\frac{x}{R} + \frac{x^2}{2r\rho_2}\right)$$

For negative values of x

$$\text{Relative velocity} = \omega r \left(1 - \frac{x^2}{2r\rho_2}\right) - \omega r \left(1 - \frac{x}{R}\right)$$

$$= \omega r \left(\frac{x}{R} - \frac{x^2}{2r\rho_2}\right)$$

The load stress along this axis will be given by putting $y = 0$ in the expression

$$p_1 = \frac{3P}{2\pi ab} \sqrt{1 - \frac{x^2}{a^2} - \frac{y^2}{b^2}}$$

The work done per second against friction forces at any point (xy) on this axis will be for positive values of x

$$Wx = \mu p_1 \times \text{relative velocity}$$

$$= \frac{\mu 3P}{2\pi ab} \sqrt{1 - \frac{x^2}{a^2}} \left(\frac{x}{R} + \frac{x^2}{2r\rho_2}\right) \omega r.$$

This expression for the work done is, obviously, zero when $x = a$ or $x = 0$, i.e., at the end and centre of the "arc" of contact. Since $x^2/(2r\rho_2)$ is very small relative to the other terms in the expression, we will, for the purpose of calculating the heat distribution, assume it $= 0$.

$$\text{then} \quad Wx = \frac{3}{2} \frac{\mu\rho}{ab} \frac{\omega r}{R} x \sqrt{1 - \frac{x^2}{a^2}}.$$

To find the point for which the expression is a maximum, we differentiate, with respect to x, and equate to zero. At maximum value

$$\frac{x}{2a^2} \frac{(-2x)}{\sqrt{1 - \frac{x^2}{a^2}}} + \sqrt{1 - \frac{x^2}{a^2}} = 0$$

$$\therefore \frac{x^2}{a^2} = 1 - \frac{x^2}{a^2}$$

$$\text{and } x = \frac{a}{\sqrt{2}} = 0{\cdot}707a.$$

Comparison of Maximum Rate of Evolution of Heat at Contact Surfaces of Journal and Thrust Bearings.

Assuming equal load and equal ball and race radii r and r_2, ρ_2 will be the same for both types.

The slip $\triangle s$ at a point xy for a journal bearing contact is $\triangle s = \frac{\pi}{f_2}(x^2 - 0{\cdot}1211a^2)$ per revolution of the ball. If the angular velocity of the ball be ω, the time required for 1 revolution $= \frac{2\pi}{\omega}$, therefore the relative velocity of the ball and the race at xy

$$= \frac{\frac{\triangle s}{2\pi}}{\omega} = \frac{\pi}{\rho_2}(x^2 - 0{\cdot}1211a^2)\ \frac{\omega}{2\pi} = \frac{\omega}{2\rho}(x^2 - 0{\cdot}1211a^2).$$

Heat is maximum at $x = 0{\cdot}75\,a$.

At this point relative velocity is $\frac{\omega}{2f_2}\left[(0{\cdot}75)^2a^2 - 0{\cdot}1211a^2\right]$

$$= \frac{\omega}{2f_2}\,0{\cdot}4414a^2$$

On a thrust bearing contact the relative velocity is $\frac{\omega r}{\mathrm{R}}\,x$, approximately.

Heat is maximum at $x = 0{\cdot}7a$.

At this point the relative velocity is $\frac{\omega r}{\mathrm{R}}\,(0{\cdot}7\,a)$ if the loads are such as to make the semi-axes equal to one another.

$$\frac{\text{Maximum Heat on Journal}}{\text{Maximum Heat on Thrust}} = \frac{\omega}{2\rho_2}\,\frac{0{\cdot}44a^2\,\mathrm{R}}{\omega r(0{\cdot}7a)}$$

$$= \frac{1}{3}\,\frac{\mathrm{R}a}{r^2} \text{ approximately.}$$

For Type W3 thrust washer $\frac{\mathrm{R}}{r} = 7$ approx.

therefore ratio $= \frac{7}{3}\,\frac{a}{r}$

therefore for values of $a = \frac{3}{7}\,r$ the ratio is about $= 1$.

if $a < \frac{3}{7}\,r$, the thrust washer develops more heat for a given load per ball.

Usually $a < \frac{3}{7}\,r$.

To find the total friction work at the contact surface in a thrust bearing.

Consider the case when the ball axis is fixed in space.

Relative velocity at the point xy on the contact surface which also is fixed in space is approximately $\frac{\omega r}{R} x$. Consider a small element, area $\triangle A$, of the contact surface co-ordinates x, y. As the ball spins about the centre of the contact surface (or as the race spins when the ball centre is stationary), the pressure on $\triangle A$, whether it be on a ball or raceway, varies as the height of the ordinate of the pressure ellipsoid. The heights will be approximately the same as those of the ordinates of the ellipse where the pressure ellipsoid cuts the plane $x = fa$ where f is a proper fraction.

Substituting in expression $p_1 = \frac{3P}{2\pi ab}\sqrt{1 - \frac{x^2}{a^2} - \frac{y^2}{b^2}}$

we have $p_1 = \frac{3P}{2\pi ab}\sqrt{1 - \frac{f^2a^2}{a^2} - \frac{y^2}{b^2}}$; when $y = 0$

semi-axis of this ellipse, $p_m = \frac{3P}{2\pi ab}\sqrt{1 - f^2}$

Similarly, the other semi-axis is found by putting $x = fa$ in the equation for the contact ellipse $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$

$$\text{i.e. } y_1 = b\sqrt{1 - f^2}.$$

The work done against friction forces at a point $x = fa$ on the ball or raceway surface in traversing the contact surface will be for each second

$$\text{relative velocity} \times \Sigma \mu p_1 \triangle A = \frac{\omega r}{R} x \, \mu \Sigma p_1 \triangle_x \triangle_y$$

In the limit when $\triangle A$ is very small $= dx\,dy$,

$$\text{work} = \mu \frac{1}{2} (\text{area of ellipse E}) \times \frac{\omega r}{R} x\,dx$$

$$= \mu \frac{1}{2} \frac{\pi 3P}{2\pi ab} b(1 - f^2) \frac{\omega r}{R} x\,dx = \frac{3\mu P}{4a}(1 - f^2) \frac{\omega r}{R} x\,dx$$

$$= \frac{\omega r}{R} \frac{3\mu P}{4a^3}(a^2 - x^2)\, x\,dx.$$

Work done for whole of contact surface per second is

$$-\int_{x=-a}^{x=a} \frac{3\mu P}{4a^3}(a^2 - x^2) \frac{\omega r}{R} x\,dx$$

$$= 2\,\frac{3\mu P\omega r}{4a^3 R}\int_0^a (a^2 - x^2)\,x\,dx$$

$$= \frac{3}{2}\,\frac{\mu P\omega r}{a^3 R}\left[\frac{a^2x^2}{2} - \frac{x^4}{4}\right]_0^a$$

$$= \frac{3}{2}\,\frac{\mu P\omega r}{R}\,\frac{1}{a^3}\left(\frac{a^4}{2} - \frac{a^4}{4}\right)$$

$$= \frac{3}{8}\,\frac{\mu P\omega r}{R}\,a.$$

Comparing the results of this integration with that for the journal bearing, we have

$$\frac{\text{Heat at thrust bearing contact}}{\text{Heat at journal bearing contact}} = \frac{3}{8}\,\frac{\mu P\omega r\, a r_i \rho_2}{R\,(0{\cdot}161)\,\mu P a^2 \omega\,(r_i + r)}$$

approximately.

For equal angular velocity of ball, maximum ball load, length of major axis and coefficient of friction

$$\text{ratio} = \frac{3}{1{\cdot}29}\,\frac{1}{a}\,\frac{r_i\rho_2}{R\,(r_i + r)}$$

$$= \text{approximately } \frac{3}{1{\cdot}29}\,\frac{r^2}{aR}\,\frac{r}{(r_i + r)}$$

For a Type 340 bearing $r = 0\ 25$ in. $R = 1{\cdot}85$ (Type W 3 [washer).

$r_o = 1{\cdot}53$ in. $\quad r = 0{\cdot}25$ in.

$r_i = 1{\cdot}03$ in. $\quad a = 0{\cdot}05$ in. say

$$\text{In this case ratio} = \frac{3}{1{\cdot}29}\,\frac{1}{0{\cdot}05}\,\frac{0{\cdot}0625 \times 1{\cdot}03}{1{\cdot}85 \times 1{\cdot}28} = 1{\cdot}26.$$

In this connection it is of interest to note that, comparing balls of the same size, the rated load per ball commonly recommended for a thrust bearing is about one-ninth that of the maximum load per ball for a journal bearing under its rated load for the same revs. per minute.

From this the author thinks we can infer that thrust bearings are, relatively to journal bearings, underrated as to their load-carrying capacity: *or* that friction effects due to pressure between the balls and the cage are much more manifest in the thrust than in the journal type. Slight variation of the diameter from one ball to another would bring this about. Thrust bearings we have fitted with balls of really uniform size (variation not more than 0·00002 in.) and tested are found to be capable of withstanding a greater percentage increase in their rated load than are journal bearings similarly fitted.

Load Stresses.

After the foregoing account of the deformations, the nature of the stresses at and near the contact surface will be apparent. From the displacement of the material which takes place at the contact surface (see Figs. 6 and 7), it is clear that the load stresses at

Table III.

Thrust Bearings.

Radius of test thrust ring. r_2.	Size of ball.	No. of balls.	Total test load on balls.	Mean load stress over contact surface.
inches.	inches.		lb.	tons per sq. in.
0·3593	$\frac{3}{8}$	3	1330	203
0·375			1330	214·7
0·3593			1995	237·2
0·375			1995	245·5
0·3593	$\frac{7}{16}$	3	1776	195·8
0·375			1776	201·4
0·3593	–		2664	223·5
0·375			2664	230·7
0·3593	$\frac{1}{2}$	3	2151	179
0·375			2151	181
0 3593			3226	205
0·375			3226	207
0·3593	$\frac{5}{8}$	3	1731	116·9
0·375			1731	119
0·3593			3463	147·2
0·375			3463	150
0·3593			5194	163
0·375			5194	172·2
0·300	$\frac{1}{2}$	16	1967*	80

* (Type W3 rated load at 1400 revs. per minute 983·5 lb.)

points on the contact surface will increase from zero at the periphery to a maximum at the centre of the contact surface. The flexure stresses will increase from the centre outwards. According to Hertz, the stress at the centre is one and a half times the average load stress obtained by dividing the total load by the area of the con-

TABLE III.—*continued.*

Journal Bearings.

Type of Bearing.	Size of Ball.	No. of Balls.	Assumed Journal test load.	Max. mean load stress over contact surface.	Assumed radii of curvature in inches.				Flexures for contact with inner ring at end of		
					Outer race.		Inner race.		Major axis.	Minor axis.	
	in.		lb.	Tons per sq. in.	r_1	r_2	r_1	r_2	min.	deg.	min.
MS 7	$\frac{9}{32}$	10	1370	142 at outer ring 173·6 at inner ring	0·7500	0·1477	0·4694	0·1477	37	2	$21\frac{1}{2}$
330	$\frac{7}{16}$	10	3005	129·6 at outer ring 168 at inner ring	1·2227	0·2297	0·7860	0·2297			
345	$\frac{5}{8}$	10	1760	89 at outer ring	1·7396	0·3281					
				98 at inner ring	1·7396	0·3359					
			2640	103 at outer ring	1·7396	0·3281					
				113 at inner ring	1·7396	0·3359					
MS 16	$\frac{3}{4}$	12	7600	129 at outer ring 149·5 at inner ring	2·1875	0·39375	1·4381	0·39375	32	1	$43\frac{1}{2}$

tact surface. It is possible therefore for the load stress at and near the centre to transcend the elastic limit, while elsewhere in the contact surface the load stress is well within that limit. Obviously, if some of the balls in a bearing are but ever so slightly larger than others, they will receive more than their share of the load, and may be over-stressed under a normal load. The nature of the stresses in the material is shown in Fig. 18, taken from one of Hertz's original papers. A is the centre and BC the axis of the contact surface—in this case a circle.

$>$——$<$ signifies tension.

$<$——$>$ signifies compression.

It will be noticed that near B and C the stress parallel to the contact surface changes from compression to tension, and that the tension forces near the surface in the region of the periphery of the contact ellipse are of greater magnitude than those within the bodies in contact. Table III. contains the results of calculating the average load stress at the contact between a ball and race when the ball in question is in line with the direction of the load. The maximum stress at the centre of the contact surface is one and a half times these figures. The results are so high that the author is tempted to believe that the elastic range of the contacting parts of the most perfect bearings is, when in actual use, much higher than is revealed by ordinary tensile and compressive tests done under isothermal and static conditions.

In Stribeck's experiments the smallest load (50 kg.) he applied to $\frac{3}{8}$ in. balls produced a measureable permanent compression. The mean stress over the contact surface is, then, by calculation, 164 tons per sq. in. for a $\frac{3}{8}$ in. ball on a flat surface, and 259 tons per sq. in. for two $\frac{3}{8}$ in. balls. The maximum load stress is one and a half times as great. The flexures at the periphery of the contact circles are respectively 1 deg. 30 min. and 2 deg. 23 min. Yet the agreement between the measured and the calculated results for the approach is nearly exact, showing that the material of the balls and plates behaves under the conditions of the test as if the elastic range were not transgressed.

As already stated, the distribution of the load stress over the contact surface has been shown, by Hertz, to be such that its magnitude at any point in that surface is represented by the height above the point of an imaginary ellipsoid described on the contact surface.

Expressed algebraically, the load stress p at any point $(x_1 y_1)$ in the contact surface is

$$p_1 = \frac{3P}{2\pi ab}\sqrt{1 - \frac{x_1^2}{a^2} - \frac{y_1^2}{b^2}}$$

where P is the total load on the contact surface and a and b are the semi-axes of that surface.

At the centre of the contact surface where x_1 and $y_1 = 0$

$$p_m = \frac{3P}{2\pi ab} = 1{\cdot}5 \times \text{mean load stress over contact surface.}$$

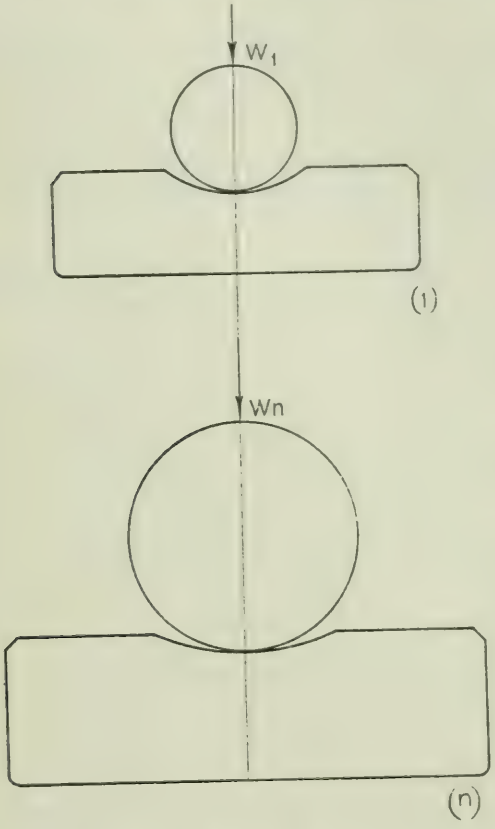


FIG. 20.

At the periphery of the contact surface, i.e., for points on the contact ellipse $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$, $p_1 = 0$.

Load-carrying Capacity and Size of Bearing.—If we assume the bearing to be stationary, it is easy to show that if bearing (1) which is loaded to its safe limit be compared with bearing (n), the linear dimensions of which are all n times as great as those

of bearing (1), the limiting safe load for bearing (n) is n^2 times that of the other.

Consider a ball on either of the raceways (see Fig. 20). Bearing (n) loaded (or any section of it) may be imagined as derived from bearing (1) unloaded either (a) by loading (1) and then magnifying n times, or (b) by magnifying bearing (1) unloaded n times and then loading it—the loads in the two cases being sufficient to produce similar deformation.

Consider a short line s (Fig. 21) (short so that it may be

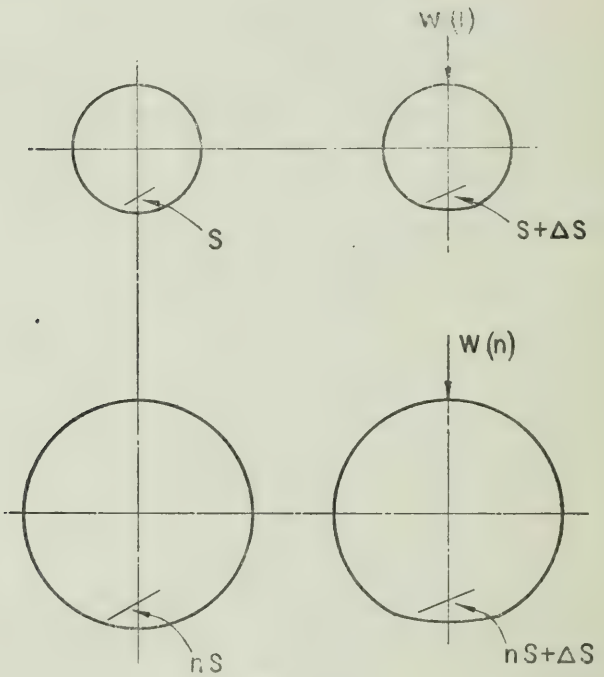


Fig. 21.

supposed to lie in a region of uniform stress) anywhere in bearing (1) unloaded. In the loaded bearing s becomes $s + \triangle s$.

The strain along s is therefore $\frac{\triangle s}{s}$.

In the (n) bearing, when the strain at a corresponding part is increased in proportion to its size, the strain along ns is

$$\frac{n \triangle s}{ns} = \frac{\triangle s}{s}.$$

As we are presupposing that stresses are everywhere within the elastic limit, we infer that in the case assumed the bearings are everywhere equally stressed at corresponding parts.

This, then, is true of the contact surfaces (or, more precisely, of the pressure surface, for it is possible to conceive of contact without pressure, though not of pressure without contact); the load stress at that surface being equal in both bearings.

But the area of contact in the (n) bearing is n^2 times that in the other. It follows, therefore, that the load on the (n) bearing must be n^2 times that on the other. This relationship has been expressed in various forms, e.g.,

$$\frac{P}{d^2} = \text{constant}\ \frac{P}{d^2} = 1000.$$

As the relationship is one which is much used, it is perhaps well to emphasise its limitations.

(1) It applies to geometrically similar bearings.

(2) It presupposes the same internal stresses, if any, and the same elasticity whether the balls and rings be large or small.

(3) The bearing is merely load-supporting.

(1) Bearings are rarely geometrically similar—their dimensions being regulated largely by the exigencies of space. The balls are geometrically similar, but we have also to consider their number, and the curvatures of the tracks.

(2) It is very questionable whether hardened balls as made at present are free from internal stress. The kind of stresses we may expect are tangential tension and radial compression near the surface, and compression nearer the centre of the ball. They arise owing to the rate of cooling through the critical range being less near the centre than at the surface. The greater this rate the less the proportion of the steel that reverts from the solid solution to the mixed condition. Since the reversion from the solid solution condition results in the density of the steel becoming less, it follows that wherever the rate of cooling varies much from outside to core, we may expect the core to put a bursting stress on the outside layers.

We must be prepared, therefore, for the existence of tangential tension forces in the surface layers of balls and races, and for these to be greater for large than for small balls and races.

Such tension forces will add themselves to those due to flexure at and near the periphery of the contact surface. In the case we are considering, provided the stresses do not exceed the elastic limit, the flexure at the periphery of the contact surface will be the same for bearing (n) as it is for bearing (1), but when the

initial tangential tension stresses differ, the available elastic range will be less for the balls or races having the higher initial stresses. As we do not know what is the criterion that decides the limiting safe load, we cannot say definitely that initial stress *will* affect that load. What we can say is that if the flexure at or near the periphery is the criterion, or a factor in the expression which is the criterion, the ball or bearing (n) which has (n) times the linear dimensions of bearing (1) will probably have a limiting safe load which is less than n^2 times that of the other when n is greater than unity, and greater than n^2 times that when n is less than unity. Similarly, if load stress is the criterion, or a factor, the initial radial compression operates in the same way, and the larger bearing will probably have less than n^2 times the limiting safe load.

(3) If friction or the heat generated is the criterion, or a factor, the limiting safe load of the larger bearing will be less than n^2 times that of the smaller. This follows from the relation between the heat evolved and the load and dimensions of the contact surface already worked out.

Work done $= \frac{1}{J}$ heat generated, and varies as n^3 for the same maximum load stress.

Consequently, if the total heat generated be the sole criterion, a bearing (n) loaded n^2 times bearing (1) will fail sooner than bearing (1).

A further consideration shows that if the heat generated be the sole criterion, the limiting safe load will vary as n^a where a is smaller than 2 and greater than 1. It can be shown that for equal heat per second a bearing having all its dimensions n times those of another, the load per ball $P_n = \sqrt[5]{n}\, P_1$.

Stresses due to Inertia of Revolving Parts.

In this category fall:—

Stresses at ball contacts with outer raceway due to centrifugal loading.

Friction between balls and cage due to any eccentricity of the latter.

Stresses at ball contacts of journal bearings owing to any inclination of the axis of the cage to that of the bearing.

Gyroscopic action at ball contacts in thrust bearings and journal bearings subjected to side thrust.

Stresses at ball contacts of journal bearings the axis of which revolves.

We have already referred to the effect of forces acting between the balls and the cage upon the heat generated at the contact surface. If the axis of the cage when revolving is parallel to, but not coincident with, that of the bearing, there will be a resultant force due to its rotation. Since the cage of a journal bearing is drilled from the outside, and so that the balls can be admitted to it from the outside but not from the inside, the effect of any such force is to increase the load upon a few balls in the direction of the resultant.

In the case of a Type W3 thrust bearing running at 1400 revs. per minute, the force in question on the cage is $M\omega^2R$ where M = mass of cage = 5 oz.

$$\omega = \text{its angular velocity} = \tfrac{1}{2}\,2\pi \times \frac{1400}{60}.$$

R = distance between its mass centre when revolving and the axis of the bearing. This will depend chiefly upon the difference between the ball diameter and that of the ball hole in the cage. The difference we allow is 0·006—0·010 in. The cage may, therefore, move parallel to itself in its own plane until its centre is 0·005 in. from the axis of the bearing.

$$\therefore \text{ Resultant force} = \frac{5}{16} \times \frac{\pi^2(140)^2}{6^2} \times \frac{0{\cdot}005}{12} \times \frac{1}{32{\cdot}2} \text{ lb.}$$
$$= 0{\cdot}02 \text{ lb. about.}$$

Force on a Ball due to its Inertia of Rotation.—Consider a thrust bearing containing eighteen $\frac{5}{8}$ in. balls rotating at 1000 revs. per minute, track diameter 4·7 in.

$$\text{Force on ball} = M\omega^2R \text{ where } M = \frac{4}{3}\,\frac{\pi(0{\cdot}625)^3}{8}\ 0{\cdot}28 \text{ lb.}$$
$$\omega = \frac{1}{2}\,\frac{2\pi 1000}{60}.$$
$$R = \frac{4{\cdot}7}{24} \text{ ft.}$$

$\therefore$ Centrifugal force for ball is

$$\frac{4}{3}\,\frac{1}{8}\,(0{\cdot}28)\,(0{\cdot}625)^3 \times \frac{\pi^2(100)^2}{6^2} \times \frac{4{\cdot}7}{24} \times \frac{1}{32{\cdot}2} \text{ lb. weight.}$$
$$= \text{about } 0{\cdot}6 \text{ lb.}$$

But for the cage this force would tend to wedge the rings apart, and would call into play forces in the direction of the axis of the

bearing the magnitude of which can be calculated in the way shown for a journal bearing.

The action of the cage is to take up any considerable centrifugal force on the balls. For a journal bearing the calculation for finding the centrifugal loading is similar.

For instance, for a $\frac{5}{8}$ in. ball in a Type 345 bearing which has a track diameter of 1·74 for the outer ring, R = 1·74 − 0·3125 = 1·43 in. about.

Whence centrifugal loading per ball at 500 cage revs. per minute

$$= \frac{1{\cdot}43}{2{\cdot}35} \times 0{\cdot}6 = 0{\cdot}365 \text{ lb. about.}$$

*Gyrostatic action.**—Hitherto, when considering the rotation of balls and races, we have tacitly assumed that that of the ball is controlled by that of the raceways it touches. Inertia effects may, however, assume the upper hand and result in the motion of the ball having no relation to that of the races at the moment in question.

So long as a ball is rotating about an axis which is always parallel to itself, and the bearing is running at constant revs. per minute, the angular velocity of the ball at any point is constant.

If, however, the axis of the ball is not always parallel to itself, its angular velocity will alter even when the bearing is running at constant revs. per minute, for angular velocity, in fact velocity generally, is a vector quantity, i.e., one which is characterised by magnitude, line of action or direction and sign. If the axis of the ball alters the direction factor alters, and hence the velocity, though not necessarily the magnitude or the sign. To produce such an alteration a couple is necessary. When the couple is not forthcoming, the angular velocity will persist and what is called gyrostatic spin will result, i.e., the ball in its subsequent position will continue to revolve about its old axis, or, at any rate, not about the axis which the movement of the raceways would prescribe for that position.

The only type of bearing in which the axis of rotation of balls is at all likely to keep parallel to itself is the annular journal bearing under pure radial load. The same under side thrust or torque tending to tilt the axis of one of the rings, however small

* The author believes we are indebted to Mr. J. J. Guest for the recognition of the part played by gyrostatic action and its effect upon the motion of balls in ball bearings.

be the force or couple, also all thrust, and all cone ball bearings contain balls the angular velocity of which is continually changing or being asked to change as the revolving race takes up successively new positions. Obviously, if the load on a ball were momentarily relieved as the revolving race moved from position (1) to position (2), the angular velocity of the ball in position (1) would persist practically unchanged until it reached position (2). It is therefore when, if ever, in the course of its circuit it comes into a region of no load or small load that we may expect gyroscopic spin to occur.

Gyroscopic Spin in a Thrust Bearing.—Consider the case of a thrust bearing containing balls of radius r, having an angular velocity ω about their own axes, and revolving on a track radius r_1 in the plane of rotation. The ball travels $r\ \omega$ in one second

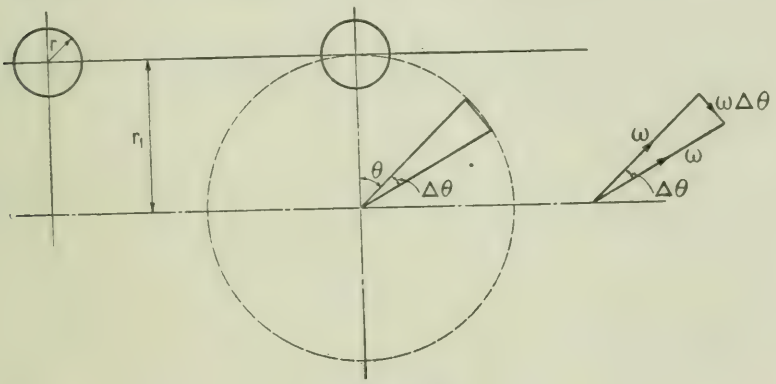


FIG. 22.

round the track. This arc $\div r_1$ is the angular velocity of the ball round the axis of the bearing, i.e., $\omega\ (r/r_1)$ (see Fig. 22).

Consider the ball at two moments, (1) after revolving through θ from the vertical, (2) after revolving through $\theta + \triangle\theta$. In both positions its angular velocity about its own axis is ω, but since ω has a different direction at the two moments, an angular velocity must have been added between them. To find this, draw vectors parallel to the axes of rotation in question. Since ω is constant in magnitude, their length will be equal. The line joining their ends represents on the same scale the added angular velocity of the ball, which is tangential to ω. From the vector diagram the added angular velocity $\triangle\omega = \omega\triangle\theta$.

To find the rate of change of this angular velocity (or the angular acceleration) and thence the magnitude of the couple responsible

for it, we will suppose that the two moments referred to above have an interval of time $\triangle t$ between them.

During this interval, the ball has rotated about the axis of the bearing $\omega \frac{r}{r_1} \triangle t$. This angle has already been referred to as $\triangle \theta$.

Substituting, we have $\triangle \omega = \omega \frac{\omega r}{r_1} \triangle t$.

$\therefore$ In the limit when the changes in ω and t are infinitely small,

$\frac{d\omega}{dt}$ = angular acceleration required $= \frac{r}{r_1}\omega^2$.

From the vector diagram and the relation between the direction of the vectors and of the axes of the ball, we infer that the acceleration in question is about an axis which is at right angles to the axis of the ball and also at right angles to the axis of the bearing. Only friction can provide the necessary couple and prevent slip due to gyroscopic spin.

This couple is moment of inertia of ball $\times$ angular acceleration $= mk^2 \frac{r}{r_1} \omega^2$.

For a ball of radius r inches, mass $m_1 = \frac{4}{3} \pi r^3 \times 0{\cdot}28$ lb. approx.

$$k^2 = \frac{2}{5} r^2.$$

Substituting, we get

$$\text{Couple} = \frac{4}{3} \pi r^3 \times 0{\cdot}28 \frac{2}{5} \frac{r^3}{r_1} \times \frac{\omega^2}{12_g} \text{ inch-lb.}$$

Now the maximum couple that can be supplied by friction $= 2r\mu P$ when P = load on ball.

To find the limiting load P on the ball when it is just sufficient to balance the tendency to spin due to inertia, we equate—

$$2r\mu P = \frac{8}{3} \pi \frac{r^6}{r_1} \frac{0{\cdot}28}{5} \frac{\omega^2}{12g}.$$

Assume $\mu = 0{\cdot}1$ and $\frac{r}{r_1} = \frac{1}{8}$

$$\therefore P = \frac{8 \pi 0{\cdot}28}{2 \times {\cdot}1 \times 32 \times 12 \times 3 \times 5 \times 8} \omega^2 r^4.$$

If $r = 0{\cdot}31$ and the bearing runs at N revs. per minute,

$\omega = \frac{r_1}{r} \times$ angular velocity of ball round axis of bearing.

$\therefore \omega = \frac{r_1}{r} \times \frac{1}{2}$ angular velocity of moving element round axis.

$$= \frac{8}{2} \frac{2\pi N}{60} = N \frac{\pi}{7 \cdot 5}$$

$\therefore$ P = approximately $12 \cdot 4 \times 10^{-7}$ N^2.

If N = 1000 revs. per minute, the load required is 1·24 lb. per ball. In view of the smallness of these inertia effects as compared with the load per contact and the friction due to slip already discussed, and which may, for instance, be 100 lb. and 10 tons per sq. in. respectively, it seems permissible to bear them in mind and leave them out in our calculations of bearings which do not revolve as a whole.

The case of a ball or roller bearing fitted in the big end of a

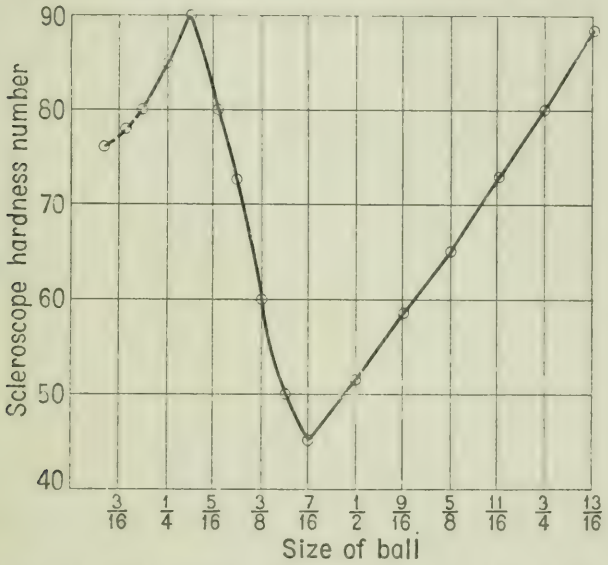


FIG. 23.—Scleroscope readings on Steel Balls.

connecting rod presents some features of interest, as the normal loading at the contact surfaces of balls and races, balls and cage and between ball and ball in cageless bearings is here considerably affected by the forces due to the inertia of revolving parts.

For instance, in a caged bearing we must expect couples of some magnitude tending to brake the balls applied at the contact surfaces between the balls and the cage. As already pointed out, the effect of these is to displace the position of the non-slip bands, and the heat generated at both ball/raceway and ball/cage contact surfaces will be increased. Furthermore, as pointed out by Captain Swan ("Automobile Engineer," July, 1919), centrifugal action may

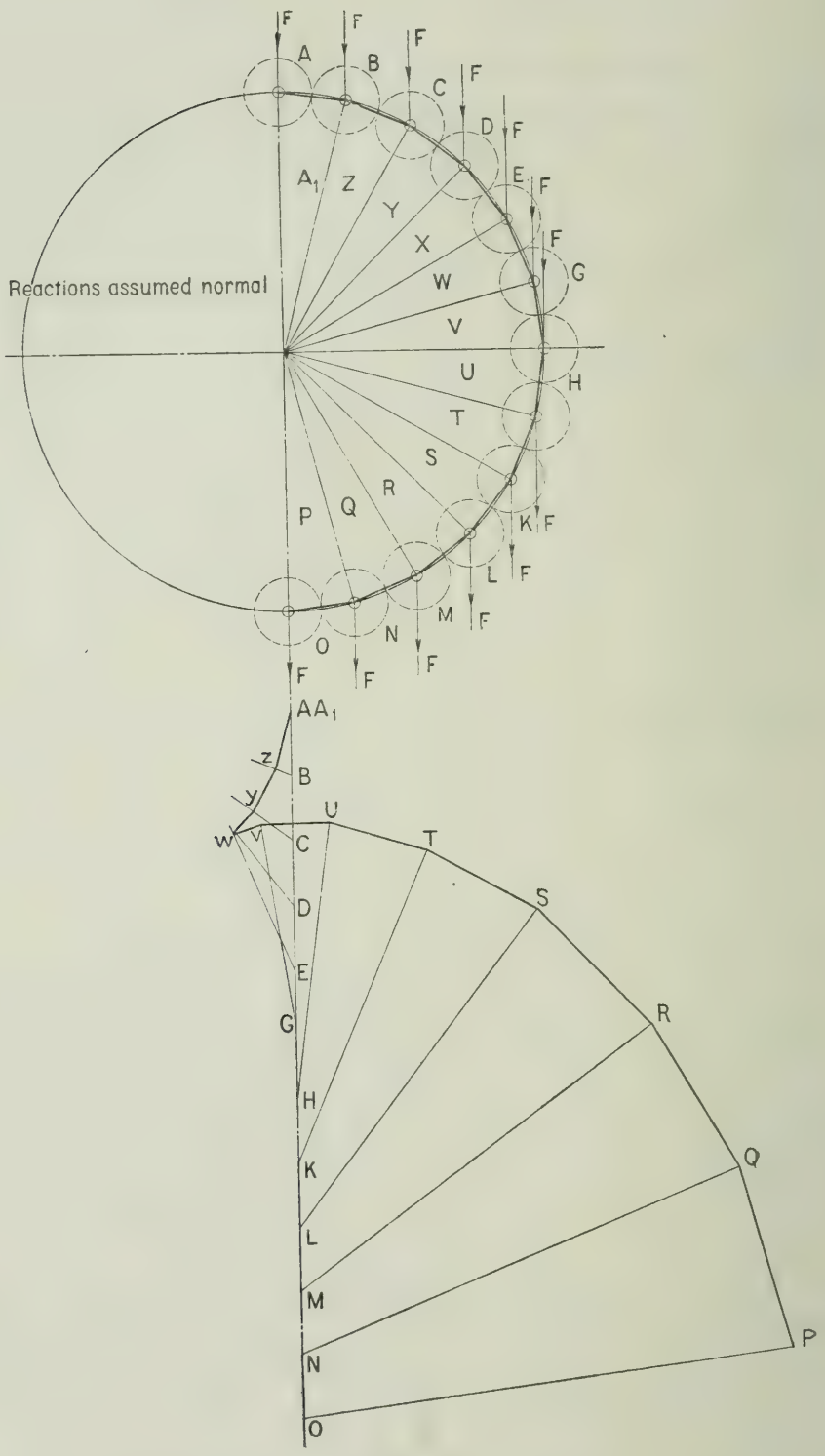

Reactions assumed normal
F
A
B
C
D
E
G
H
K
L
M
N
O
P
Q
R
S
T
U
V
W
X
Y
Z
A1
AA1
z
y
v

Fig. 24.

result in the cage wedging between the balls, and in crowding the balls together in a cageless bearing.

Another special feature is the acceleration and deceleration of the cage and balls as the connecting rod oscillates and the outer ring turns with it, first in one direction and then in the opposite direction, once each revolution of the flywheel. The forces producing this acceleration of the balls and cage reside in the contact surfaces and are due to friction. They come into operation when the position of the non-slip bands is displaced, and if the friction called for is greater than can be provided by displacement of the bands to the centre or ends of the "arc" of contact, skidding will result.

We can find the forces called into play by ascertaining first the maximum acceleration of the outer ring relative to the inner ring,

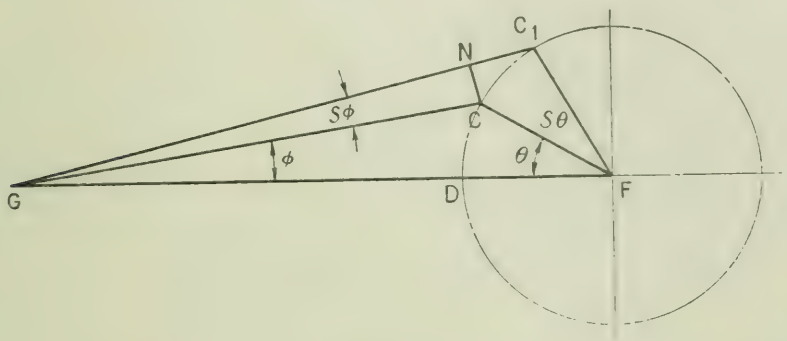


FIG. 25.

and the corresponding angular acceleration of the cage and balls about the axis of the bearing, and that of the balls about their own axes, and then equating the product of the moments of inertia of the accelerated parts and the angular acceleration to the couple producing the accelerations.

Angular Acceleration of the Outer Ring relative to the Inner Ring.—In Fig. 25, F represents the axis of the flywheel, C the axis of the bearing and G that of the gudgeon pin.

Consider a point C on the circle described by the bearing centre. If CC^1 represent the arc traversed and $\delta\theta$, $\delta\phi$ be the angles swept through by FC and GC in an interval of time δt, then $\delta\phi =$ angle turned through by outer ring in time δt about its axis.

$\therefore$ Angular velocity of outer ring $= \dfrac{d\phi}{dt}$

If DF = R and GD = L

At the inner dead centre

$$R d\theta = L d\phi$$

$$\text{whence} \quad \frac{d\phi}{dt} = \frac{R}{L}\frac{d\theta}{dt} = \frac{R}{L}\omega$$

where ω represents the angular velocity of the flywheel.

In Fig. 25 CN is drawn perpendicular to GC^1

$$\delta\phi = \frac{CN}{L} \text{ approximately}$$

$$= \frac{1}{L} CC^1 \sin NC^1C \text{ approximately}$$

$$= \frac{1}{L} R\delta\theta \sin NC^1C \text{ approximately}$$

In the limit when $\delta\phi$, $\delta\theta$ are infinitely small, C^1C becomes a tangent at C and

$$\text{angle } NC^1C = \text{angle } GCF - 90 \text{ deg.} = 90 \text{ deg.} - (\phi + \theta)$$

$$\text{then} \quad d\phi = \frac{R}{L} d\theta \cos(\phi + \theta)$$

$$\text{and} \quad \frac{d\phi}{dt} = \frac{R}{L}\omega \cos(\phi + \theta).$$

The maximum value of this is when $\phi + \theta = 0$ or 180 degrees. Differentiating, we have—

$$\frac{d^2\phi}{dt^2} = \frac{R}{L}\omega \frac{d}{d(\phi + \theta)} \cos\phi + \theta \frac{d(\phi + \theta)}{dt}$$

$$= \frac{R}{L}\omega \left(- \sin(\phi + \theta) \right) \frac{d(\phi + \theta)}{dt}.$$

The maximum value of this is when $\phi + \theta = 90$ degrees. In this position the change in ϕ for a given small change in θ is zero, $d(\phi + \theta)$ becomes $d\theta$

$$\text{and} \quad \frac{d^2\phi}{dt^2} = \frac{R}{L}\omega(-1)\frac{d\theta}{dt}.$$

$\therefore$ Maximum angular acceleration of outer ring about its axis $= -\frac{R}{L}\omega^2$.

The corresponding angular acceleration of the cage and balls about the same axis is

$$-\frac{R}{L}\omega^2 \left(\frac{R_o}{R_o + R_i}\right)$$

where R_i = radius of inner and R_o that of outer raceway: that of the balls about their centres being

$$-\frac{R}{L}\omega^2 \frac{R_o R_i}{R_o + R_i}\frac{1}{r}$$

where r = the radius of the ball.

If M_b and M_c are the masses of a ball and cage, and C_o, C_i the outer and inner radii of the cage, then

Moment of inertia of n balls about the axis of the bearing is

$$n\,M_b\left(\frac{2}{5}r^2+(r+R)^2\right)$$

Moment of inertia of cage about axis of bearing is

$$M_c\left(\frac{C_o^2+C_i^2}{2}\right)$$

and moment of inertia of n balls about their centres is

$$n\,M_b\,\frac{2}{5}r^2.$$

Therefore the couple required to produce the said acceleration of the balls round their centres and of the balls and cage round the axis of the bearing is

$$\frac{R}{L}\omega^2\,\frac{R_o}{R_o+R_i}\,n\,M_b\left(\frac{2}{5}r^2+(r+R_i)^2\right)+\frac{R}{L}\omega^2\,\frac{R_o}{R_o+R_i}\cdot M_c\,\frac{C_o^2+C_i^2}{2}+\frac{R}{L}\omega^2\,\frac{R_o R_i}{R_o+R_i}\,\frac{1}{r}\,n\,M_b\,\frac{2}{5}r^2.$$

For the case when $R=2\frac{1}{2}$ in., $L=10$ in., revs. per minute $=1600$, $\omega=\frac{1600\times 2\pi}{60}=168$ radians, $n=10$, $r=0{\cdot}25$ in., $M_b=0{\cdot}0183$ lb.

$R_i=0{\cdot}875$ approximately. $R_o=1{\cdot}375$ approximately, $C_o=1{\cdot}25$ in., $C_i=0{\cdot}9687$ in., $M_c=0{\cdot}25$ lb.

Substituting, we have—

$$\text{Couple}=\frac{2{\cdot}5}{10}(168)^2\frac{1{\cdot}375}{2{\cdot}25}\left(10\,(0{\cdot}0183)\left(\frac{2}{5}\,\frac{1}{16}+1{\cdot}125^2\right)+\tfrac{1}{4}\left(\frac{1{\cdot}25^2+0{\cdot}9687^2}{2}\right)+\frac{0{\cdot}875}{0{\cdot}25}\,10\,(0{\cdot}0183)\,\frac{2}{5}\,\frac{1}{16}\right)\text{inch-poundals.}$$

$=4130\,(0{\cdot}2363+0{\cdot}3127+0{\cdot}016)$ inch-poundals.

$=4130\times 0{\cdot}565=2435$ inch-poundals.

$=6{\cdot}3$ inch-lb.

This couple is applied at the ball contacts. If F is the total frictional force called into play

$$F\times 0{\cdot}5=6{\cdot}3$$

$$\therefore\quad F=12{\cdot}6\text{ lb.}$$

If $\mu=0{\cdot}1$ a total radial load of 126 lb. is required. Normally, the load will provide more than this force, but only on less than

half the balls. Balls with no load upon them, or with a small load, will not be accelerated by the friction at their own contacts with the raceways, and will be skidded between the races by the other balls. If for any reason the cage exerts a braking action upon the balls or upon any one or two of them, the tendency of such balls to skid when the outer ring is accelerated is greater than would be the case were the cage located by either of the rings instead of by the balls. In this respect the balls will behave in the same way as a wheel travelling over an undulation of the road surface.

Force between Ball and Ball in a Cageless Bearing which revolves as a whole about an Axis parallel to the Axis of the Bearing.

Captain Swan* proceeds by computing the centrifugal force on the balls or rollers (assumed the same for each ball), and neglects the force on each succeeding ball due to the forces on those behind it. Instead, the numerical sum of all the forces is taken at the end as the maximum force between two balls. The procedure is only intended to be approximate. Needless to add, such forces cannot be added numerically.

A much simpler method is to treat the problem graphically. A ball has the convenient property that all normal forces pass through its centre, so we may substitute for the balls in contact an imaginary framework of rods hinged at all centres of the balls and at the centre of the race, with a proviso that the rods carrying inter-ball forces can resist only compression. The force in the radial rods will be tension or compression according as the reaction is taken by the inner or the outer race. In order to arrive at a first approximation to the inter-ball forces, we will neglect the friction at the ball/race contacts.

Fig. 24 represents such a framework. The centrifugal force on each ball is assumed to be the same (= F) and in the direction shown. Obviously, the compression in the top link of the framework due to the action of these centrifugal forces is zero. Below is a collection of triangles of force representing the forces in the various links. Each line is used as part of two triangles of force, inasmuch as the force in any member acts upon two joints of the link work. BZ represents the magnitude and direction of the force between the second and third balls, etc., OP representing the

* *Loc. cit.*

maximum inter-ball force. Approximately, OP scales 0·63 of half the sum of the centrifugal forces on all the balls.

For the case when there are ten $\frac{1}{2}$ in. balls in a bearing revolving 1600 times per min. about an axis $2\frac{1}{2}$ in. from its centre, for steel balls—

Centrifugal force per ball $= 5{\cdot}22 \times 10^{-7}$ D.d^3N^2 lb.

where D = diam. of path in inches.

d = „ ball in inches.

N = revs. per minute.

Substituting, we have—

Average centrifugal force per ball $= 5{\cdot}22 \times 10^{-7} \times 5 \times (0{\cdot}5)^3 (1600)^2$ lb.
$= 0{\cdot}855$ lb.

for five balls $= 4{\cdot}275$ lb. approximately,

whence maximum inter-ball pressure $= 2{\cdot}7$ lb. approximately.

This is so small that it is difficult to see how it can account for the failure of such bearings unless it be that owing to the high relative velocity of the parts in contact, the rate at which heat is evolved is very great.

To investigate this, we will calculate the rate at which work is done against friction forces at a ball/ball contact surface, and then compare the ratio of this to the area of the contact surface with the corresponding ratio for the ball/raceway contact surface.

Friction and Heat at the Contact Surface between Ball and Ball in a Journal Bearing.

If ω_b be the angular velocity of the balls about their own centres, and r their common radius, then the relative velocity at all points on the surface of contact is very nearly equal to $2r\omega_b$.

The rate at which work is done in moving a small element area $dy.dx$ through a distance so small that the load stress p_1 may be considered to remain constant is $\mu p_1 \, dy.dx \, 2\, r\omega_b$.

The sum for all the elements of the contact circle is the rate at which work is done against friction forces per contact and

$$= \int_{x=+a}^{x=-a} 2\mu r \omega_b p_1 \, dy \, dx$$

Assuming μr and ω_b to be constant for all points

$$\text{Work per sec.} = 4\mu r \omega_b \int_0^a p_1 \, dy \, dx.$$

$$= 4\mu r\omega_b \int_o^a \text{area of section of pressure ellipsoid at distance } x.$$

For contact ellipse $p_1 = \dfrac{3P}{2\pi ab}\sqrt{1 - \dfrac{x^2}{a^2} - \dfrac{y^2}{b^2}}$

$\therefore$ for contact circle $p_1 = \dfrac{3P}{2\pi a^2}\sqrt{1 - \dfrac{x^2}{a^2} - \dfrac{y^2}{a^2}}$

Now the area of section of the pressure ellipsoid at the distance $x = \frac{1}{2}\pi y p_x$. To find p_x, we put $y = o$ in equation to ellipsoid

$$\text{whence } p_1 = \frac{3P}{2\pi a^2}\sqrt{1 - \frac{x^2}{a^2}}$$

y is given by the equation to the contact circle

$$\frac{x^2}{a^2} + \frac{y^2}{a^2} = 1 \qquad \text{whence } y = \sqrt{a^2 - x^2}.$$

Substituting, we have—

$$\text{Work per sec.} = \frac{4\mu r\omega_b \pi 3P}{2 \times 2\pi a^3}\int_o^a (a^2 - x^2)\,dx$$

$$= \frac{\mu r\omega_b 3P}{a^3}\,\frac{2}{3}\,a^3$$

$$= 2\mu r\omega_b P.$$

The radius a cm. of the contact circle when two stationary balls radius r cm. sustain a load P (kg.) is given by the expression

$$a = 0{\cdot}70 \sqrt[3]{\frac{P2r}{E}} \text{ cm., where E} = 2{,}150{,}000 \text{ kg. per sq. in.}$$

For two $\frac{1}{2}$ in. balls sustaining 2·7 lb. P = 1·22 kg., $r = 0{\cdot}635$ cm.

$$\therefore a = 0{\cdot}70 \sqrt[3]{\frac{1{\cdot}22 \times 1{\cdot}27}{2{\cdot}15 \times 10^6}}$$

$$= 0{\cdot}00628 \text{ cm.} = 0{\cdot}0025 \text{ in. approximately.}$$

mean load stress = *about* 61·5 *tons per sq. in.*

Ratio work per sec.: area of contact surface for ball/ball contact

$$= \frac{2\mu\, r\, \omega_b\, P}{\pi a^2} = \frac{0{\cdot}2 \times 0{\cdot}25 \times 2{\cdot}7}{\pi\,(0{\cdot}0025)^2}\,\omega_b = 69 \times 10^2\,\omega_b$$

and ratio for ball/inner race contact

$$= \frac{0{\cdot}16\,\mu P}{\pi ab}\,\frac{(R_i + r)}{R_i}\,\frac{a^2}{\rho_2}\,\omega_b.$$

For the case when the load on the bearing is 1000 lb., the maximum load per ball, according to Stribeck, is about 500 lb.,

$R_1 = 0{\cdot}875$, $r = 0{\cdot}25$, $a = 0{\cdot}072$ in., $b = 0{\cdot}0087$ in., $f_2 = 0{\cdot}256$ in., and the latter ratio becomes

$$\frac{(0{\cdot}16)\,(0{\cdot}1)\,500 \times 1{\cdot}125 \times (0{\cdot}072)^2}{\pi\,(0{\cdot}072)\,0{\cdot}0087\;0{\cdot}875 \times 0{\cdot}256}\;\omega_b$$

$$= 104\;\omega_b$$

or the rate at which heat is generated per square inch of contact surface is between ball and ball about 66 times as great as it is at the ball/raceway contact surface.

So great a rate is most likely to exert a destructive effect upon the balls, and the author ascribes the failure of the bearings referred to by Captain Swan to this heat effect rather than to the load stress.

By fitting a cage, the relative velocity at the ball cage contacts is halved, and if the cage is of brass its superior heat conductivity must be an advantage. By locating such a cage on one or other of the rings, the braking of the balls is minimised and inter-ball pressure is obviated, both of which secure to the ball the greatest obtainable immunity from inertia effects causing heat evolution to an objectionable extent.

We will now pass on and briefly review the main operations which the ball bearing goes through in the making.

1. The Ball Races.

Automatic Shop.—Between receiving the bars of chromium steel and issuing them to the machinists, they are tested for hardness by means of the Rudge-Whitworth Auto Punch, every bar being tested near the two ends and at the middle. Only bars with a hardness corresponding to 201 Brinell No. or less are accepted. Bars near this limit are Brinelled. The bars are marked to indicate their hardness, so that bars of similar hardness may be machined at the same tool setting, thereby minimising rejections for variation of dimensions after turning, and ensuring a high degree of uniformity of the internal stresses produced by the machining operations and minimising warping.

The machining operations vary somewhat according to the size of bar and the design of the bearing. Bars of 2·4 in. diameter and over are cut up into blanks. Smaller bars are machined in four-spindle Gridley Automatics.

Bars 2·4 in. diameter and over are first dealt with in Lang variable speed cutting off machines. Two parting tools operate at the same time, the finishing diameter at the bottom of the cut

being slightly less than that of the bore of the turned bearing. The blanks are cut off in fours, and are then drilled vertically, being held in a revolving chuck, the drill completing the parting operation left by the Lang. The separate blanks are then chucked by the outside in $2\frac{1}{4}$ in. single-spindle Gridley automatics and finish-machined on the face and bore. The blank is then chucked from the finished bore with the finished face inside. The second face is machined and the outside diameter turned simultaneously. All radii and chamfers on one side are formed, and trepanning of the outer ring carried beyond the centre. The blank is reversed in the chuck and the radiusing operation and trepanning repeated with the same slides as used in the first instance to ensure identity of the radii on both sides. The second trepan removes the outer ring, and allows of the tracking of the inner ring at the same setting.

The outer ring then has the bore made to size and the ball track is formed.

This is followed by milling ball slots in both inner and outer rings. In order to minimise warping during hardening, the slot is not milled right away. A web is left which is removed by grinding.

Bars under 2·4 in. diameter.—The operations on these are as follows:—

Bar 1.—A. Form width of outer ring and radii.
B. Turn outside diameter.
C. Rough-drill the inner ring.

Bar 2.—A. Trepan off outer ring.
B. Bore inner ring concentric.

Bar 3.—A. Form ball track and chamfer inner edge.
B. Turn outside diameter.
C. Ream bore to size.

Bar 4.—A. Cut off inner ring.
B. Feed bar up to stop and index.

It is to be understood, of course, that the operations (A) on bars 1, 2, 3 and 4 proceed simultaneously; also that the succeeding operations (B) proceed simultaneously, and so on.

The operations on the outer ring proceed as already indicated for the larger sizes, as follows:—

Outer ring chucked by outside, tracked, and bore chamfered.

The inner ring is radiused at the bore.

Both rings have ball filling slots milled as already described.

Thrust Washers.—The operations on these as carried out in Gridley four-spindle automatics are as follows:—

Bar 1.—A. Rough-drill.
B. Face.
C. Turn outside diameter.
D. Radius outside edges.

Bar 2.—A. Bore hole concentric.
B. Finish-turn outside diameter.

Bar 3.—A. Ream bore.
B. Track.
C. Finish edges of face.

Bar 4.—A. Part off.
B. Feed bar to stop and index.

The shaft ring has the bore chamfered, and the clearance ring has the outside edge of the bore radiused.

Hardening—Outer and Inner Rings and Thrust Washers.—Outer rings with inner rings within them are placed on trays, the total weight of which is constant for each size of bearing. These trays are pushed into the furnace through a slot in the front, one tray every so many minutes—the interval being constant for each size of bearing. They are in the furnace from 20 to 45 minutes, the period depending again upon the size of bearing. The contents of the trays are ejected automatically at the proper time into a special hardening oil maintained at a steady temperature, and the trays travel underneath to the front of the furnace ready for future use. The furnace is heated by a gas and air mixture supplied from a Selas Turner compressor, whereby the ratio of gas to air and the pressure at which it is supplied to the furnace can readily be controlled. With this and the aid of carefully and regularly calibrated pyrometers, in which a platinum, platinum-rhodium couple is used, and an observer who switches the pyrometers successively in series with the same indicator (of Cambridge Scientific Instrument Co.'s manufacture), the temperature is maintained within very narrow limits, generally $\pm$ 2° C. for hours on end. In order to ensure that all the bearings are uniformly heated, many precautions are taken, including that of feeding trays of scrap rings through the furnace to stabilise the temperature both before commencing the batch and after finishing it.

Small rings are heated in rotary furnaces, the speed of which can be regulated to give the rate of heating and time in the furnace suitable for the various sizes of rings to be dealt with. The rings

run through the worm in from 20 to 45 minutes, and are then quenched in the special hardening oil.

After hardening, all bearings are tempered in oil sufficiently to relieve undue internal stresses without reducing the elastic limit. The temperature employed is 125° C., and the time varies from one hour upwards, according to the size of the bearing. Impact tests have shown that the energy absorbed in breaking is doubled by the tempering treatment.

Grinding.—Every part of the Rudge-Whitworth bearing is ground except the radii and chamfers, and the inside diameter of the outer ring and the outside diameter of the inner rings. The first operation is to place the hardened rings on the magnetic chuck of a Blanchard grinding machine. Five to six thousandths of an inch are removed from each side, the maximum permissible variation of width from ring to ring being 0·002 in.

Bore Grinding.—Inner rings are chucked by the face being held by two clamps or dogs, which press the ring lightly against the chuck face. They are centralised by the grinding wheel and wet ground in the bore, the maximum permissible variation from ring to ring being 0·0005 in. Go and Not-go gauges are used, the tolerance on each of which is 0·00005 in., the Not-go being nominally 0·00025 in. larger than the Go.

Production and Maintenance of Gauges accurate to 0·00005 *in. or less.*—It will be readily understood that the production of an adequate number of such bore gauges, and their maintenance between the prescribed limits is no light task, and one of the most difficult that falls to the lot of the ball bearing manufacturer. It is accomplished at the Rudge-Whitworth Works by making use of a 30-ton Denison testing machine for rectifying all that do not happen to come within the limits after lapping. The ends of the gauge are placed between two balls and pressure applied in a direction at right angles to that in which the gauge is to be expanded.* As pressures of several tons are required to increase the gauging dimensions a few tenths of a thousandth of an inch, by careful choice of the correct pressure the dimensions may be altered by a few hundredths of a thousandth of an inch with ease. To facilitate the process and minimise variations in the permanent extension of the gauges due to variation of their hardness and

* British Patent Application 29857—1919.

internal stress collars are put round the balls, the length of which is less than that of the diameter of the balls by a definite amount. This secures depressions of a definite size, all excess pressure above what is required to force the balls into the gauge being taken by the said collars.

This rectifying process is applied to gauges which have worn small, and has much simplified the work of the travelling inspectors who are responsible for taking gauges out of commission immediately they are outside the specified limits, and replacing them by correct gauges. Furthermore, it makes for greater uniformity in the dimensions of the finished work.

Grinding Outside Diameters.—In grinding the outer diameter of the outer ring, several rings are clamped together in gangs with a minimum amount of end pressure. The use of a greater pressure results in the production of rings which may be oval when the pressure is relieved. The grinding is finished when the rings measured with a micrometer checked against a disk for reference are correct. Two of the rings from each gang are tested with the Prestwich fluid gauge for size and circularity, and if not correct all are returned for further grinding until they are correct. The maximum permissible variation from ring to ring is 0·0005 in. for sizes up to 50 mm. and 0·0008 in. for larger sizes.

Track Grinding.—The outer rings are held by four clamps which practically encircle the outside diameter. The chuck is rotated and oscillated about an axis which is normal to the centre of the raceway. This ensures the true form of the raceway.

The inner rings are mounted on a mandrel and ground one at a time to be a match with an outer ring. Both the tracks are polished, after which the pair are clipped together and ultimately assembled together. To ensure the correct fit of balls, the final polishing operation done on the inner ring is not gauged. Standard balls are assembled between the two rings, and the process stopped when the fit is judged correct. The correctness of the fit is judged by assembling the rings with two sets of measured balls, the diameters of which differ by 0·0002 in. If the rings are tight to the larger and loose to the smaller balls they are passed and assembled with balls of the proper size to give the desired fit.

Slot Grinding.—The slots are ground with a small wheel, the axis of which is inclined to that of the ring by about 3 degrees.

2. Ball Making.

Blanks turned from the bar or slugs from dead soft wire are headed between two hemispherical dies, any blanks which show faulty material or bad shape being thrown out. The blanks which are passed are normalised in the heat-treatment shop and sent to the ball grinding department.

(1) *Oil Grinding.*—The blanks are fed into the hoppers of rotary grinding machines, and pass between a stationary cast iron plate and a large grinding wheel (24 in.—30 in. diam.). The cast iron plate is cut away to allow the balls to run out and enter again after being mixed with their fellows. The balls in the machine sustain a pressure of 500—3000 lb. graded according to the size of ball and hardness of the grinding wheel. The operation is stopped when the balls are, according to their size, 0·004 to 0·010 in. over nominal size. They are then inspected and passed to the hardening shop. Here they are hardened in gas heated, rotary, spiral worm furnaces like those used for the smaller bearing rings. The time occupied in passing through the furnace and the temperature are chosen to suit the size of ball undergoing treatment. A common temperature is 820° C. and time about 15 min. Hardened balls are tempered in oil in much the same way as the rings.

(2) *Lapping.*—After inspection and rejection of any showing hardening cracks, the balls are fed into the hoppers of the lapping machines. These are very similar to the oil grinding machines, but the wheels used are of a harder grade and a finer grit. Paraffin is run through the machine to remove abraded particles and maintain a uniform temperature. This process reduces the balls to nominal $\pm$0·0002/0·0004 in. according to size, and makes them very nearly spherical.

(3) *Tumbling.*—The balls are then placed in tumbling barrels together with a supply of a slow cutting agent and tumbled together for a day or two, their size to the nearest 0·00001 in. being noted from time to time.

(4) *Polishing.*—This also is done in tumbling barrels, and its success depends largely upon the slow cutting agent employed in the previous operation. The ideal is one which has particles not larger than the depth of scratches on the balls at the commence-

ment of the operation, and which reduce in size at about the same rate as the scratches reduce in depth. Given this, a first-class polish is assured.

Machine for Gauging Balls.—A full realisation of the importance of having all the balls in a bearing of uniform size has led us to devote some attention to the problem of gauging balls. Our solution of it is a little machine* capable of sorting balls which range over 0·0001 in. between maximum and minimum into, say, five different lots, the mean sizes of which differ by 0·00002 in. The essential part of the contrivance is a small tubular gauge having what gun makers would call a choke bore, the inside diameter of which is *less* than that of the minimum size of the balls to be sorted. Balls placed in the mouth of this gauge will require some force to push them through, the amount of which will depend upon the amount of the interference between the ball and gauge and the elasticity and thickness of the walls of the gauge. The rest of the machine consists of means of delivering balls to the gauge and conveying them from the gauge to boxes. It works automatically, but requires testing from time to time with a standard ball to ascertain which lot is the same as standard. As the gauge wears the boxes are moved round the rotating table.

Gauging and Inspection—Finished Bearings.

The following is a list of the dimensions which are gauged, the type of gauge employed for the purpose and the total tolerance on the dimension in question:—

Eccentricity of the outer ring when rotated on the inner.	Clock faced indicator.	0·001 in.
Eccentricity of the inner ring when rotated on the outer.	,, ,, ,,	0·001 in.
Wobble of the faces of outer and inner ring.	Starrett's Multiple indicator.	0·003 in.
Alinement of the faces of inner and outer rings.	Straight edge and feeler.	0·004 in.
Amount of end play.	Doubtful ones with clock indicators.	0·003—0·007 in.

* British Patent No. 138427—1919.

Gauging and Inspection—Outer Rings.

idth.	Gap gauge.	0·002 in.
Outside diameter.	Ref. Disk and Prestwich.	0·0005—0·0008 in.
Diameter ball track.	Piece of spring steel with ball fitted at either end.	
Curvature of ball track.	Disk gauge.	2½ per cent of ball radius.
Ball track position.	Scriber gauge.	0·005—0·008 in.
Concentricity of track, and outer diameter.	Dial indicator.	0·0005 in.
Chamfer and radius.	Radius and chamfer gauges, *i.e.* form gauges.	5 per cent ball radius.
	Track curvature.	
Disk gauge.		
Thickness of each washer.	Dial indicator.	0·005 in.
Thickness of assembled washer.	Micrometer.	0·004 in. *and true.*
Scleroscope.	80.	
Scratch test.		
Examination for flaws.		

Thrust Cages.

External diameter.	Bow gauge.	0·006 in.
Internal ,,	Plate plug.	0·006 in.
Width.	Gap gauge.	0·006 in.
Diameter of ball hole.	Plug.	0·004 in.
Track pitch of balls in cage.	Correct washer used with balls in cage.	No limit.

Gauging and Inspection—Inner Rings.

Width.	Gap.	0·002 in. total tolerance.
Bore.	Plug.	0·0005 in.
Ball track curvature	Disk.	0·004 in. (on radius of curvature).
Track position.	Position gauge.	0·001 in.
Filling slot position.	,, ,,	0·005—0·008 in.
Concentricity of track and inside diameter.	Clock.	0·0005 in.
Chamfer.	Form.	
Radius.	,,	No limit.
Ball track diameter.		
Scleroscope.	80.	
Scratch test.	Flaws.	

Gauging Cages.

Bore of cage.	Plate plug.	0·003 in.
Ball hole diameter.	Plug.	0·010 in.
External diameter.	Bow gauge.	0·01 in.
Width of cage.	Gap gauge.	0·003—0·005 in.
Diameter over balls in position.	Ring gauge.	0·005 in.

Gauging and Inspection—Thrust Rings.

Outside diameter (clearance washer).	Prestwich.	0·001 in.
Inside diameter (Shaft Washer).	Plug.	0·001, 0·002 in. on same.
Track pitch.	Track pitch gauge.	No limit.
Scleroscope.	80.	
Scratch test.	Flaws.	

THE BEARING—UNDER TEST.

Two types of machine are in constant use, night and day, testing thrust bearings under axial load and journal bearings under radial load, axial load, and various combinations of radial and axial loads.

The one consists of an electric motor connected by a flexible coupling to a spindle supported near its ends in roller bearings, and carrying at its centre the bearing under test. The outer ring of the test bearing is an easy push fit, in a housing which is connected underneath to a lever system, enabling loads to be applied up to nearly 5 tons. A pump circulates oil through the bearings.

The outer ring is locked into its housing by a lock ring which screws up to one face of the bearing. The inner ring is a tight fit on the spindle.

All bearings are disassembled before running, and the balls are carefully measured to ascertain the size (i.e., mean size) and spheric error (difference between maximum and minimum diameter) for each ball.

They are then re-assembled, and the mounted bearing is tested for end play in order to ensure that the fit is not too tight. After assembling for testing, the radial up and down movement of the housing, as the shaft is revolved, is measured.

These measurements are repeated after the test and the wear noted. During the test a recording wattmeter in series with the motor records the fluctuations, if any, of the power required to drive the motor and bearing, and a recording pyrometer records the

temperature variations of the housing. This type of machine is used for testing under pure radial or combined radial and thrust load.

Under pure radial load the machine tends to put end thrust on the test bearing owing to the roller bearings tending to run axially one way or the other. This is prevented by putting small thrust bearings at the end of the spindle carrying the test bearing and of the motor shaft.

For applying combined radial and axial loads, a Type W3 thrust bearing is fitted to an extension of the spindle, and by means of a single weighted lever compression is applied between the stationary member of the thrust washer and a cross piece connected by tension members to the housing. The result is axial side thrust on the journal bearing under test, and non-axial load on the W3 bearing.

For applying pure thrust to journal bearings, and for testing thrust washers and balls, another type of machine is used. As before, a spindle is driven by an electric motor running at 1400 revs. per minute, and connected to it by a flexible coupling. This spindle is located by two journal ball bearings, one small and the other large. A W3 thrust washer takes all the thrust applied. The end of the shaft away from the motor carries an adapter which houses the outer ring. The thrust is applied to a bush, on which is mounted the inner ring. This machine is not provided with an oil pump and the bearings are greased.

When testing balls, we usually fit three only into a cage and run them on W3 washers ground to a curvature $r_2 = 0{\cdot}36$ in.

Running tests carried out in these machines fall into two categories:—

1. Destructive tests intended to cause the bearing to fail;
2. Duration tests in which the bearing or balls are not expected to fail.

In the former, the load on a journal bearing is about three and a half times the limiting safe load as listed, or for a ball, ten to twelve times the limiting safe load per ball listed for that size of ball in its proper thrust washer. In the latter, the radial load is one and a quarter to one and a half times the listed load for a journal bearing, and two to five times the listed load for a thrust washer.

Results of Running Tests.—Many interesting results have been obtained in the course of these tests, of which the following are quoted.

The balls in journal bearings tested to destruction generally wear at different rates, the smaller ones wearing away faster than the larger ones.

Size of ball.	Wear in inches of largest in same bearing.	Wear in inches of smallest in same bearing.
$\frac{3}{8}$ in.	0·00003	0·00008
	0·00001	0·00008
$\frac{1}{2}$ in.	0·00005	0·00003
	0·000016	0·00002
	0·00005	0·00007
	0·00003	0·00007
	0·00005	0·00012
	0·00005	0·00014
$\frac{3}{4}$ in.	0·00008	0·00013
	0·00008	0·00014

The balls in tested bearings vary in size more after the test than before.

Type MS 11, run under $1\frac{1}{2}$ times
rated load ..... 10 balls.
variation before. 0·00001 in. 200 hours.
,, after. 0·00007 in. Not damaged.
(The figures are the differences between the greatest and the smallest dimensions obtained by measuring all the balls all over, i.e., in 10—12 places on each.)

Type 345 run under $3\frac{1}{2}$ times
rated load ..... 10 balls.
variation before. 0·00002 in.
,, after. 0·00014 in. Failed after 70h. 21m.
,, before. 0·00004 in.
,, after. 0·00032 in. Failed after 14h. 57m.

Type 340 run under $3\frac{1}{2}$ times
rated load ..... 12 balls.
variation before. 0·00009 in.
,, after. 0·00024 in. Failed after 14h. 10m.
,, before. 0·00009 in.
,, after. 0·00030 in. Failed after 6h. 30m.

This variation in size is largely a heat effect. Samples of balls of various makes are found to alter permanently in size when heated in oil at 100° C. for 12 hrs., allowed to cool for 12 hrs., heated again for 12 hrs., and cooled again for 12 hrs.

Size.	Make.	Variation before.	Variation after.	Alteration of mean size.
in.		in.	in.	in.
$\frac{11}{16}$	D. W. F. (1)	0·00013	0·00017	− 0·00026
$\frac{11}{16}$	D. W. F. (2)	0·00009	0·00016	− 0·00020
$\frac{11}{16}$	H.	0·00001	0·00001	− 0·00024
$\frac{5}{8}$	H.	0·00007	0·00027	− 0·00019
$\frac{1}{2}$	S. (1)	0·00001	0·00002	− 0·00012
$\frac{1}{2}$	S. (2)	nil.	nil.	− 0·00019
$\frac{1}{2}$	R. W.	nil.	0·00004	− 0·00005
$\frac{7}{16}$	R.W. special.	0·00006	0·00006	nil.
$\frac{1}{2}$	,, ,,	0·00002	0·00002	nil.
$\frac{5}{8}$	,, ,,	nil.	0·00002	− 0·00004

Journal bearings tested under pure axial load equal to their limiting safe load ($P = T$) run for considerable periods before they fail, some not failing even after a long run.

A Type 345 single row R.W. journal bearing ran 18 hours 33 min. at 1400 revs. per minute under a pure axial load of 1590 lb., the direction being such as to force the balls toward the slot of the inner ring.

Result: Inner ball race chipped at slot.

A Type 345 double row radial type ran 7 hours 45 min. at 1400 revs. per minute under pure axial load of 1590 lb.

Result: Inner ring chipped on track that took the load. Balls in this row were a brownish copper colour, the others being very slightly coloured.

A Type 345 single row journal bearing made with slots on opposite sides ran 32 hours 18 min. at 1400 revs. per minute without injury under pure axial load of 1590 lb., the direction being such that the balls were not forced towards the slots.

A Type 150 single row journal bearing ran 20 hours 15 min. at 1400 revs. per minute without injury under pure axial load of 1113 lb., the direction being such that the balls were forced towards the slot of the outer ring.

Journal bearings will sustain considerable axial thrust alone, and combined with radial load.

Type of bearing.	Listed safe radial load at 1400 revs. per minute.	Test loads. Radial.	Test loads. Axial.	Time run. h.	Time run. m.	Condition after test.	Change in side play.
	lb.	lb.	lb.	h.	m.		in.
1 J1	380 (calc'l'd)	nil	180	46	50	O.K.	+ 0·0035
12, $\frac{1}{4}$ in. balls ..		640	nil	40	15	O.K.	− 0·0010
125.							
12, $\frac{9}{32}$ in. balls .	464	nil	180	2	45	O.K.	+ 0·0015
		nil	580	27	35	O.K.	+ 0·0005
		437	nil	2	40	O.K.	
MS 11 (1).							
10, $\frac{7}{16}$ in. balls .	842	nil	263	45	0	O.K.	nil
		214	262	39	26	O.K.	nil
		380	220	45	25	O.K.	− 0·0005
		1280	nil	65	10	O.K.	nil
MS 11 (2).							
10, $\frac{7}{16}$ in. balls.	842	nil	263	47	15	O.K.	+ 0·0020
		214	262	41	30	O.K.	nil
		380	220	27	13	O.K.	nil
		1280	nil	45	45	O.K.	
MS 11 (3).							
10, $\frac{7}{16}$ in. balls.	842	nil	263	53	50	O.K.	nil
		214	262	44	40	O.K.	nil
		380	220	24	50	O.K.	nil
		1280	nil	47	30	O.K.	nil
345 (1).							
10, $\frac{5}{8}$ in. balls..	1590	2050	nil	41	40	O.K.	+ 0·002
		1280	192	39	15	O.K.	− 0·001
		470	395	42	45	O.K.	− 0·001
		nil	497	50	35	O.K.	
345 (2).							
10, $\frac{5}{8}$ in. balls..	1590	nil	497	65	25	O.K.	+ 0·0025
		470	395	34	45	O.K.	
		1280	192	11	20	O.K.	
		nil	192	11	20	O.K.	?
		2050	nil	11	50	O.K.	+ 0·002
MS 16 (1).							
12, $\frac{3}{4}$ in. balls..	2650	nil	828	41	47	O.K.	+ 0·004
		1280	508	36	0	O.K.	
		2050	316	?		O.K.	
		3312	nil	41	15	O.K.	nil

All the above bearings had the usual filling slots in both rings and were fitted with cages.

Journal bearings—continued.

Type of bearing.	Listed safe radial load at 1400 revs. per minute.	Test loads. Radial.	Test loads. Axial.	Time run. h.	Time run. m.	Condition after test.	Change in side play.
	lb.	lb.	lb.	h.	m.		in.
MS 16 (2).							
12, $\frac{3}{4}$ in. balls..	2650	nil	828	27	25	O.K.	− 0·003
		1280	508	28	0	O.K.	
		2050	316	24	40	O.K.	
		3312	nil	63	5	O.K.	+ 0·001
MS 16 (3).							
12, $\frac{3}{4}$ in. balls..	2650	nil	828	48	25	O.K.	nil
		1280	508	42	10	O.K.	
		3312	nil	52	25	O.K.	+ 0·002
All the above bearings had the usual filling slots in both rings and were fitted with cages.							
35 J2, no filling slots in either ring.	853	nil	345	38	20	O.K.	nil
8, $\frac{1}{2}$ in. balls		1380	nil	41	10	O.K.	nil

The inference the author draws from these tests is that such bearings, and probably others, do not fail when the side thrust T and the applied radial load R comply with the formula $1{\cdot}5\,P = R \times 4\,T$, P being the listed limiting safe load.

Other Tests done on Bearing Rings and Balls.

Bearing Rings.—These are tested for hardness by the Shore Scleroscope. The indications of this instrument are to an extent dependent upon the dimensions of the article tested. A scleroscope reading of 80—90 is preferred.

The Brinell hardness test carried out with a small ball, radius r (cm.), the load being reduced to p (kg.) as given by the expression $\frac{p}{r^2} = \frac{3000}{0{\cdot}5^2}$ is independent of the dimensions of the article, provided that, as recommended by Moore, the distance from the centre of the depression to the edge of the surface tested be not less than five times the radius of the depression. This affords a means of, and is used for, correctly ascertaining the hardness of all sizes of bearing rings and of establishing a range of limits for use with the scleroscope. A Brinell hardness No. of about 700 is preferred.

The scratch test, in which a hardened rod of circular section is ground flat at the end, is a very useful one for testing ball bearing races. This can be standardised by providing standard reference blocks of different hardness and producing test rods of sufficient hardness to scratch a block of low limiting hardness.

Internal stress in bearing rings is tested for by measuring the curvature (with roller and micrometer) before and after grinding it through in one place. A minimum internal stress is preferred.

Balls.—These are tested for hardness by the Shore Scleroscope, the indications of which vary enormously with the size of the ball tested.

The graph (Fig. 23) shows the mean results of testing many balls of each size indicated, all of which were quite hard.

Compression Test.—A useful test is to press together two balls of the same size under a load sufficient to cause permanent compression and produce a measurable circle. As pointed out by Stribeck (Zt. d. Ver. deut. Ingr. 14.9.07), the effect of such loading is to produce a circular crack or cracks near the periphery of the "flat" produced, and which are best shown by lightly etching the ball. This is in line with the results of the investigation of flexure—which, it will be remembered, was a maximum at the periphery of the contact surface. The minimum diameter of "flat," with good resistance to crushing is preferred.

Crushing Test.—In this, three balls are placed on end and the end ones loaded until the middle one breaks. The ball we consider most perfect is one which breaks apparently owing to the pressure at the contact surfaces driving in cones which shear and burst the ball. High results are regarded with favour only when associated with a small flat in the compression test.

Etching Test.—Light etching with hot concentrated hydrochloric acid is very effective in detecting "pipe" porosity or lack of homogeneity. Likewise, etching with a weaker reagent serves a useful purpose in dispelling the brilliant polish on balls and races which too often masks surface scratches, grinding marks, and other defects, which even the most highly skilled of inspectors may fail to notice.

Tests on Raw Material.—Ball wire is tested for "pipe," decarburisation, seams and hardness.

Hardness is detected by bending the wire to and fro and counting the bends until it breaks. The length of the bent part is constant for each size of wire. Pipe, or want of homogeneity of the core, is

disclosed by longitudinal shear planes in the fracture: hardness by an insufficient number of bends before breaking: seams, if deep, will be apparent at the edge of the fracture.

Decarburisation of the surface is tested for by filing a notch in the wire deep enough to ensure its breaking there when bent once. If the fracture is lipped or drawn out at the edge, or if the wire breaks leaving only a small piece which acts like a hinge, the wire is decarburised. Doubtful cases are confirmed by an examination of the microstructure.

Bar Steel for Bearings.—This is tested for Brinell hardness to ensure proper machineability, and marked to indicate its hardness. All bars are measured and marked to indicate their size.

Brass Bar for Ball Cages.—This is also tested for hardness, measured for size and marked.

In addition to the foregoing routine tests, samples are regularly taken from materials as delivered, and sometimes as used, and subjected to thorough-going analysis and tests.

New materials are tried from time to time, and the small scale ball making plant installed in the Research Laboratory has proved itself very useful by trying special steels and variations of the processes employed with a view to arriving at the most perfect product obtainable.

THE DISCUSSION.

Mr. T. W. Cooper, in opening the discussion, said: I cannot claim to rank among the higher order of mathematicians, and it is therefore possible that I have not appreciated all the information that is given in the various calculations which the author has made, but close contact with the practical side of the question prompts me to make a few observations. On page 574 the author has referred to some experiments with regard to the expansion of a cylindrical ring when fitted to a plug. I should like to ask him whether the plug and ring used in that experiment were hardened.

Mr. Heathcote: Yes.

Mr. Cooper: Some few years ago my firm, the Hoffmann Manufacturing Co., conducted some experiments with ordinary inner races, i.e., the inner ring of the ball bearing, with various fits on a soft shaft, and I then found, using a bearing of something like $1\frac{1}{2}$ in. diameter in the bore, that there was no expansion of the inner race until an interference of 0·0005 in. was reached. After that, the ring expanded every one ten-thousandth added to the diameter of the shaft.

In the author's reference on page 590 to the effect of end thrust on journal bearings, I think he rather ignores or under-estimates the magnitude of the bursting effect which balls have upon the outer race when loaded as referred to. If we can visualise the two races as conical cylinders, then I think it stands to reason that if we force one into the other axially, with the balls as a rolling medium, there must be a considerable bursting strain on the one and a compressive strain on the other. Now, if we house the outer race in a solid housing, we are converting that bursting strain into a very intense surface pressure between the balls and the races, and it is for that reason that the bearings fail so quickly under end thrust.

The formulæ given on page 599 for the dimensions of the contact surfaces between the races and the balls appear to assume infinitely hard surfaces, and are, therefore, I think, rather more theoretical than practical. From some experiments made recently on this question, I obtained areas of contact varying from 0·03 to 0·049 sq. in.

(Mr. T. W. Cooper.)

with a $1\frac{1}{8}$ in. ball subjected to a load of 10,000 lb. with a 5 per cent track curvature. The author might, perhaps, like to check these figures with his formulæ. It is a little difficult to obtain these areas with exactitude, but I think that if something more could be done along these lines it would be a considerable help with regard to the determination of load-carrying capacities of bearings. It is a factor which naturally must vary in accordance with the softness of the material, and curvature of the race.

On page 611, the author refers to the nature of the coloured film produced at the contact surfaces, and he there quotes Eden, Rose and Cunningham, who state that the corrosion of ball bearings is usually due to the varying stresses between the test specimen and its holder, whilst his own experiments led him to believe that the coating which formed on steel surfaces when subjected to repeated impact, was oxide of iron. It is not quite clear to me whether the author intends this corrosion to refer to the balls and races or to the surfaces between the inner and the outer race and its shaft and housing respectively. If it is the latter, I quite agree, but I have never seen any evidence of such corrosion between the balls and the races. I was always under the impression that this oxidisation arose from the fact that there was a slight movement or breathing between the outer ring and its housing, or the inner ring and the shaft, which set free very small particles of steel which are subject to easy corrosion from any oxygen present.

Coming to the question of race curvatures, dealt with on page 620, in order to overcome the temperature rise on the inner races and the consequent expansion which overloads the balls and races, the author suggests the adjustment of the curvatures to evolve more heat on the outer race than on the inner race. To do this, the curvature of the inner race must be increased, and in so doing the ball contact is decreased. In decreasing the ball contact, the intensity of pressure is naturally increased. Now, I think it is admitted that the inner race is always the weaker member of a bearing, and I therefore do not think it advisable to put it at a greater disadvantage in this respect. It seems to me that a more feasible method is to allow an original slackness between the balls and the races, so that any expansion is allowed for in that slackness. It would not matter then, practically within limits, what the temperature of the bearing rose to, provided it did not get beyond the safe temperature for softening the steel.

Speaking of the load-carrying capacity and the size of bearing, the author states on page 631 that the area of contact in the (n) bearing is n times that of the other, and that it therefore follows

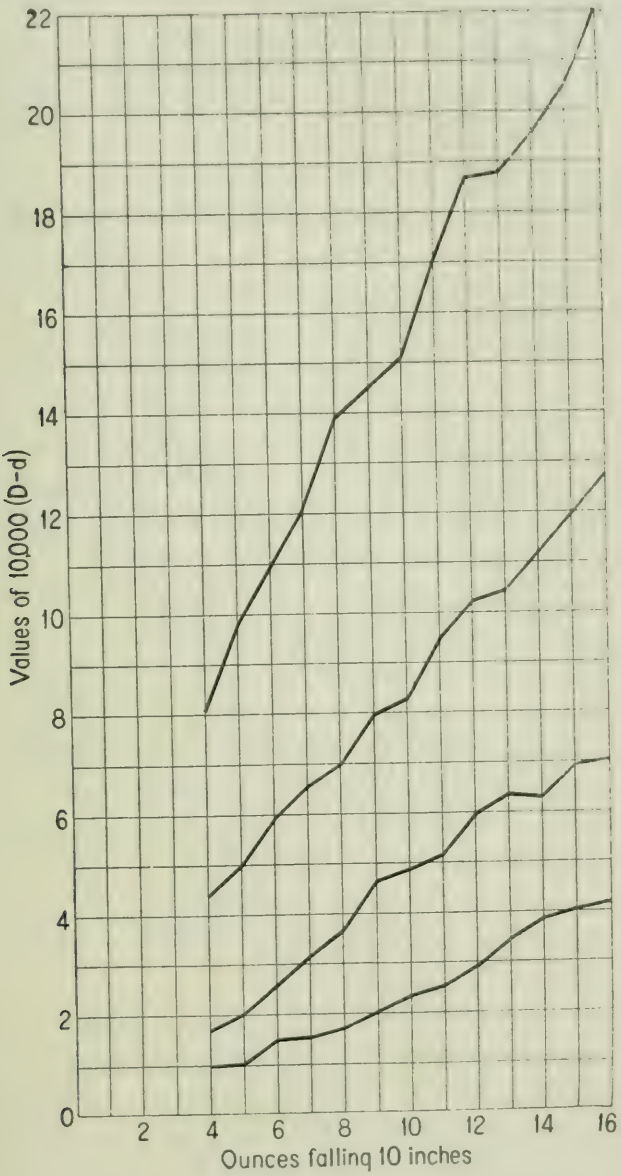


FIG. 26.

that the load on the (n) bearing must be n^2 times that on the other. Mr. Barrett read a paper before the Junior Institution of Engineers at Birmingham in March, 1917, in which he described a series of experiments which he conducted with three balls

(Mr. T. W. Cooper.)

so mounted that one was on top of the other. On the upper ball he dropped weights varying from 4 oz. to 16 oz. through a distance of 10 in. and obtained a dead flat on the balls which he measured; the results obtained are set out in Fig. 26. Along the vertical ordinate are shown the dimension of the flat in ten-thousandths of an inch. Along the horizontal ordinate is shown the value in ounces of the weights which were dropped. We have here four balls, 1 in., $\frac{3}{4}$ in., $\frac{1}{2}$ in. and $\frac{1}{4}$ in., and with a four ounce weight falling through the 10 in. on to the 1 in. ball, we get a deflection of 1/10000, or taking the 16 oz. weight falling through

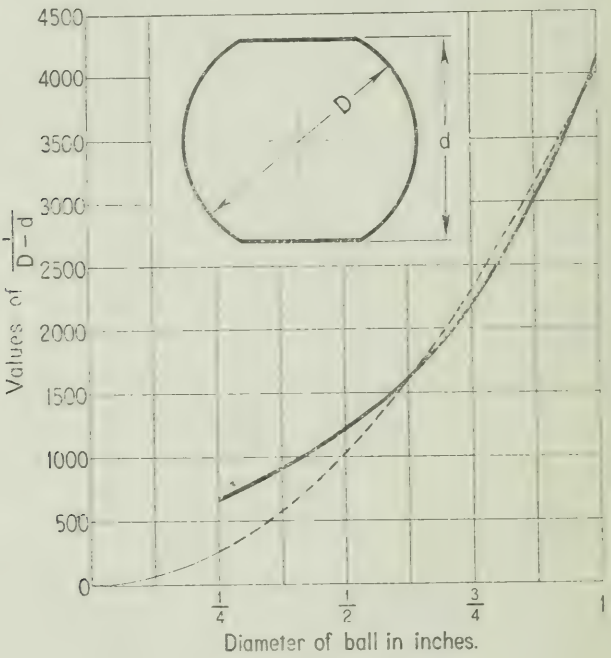


FIG. 27.

the 10 in. on to the $\frac{1}{4}$ in. ball, we get a deflection of 22/10000. I have plotted that in inverse ratio in Fig. 27; that is, an average of the deflections in Fig. 26, when inverted, i.e., putting $1/D - d$, gives Fig. 27, starting off with the small ball first and working upwards. Taking the $\frac{1}{2}$ in. ball as a basis and working from that as the square of the diameter, gives the chain chart curve, which is to a certain extent approximate to the one found by practice. It will be noticed, however, that this tails off very much at the lower end where the smaller sizes are concerned, whilst again at the top it is apparently running right away from

the practical curve, which is more or less straightening out. I think these results disprove the square root law, but I will admit that it covers most sizes of balls used in journal bearings.

I fear that the results of the tests given on page 659 may be somewhat misread, and would lead to some trouble if adopted by ball bearing users without consideration. In the first place, I think the length of the tests is hardly sufficient to warrant any deductions being drawn. No user of a ball bearing would be satisfied with a run of 65 hours 25 minutes, which is apparently the longest run on the list. Moreover, it is to be noted that the whole of these tests have been carried out by means of lever loading. We have established the fact that bearings loaded under any type of spring loading will have a considerably longer life than the same bearing loaded under a dead mass and supported in rigid pedestals. I think this is accounted for by the fact that in the latter case the balls and races have to absorb the whole of the shock set up by any inaccuracies or want of balance in the rotating mass, whilst with any type of lever or spring loading all these shocks are absorbed in the movement or the moving parts of the loading mechanism. It is for this reason, in my opinion, that the load tables published by many makers are rather misleading, and should not be used except as a comparison between one bearing and another, when any known results of any special application have been established. It is quite out of order to think that a bearing listed, we will say, at 1,000 lb. for 500 revs. per minute will stand a dead mass loading for the same period as it will if it were loaded to that same extent under a spring, so that the conditions of loading have to be taken into account very much when considering the application of anti-friction bearings.

Mr. A. E. Parnacott: Experience in the repair shop has led me to realise that it is almost impossible to keep dirt and foreign matter out of the bearings, and when the balls, and more particularly the races, are scratched, then the breakdown of the race occurs from that cause and not from excessive loading or bad fitting. I should like to suggest that steel washers forming a groove should be spot-welded electrically on to the respective races, with a floating packing or washer between them that would keep out the dirt, as indicated in Fig. 28. The great difficulty, of course, is, in fixing a casing, to do so without upsetting the usual working dimensions, but if my suggestion were adopted

(Mr. A. E. Parnacott.)

the races could be depended upon to do their work, however much they were abused and thrown about in the dirt of the workshop.

Mr. R. Allan: The mathematical investigation of the forces called into play within a ball bearing when in use is of great importance, and it is both pleasing and reassuring to note that independent investigators are reaching practically similar conclusions, and that some of the calculated values agree very closely with experimental results. Unfortunately, however, there are still conditions on which theory and fact are not in agreement, and the theoretical methods in use assume importance of a comparative character rather than that of finality. The hypothetic conditions of Hertz, enumerated on page 593 of the paper, still remain the basis from which the conclusions are derived.

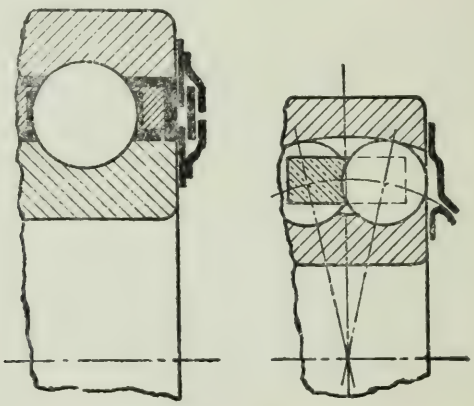


Fig. 28.

In dealing with the surface contact, one of these conditions, an important one, is that the dimensions of this surface are relatively small as compared with those of the contacting bodies. On page 601 of the paper it is also shown that the major axis of the elliptical contact surface for a Type 345 bearing is 0·214 in. in length. Now the circumference of the ball is only about 2 in., so that there is contact along one-ninth of its periphery in that plane, and it is clear that the surface in question is something very different from a relatively small quantity. Calculated and experimental values for the size of the surface are certainly in very close agreement, but we are facing a different problem when the stresses set up are being investigated. It is assumed that the intensity of pressure at any point in the surface is represented by the height of the pressure ellipsoid above that point. As

shown on page 631, this gives a maximum load stress at the centre and a zero load stress at the periphery of the contact ellipse. It is, however, not only the vertical stresses that affect the strain in the material at the periphery. Mr. T. Huber has investigated the strains in a circular area of pressure, and Sunberg those in an elliptical area, and according to these theories the maximum tensile strain in the case of an elliptical pressure surface occurs at the extremity of the major axis. This, however, conflicts with the results of experiments carried out with balls of different diameter, and contacting surfaces of different form. In such experiments the first crack invariably occurred at the extremity of the minor axis. The explanation of these differences lies in the fact that pressure surface is not small relative to the boundary surfaces of the contacting bodies. The author has dealt with flexure in a similar manner to that of Palmgren, and I would like to suggest that the sharp change in curvature at the periphery of the contact surface is worthy of fuller investigation. Apparently, the change in curvature will be greatest at the end of the minor axis, and this may account for the tensile cracks showing there first. In any case, it appears to be evident that the magnitude of the contact area is not the only deciding factor.

What we are unable to obtain from theory we endeavour to extract from experiment, but even here we must be conservative in our conclusions. Any reputable ball bearing firm can, in all good faith, give experimental evidence of good results. Such tests, however, are usually tests to destruction within periods short enough to be practicable. They require loading conditions of an abnormal character, and, no doubt, bring into play forces that would be unknown under normal service; their value, therefore, must be also considered as comparative. The author suggests that filling-slot bearings are even superior for taking thrust load, and on page 658 refers to experiments in which he obtained a maximum run of 18 hours 33 minutes with an axial load only of 1,590 lb., the balls being forced towards the slot in the inner ring. My Company (The Skefko Ball Bearing Co.) have also made tests on bearings of exactly the same size, double-row spherical and single-row, under a thrust load of 1,540 lb. in conjunction with a radial load of 4,400 lb. at a speed of 1,000 revs. per minute. The spherical self-alining bearing ran for 142 hours before fatigue failure occurred in the outer ring, whereas the single-row bearing only ran 77 hours before similar failure took place. Comparative

(Mr. R. Allan.)
tests of single-row filling-slot and non-filling-slot bearings were also made with the same sizes, the radial load being 3,520 lb. and the thrust load 880 lb., the speed again being 1,000 revs. per minute. In these tests, the best filling-slot bearing ran for 77 hours, and the poorest for 20 hours, before failure occurred at the filling slot in both cases. One of the non-filling-slot bearings was removed after 100 hours, and another after 122 hours, both being faultless when examined. Evidence of this kind, and the more satisfying evidence from practical experience, goes to prove that the filling-slot bearing is not the right bearing for dealing with thrust duty, and if further evidence is required, it may be said that certain makers of filling-slot bearings use the non-filling-slot type for heavy thrust duty. The filling-slot bearing usually has the filling aperture on the same side of the two rings, otherwise the balls could not be inserted. Because of this, thrust in one direction will force the balls into the slot on the inner ring, and if in the opposite direction they will be forced into the slot on the outer ring. On the test in which the author obtains a run of 32 hours without failure, it is stated that the balls were not forced into the slots. They must, therefore, have been cut on opposite faces of the rings and consequently have been of no service in getting in the maximum number of balls, so that the best result was obtained with fewer balls and under conditions equivalent to those of a bearing without slots—except that the rings were weakened by the unnecessary presence of the slots. I would add that when there is any misalinement of a shaft, the single-row bearing is at a disadvantage, and if there is thrust load in combination with misalinement, the disadvantage is greatly magnified, since it cannot then take the thrust over the full complement of balls. The author has also dealt with the question of the heat evolved in a bearing when it is operating, and he points out that the reduction of the curvature of the race will tend to correct this. If that is the cause of failure, the spherical double-row bearing has already taken advantage of that fact, and we thereby eliminate much of the heat effect which takes place.

Many other points deserve attention, but for the practical man it may be said that there is a correct bearing for every service, satisfactory and durable results being only obtained by proper selection.

Lt.-Col. T. M. Hutchinson: A large number of ball bearings which I have examined after failure do not show so much signs

of overheating, but they were defective due to deep pitting and fracture of the balls or the races. When analysed the reports gave various statements to the effect that the material was unsuitable or it had been wrongly heat-treated, and the general impression left in my mind is that these failures should not have taken place with correct manufacture. I specially noticed during the war, when we had a large number of bearings of foreign manufacture in use, that there was a great variation in quality; further, that whilst bearings of a certain make would run quite satisfactorily in one type of machine in a particular position, certain other makes invariably failed badly when employed under the same conditions. In those makes which failed I usually had to complain of the material or its manufacture. In some cases war finish and material accounted for this, but I do not think that this covers the whole story. Quite lately some bearings have been found defective which are obviously made of very unsuitable material. On analysis it was found to be high carbon steel that had subsequently been further carbonised until it has been brought back to material almost approaching cast iron. Serious difficulty was experienced in the fitting of these bearings on to their shafts and in their housings. A large number of expensive shafts had to be scrapped merely on account of a small amount of wear, due to what is commonly called "creep." Several cases occurred in connection with engine crankshafts, in which the housing had slightly enlarged, and we were faced with the problem of either scrapping the crankcase or attempting some method of retrieving it. Electro-deposition was tried, but I think that the design in the first place should have permitted of a proper grip of the ball race, so that extensive "creep" could not have been permitted, though provision may be necessary for some relative expansion of the crankcase. As a matter of fact, there was a great shortage of certain types of crankcases, and in these the existing housing was bored out to a taper and a tapered wedge-shaped packing piece was pulled in by half a dozen bolts. This was very laborious work, and had to be carried out under rather difficult conditions. One other little point is, that the cages which hold the balls together are sometimes much too light and allow the balls to get adrift into such parts as gear-boxes. Very light sheet metal or even bent wire has been employed in some cases, but I have found a solid substantial cage which was not likely to break up to be much more satisfactory.

(Col. H. J. Higgs.)

Col. H J. Higgs: I would like to make a few remarks on the paragraph regarding the fit of ball bearings, at the bottom of page 570, which begins as follows:—"Considerable difference of opinion exists as to the proper fit. . . ." I should say that the prime objection to standardising the diameter of housings and shafts is the considerable variation in the elasticity of materials and the desire to avoid excessive creep. As an instance, it is usually desired to fit bearings into aluminium tighter than into cast iron, as any tendency to creep would rapidly enlarge the housing to such an extent as to cause the bearing to lose its efficiency. Another difficulty which presents itself is this: to allow of some reasonable interchangeability of bearings, with shafts and housings, a very much closer tolerance is required than the average engineering firm would deem necessary. My experience of bearing users enables me to affirm quite positively that the proportion of these who are prepared to work to 0·0005 in. is exceedingly small, except in very special cases, and where the housings can be conveniently ground. Those who are compelled to work not only to a variation of 0·0005 in. but a tolerance of 0·0005 in. comparative with national standards, know the difficulty of such a proposition; certainly the majority of users I am acquainted with work in the general case to 0·001 in., and in larger sizes to 0·002 in., and even then no such precise inspection is put on the housings for roundness, parallelism, truth of faces with bores, etc., as is imposed by the bearing manufacturer on his product. This being so, then the amount of interference must be considerably more than that shown in Tables I. and II., which give the fits recommended by various authorities, and this interference is allowed for by the firm I am connected with (The Ransome and Marles Bearing Co.) by making the diametral clearance between the races and the balls suitable for accommodating this interference. To limit housing diameters to 0·0005 in. is asking the impossible from at least 95 per cent of ball bearing users, as unless the housing is to be ground (and in some cases grinding, is not possible owing to the situation of the ball race), this limit is not possible by any general manufacturing process known to me. However, I should like to know if the author has made any practical demonstrations to strengthen the statement at the top of page 574 which insists that the interference shall not exceed a few tenths of a thousandth of an inch, in so far as it is a purely English practice to have what is known as "side play" in bearings.

I have made inquiry of Continental experts and ascertained that their opinion is that side play is not necessary or desirable. The argument is put forward by them that when a bearing is loaded anywhere near its listed capacity the balls which are not under load are clear of the tracks, and that therefore no additional load is put on to the balls by means of the original interference by the fit, and this opinion seems to be worthy of consideration. I might also say that some experiments made by my firm on some Continental bearings rather bear out this claim. Perhaps the author will inform us if he has made any experiments on bearings fitted tightly with balls. During the war my firm made extensive experiments on ball bearings fitted to aero engines and the like, which were fitted without side play; the results, however, were far from satisfactory, and a considerable amount of side play had to be allowed. I venture to put forward the opinion that the reason for failure was not due to interference radially but to the errors in alinement which take place in such a flimsy structure as an aero engine under full power.

Another point, which appears to me to be of very great importance, is the question of ball clearance in the tracks. The present and past Continental practice has been to make the ball clearance considerably less than is the English practice; $1\frac{1}{2}$ per cent clearance seems to be the rule on the Continent, as against 3 per cent in our own works and 5 per cent in other makes of repute. Whilst it is claimed by the author that more heat is developed as the arc of contact increases, it is also reasonable to conclude that, given good workmanship and accurate formation of the tracks, a wider contact lowers the stress on the balls and the races to a considerable extent, and the additional heat generated would not be perceptible at average running speeds. These conclusions, I would say, have been fully borne out by experiments, and I should be pleased to hear whether the author has made any actual experiments of long duration, the results of which strengthen his opinion that a 5 per cent clearance is more suitable than a lesser one under general working conditions.

Mr. L. A. Legros: I was connected with ball bearing making as far back as 1894, and at that time we were in entire doubt as to what was the right way of handling steel for the production of balls. This was a much more difficult question than that of the races, because the races were of plain cylindrical form, and the thrust bearings had plane surfaces. It may be well, therefore, to

(Mr. L. A. Legros.)
repeat here what was then attempted. The steel was taken as cylindrical rod, and attempts were made to forge the balls from it. In one case they were cut off as blanks and stamped by one process so that the forged ball had an equatorial rim which required to be removed by shearing in a die. In another case, the balls were forged by a rolling process in a machine in which the rod was fed between two dies formed with progressively deepened grooves. In the Simonds American machine there were two vertical slides facing each other on which the dies were mounted. These slides were gear-driven, so that one ran downwards as the other ran upwards, the bar being inserted through a slot in a guard plate in front of the dies. The series of grooves of progressively increasing depth varied in form, the first having a very flat groove, the second one embracing rather more of the circle, and so on till the section was very nearly a semi-circle when the fourth or fifth groove was reached, both the dies being exactly similar. The distance between the dies also became progressively shallower from the entry end to the back, so that the operations of reducing the balls to spherical form were produced by regular stages, one ball being forged at each pass of the dies, and the rod advanced by the necessary amount for the next pass.

The end ball was twisted right off in the operation, and was left with only a very small polar projection at each end. Thus the amount of metal that had to be removed from the ball in the grinding process was very small, because the polar projections were much smaller than the equatorial projections formed by the other method.

There was another machine, called the " Werdna," that attempted, by a slightly different method, to attain the same result. It was very much like a vertical drilling machine, and had a spindle carrying a head underneath which there was another head rotating in the opposite direction. On the opposed faces of these two heads there were spiral grooves so formed that the rod was fed in at a point at which there was an opening so that it could move forward and become engaged with the ridges of the spiral grooves, which were formed similarly to the grooves in the Simonds machine; the grooves were, however, continuous, so that each ball was cut off as it was formed and the stock was fed continuously instead of intermittently. The ball forging was thus finished in the one pair of dies, and the process was virtually the same as that of the Simonds machine, but with the added

difficulty of maintaining the heat in the stock as it was fed into the machine.

The sizes of balls made by the rolling process were generally small; the majority were 3/16 in. to $\frac{1}{4}$ in. in diameter, but the machines could make them up to $\frac{1}{2}$ in. diameter.

What actually happened in these cases was that an effect was produced similar to that in the Mannesmann tube-rolling machine; that is to say, an axial core was formed, with the result that the balls cracked with a very small load compared with the balls of later days made by turning from the solid bar. The core line showed in the centre, and the cleavage was such as to give the appearance of the material having been subjected to shearing at some stage of the process. It is well to recall that such an apparently hopeful method of saving work in the making of the ball proved a failure in practice.

The Hoffmann Manfg. Co., Ltd. and the Auto Machinery Co., Ltd., used the method of turning from the solid bar, but I am not sure whether Ransome and Marles do. There are, I believe, only three or four firms making balls in quantity in this country at the present time. In the turning process the bar starts with being parallel, and the nicks are made progressively deeper by feeding a cutter knife radially, so that each ball gets more and more nearly finished as the knives work on the stock; the end ball is chucked and finished in an independent chuck carried in a separate head at the further end of the machine, so that even the point at which it has been parted off is finished completely; the automatic head then brings the bar forward and the operations are repeated. Although the balls made by the two processes I have described were accurate in form, they were much inferior in strength and reliability to those made by the Hoffmann process, which demonstrated that it was actually cheaper to throw away material and to cut the blanks out of the solid.

Mr. T. Clarkson: There is one aspect of the subject that has not received sufficient attention, and that is the point of view of the user. However well ball bearings are designed, manufactured and tested, they are no use unless they will stand ordinary everyday commercial conditions. This is particularly the case with commercial vehicles, where it has been found better to scrap the ball bearings used in the wheel hubs and replace them with plain sleeves. This is largely due to the fact that little attention has been given to the necessity for excluding dirt and moisture, so

(Mr. T. Clarkson.)

that when a wheel is taken off the bearing may be allowed to fall on the floor and be coated with dirt, and unless this is very carefully washed off before the bearing is replaced, the bearing will very soon be ruined. I suggest that ball bearings should, therefore, be so designed that the wheel can be taken off without exposing the balls to such risks. Nature teaches us the necessity of this by carefully enclosing all the mechanism of the body so as to protect the lubricant and to exclude any foreign substance.

It is certainly usual to put a dust excluding washer outside the bearing, and I understand that compressed cork has been used for this purpose, and I would be very glad to have further information on that point. Cork appears to have certain advantages over felt: it is not fibrous, it is elastic for many years and will resist either oil or water.

Mr. Max Lawrence: I was rather startled to see in the early part of the paper dimensions with four or five naughts after a decimal point, and I wonder how it is possible to even think of such figures. It does not appear in the paper how they are attained; we have a mathematical discourse upon the necessity for the exact shape of the race for the ball to run in, but to measure such a race to that degree of accuracy is a thing absolutely outside my experience as a practical proposition. In grinding the bearings where this accuracy is attained, the internal grinding is performed in the usual way, probably on a Landis ball-race grinder, which forms the radius of the ball path by swivelling the head and is dependent for accuracy upon the correct centre distance between the head and the axis of the grinding wheel spindle. We all know, however, that grinding wheels wear, and wear fairly rapidly, and therefore the amount of adjustment to be made, even in grinding one race, will certainly equal the tolerance to which the bearing has got to be made. Then the external ball path ground on the inner ring is evidently ground on a form wheel, and the wheel has to be trued with a diamond, and probably the centre of the wheel will wear rather faster than the edges, so that it would not appear possible to get very minute accuracy on the shape of these paths—certainly not to 0·0005 in. I do not see in the paper whether the effect of the one race wanting to go round the shaft or to rotate in its housing is treated mathematically, but I should be interested to know what forces affect that tendency. In a bearing like that of a gear-box, the outer race should always remain stationary and the inner race only revolve, though that should also remain stationary on its shaft.

Mr. O. D. North: I should like to ask the author whether he has equated the theoretical amount of heat developed by the various items of friction in the bearing to the total frictional resistance of the bearing, which, of course, can be determined quite easily, and, if he has been able to make any satisfactory comparison, what coefficient of friction he would deduce from that comparison. I am very pleased that the author's views are supported by theoretical evidence, because they agree with my little practical experience. For instance, although I cannot claim to have studied the reason, I have always found that bearings with brass or gunmetal cages of solid construction giving a fair amount of surface contact of the ball, wear very much more satisfactorily than bearings with cages made of twisted wire or mutilated steel of various forms. I think that a pressed steel cage which is first drawn into a particular shape, then notched in a press and then doubled back, is liable to fail if there are any flaws in the steel, and that is enough to ruin the whole bearing. I am glad the author states that a single-row journal bearing will take end thrust satisfactorily. Probably the makers of ball bearings in the past were well advised to say they would not, because in the old days engineers did not know much about ball bearings, yet I have found there are many cases in which a single-row journal bearing is a more effective thrust-carrying member than an ordinary thrust bearing. In one case of a large tractor built before the war with a 105 h.p. engine, an external cone clutch was kept in by an external spring, and this load, amounting to 700 to 800 lb., was satisfactorily carried continuously by a $2\frac{1}{4}$ in. medium journal bearing. I got into correspondence at one time with the makers of a certain bearing, and asked them to tell me what were the maximum end loads that their journal bearings would take under conditions where there was no radial load, and they were kind enough to test a 3 in. medium ball bearing under end load, and they ran it with seven tons for a considerable period. My idea was simply to get an idea of the maximum load that it would take as a hub bearing when the car skidded into the curb, and seven tons satisfied me, because that was enough to bend the axle-shaft. I am relieved to find that single-row bearings can be trusted to take end thrust, because this allows the number of parts of a motor car to be cut down by dispensing with a thrust bearing. I would instance the front hub of one car, in which two single-row journals are used butting against shoulders in the

(Mr. O. D. North.)
hub and drawn together to nip a tubular distance-piece on the stub axle by the usual nut. The precise amount of end play—or, alternatively, great initial end load—depends on a number of overall dimensions being correct (which they never are in practice), and only careful trial assembly and subsequent adjustment of the length of the distance-piece can give satisfactory running.

I have used a four-point bearing on a steering column of a screw and nut gear, in which the screw and spindle rotated in the nut and had no other bearing at the lower end, but at the top was a four-point bearing, 45 degrees angle, twenty $\frac{1}{2}$ in. diameter balls in a groove, the outer ring being in one piece and the inner cone being two separate cones with two lock nuts on the outside, and this works perfectly.

I would like to ask for more information about creeping, because the damage that creep can do is rather mysterious to me. The makers of one roller bearing told me that if a bearing cannot be made a force fit on a shaft better results will be obtained if the inner race is keyed, and I think they provided a notch on the end of the race to engage with a lug on a collar to prevent it from rotating.

I have always understood that creep is simply due to the fact that the bore of the race is larger than the shaft. There must be a difference of angular velocity as the shaft goes round. The radii are so nearly the same, however, that I should not think it would have any effect with a reasonably hard material, and that the pin to prevent it slipping would cause more trouble than if the race were allowed to slip round the shaft. I must say also that my experience does support the fact that the single-row bearing is quite as good, for most applications, as the bearing with the spherical outer race. It is very much better in some respects, particularly in ease of assembly.

Mr. C. R. Garrard: Mr. Legros referred to making ball bearings in 1894, but I made the ball bearings for the tall ordinary bicycles in Paris in 1882. Some speakers have mentioned the question of creep, which was dealt with very exhaustively by Prof. Goodman. I know it is the practice of Messrs. Hoffmann very often to leave a small degree of play in the bearing to obviate the ill-effects of driving it in and/or on tight and getting an enormous pressure on the balls. Another idea, covered by a patent specification, published some time ago, was to fit a sleeve into the housing of the form shown in Fig. 29, and that prevented

the bearing from being overloaded, because the sleeve was elastic to a very small degree, and that would obviate to a great extent the necessity for leaving slackness in the bearings, while the smaller surface in contact tended to resist creeping. Mr. Clarkson spoke of protecting ball bearings. In the Mercedes Co. in Germany the practice is to put a thin steel washer up against the bearing and let it rub on the outer surface. It is very thin and the rub is very light, but it makes the bearing practically dirt proof and watertight. The Mercedes Co. do that on the whole of their bearings, and it applies to the new design of cars at Stuttgart at the present moment. Doubtless the author will deal with the question of radius grinding, but in reference to Mr. Lawrence's

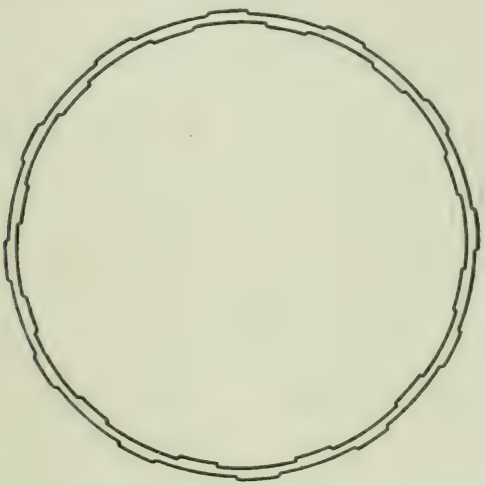


FIG. 29.

remarks I would say that the work curves round the wheel at the correct radius all the time, and the wear on the wheel will not alter the radius in the least. The wheel is fed up to compensate for the wear and the radius is always constant.

MR. G. M. JUNNER: I should like to endorse the remarks of some of the previous speakers with reference to the importance of creep of ball races in their housings, which in the case of a very large number of commercial vehicles which are being overhauled by the manufacturers themselves, has caused a great deal deal of expense. I have seen very many housings scrapped because of the wear, and various expedients have been resorted to. Among these is the deposition of iron by electrical methods. Another manufacturer knurls the housing and the shaft. That

(Mr. G. M. Junner.)
cannot be done with a hardened shaft or with all housings, but I am informed that it has proved fairly satisfactory in a number of cases, although, in the opinion of many engineers, it is only a temporary way out of the difficulty and does not delay the need for replacement for any considerable length of time.

Mr. R. Allan: I think one point has been omitted in regard to the question of creep. If a shaft is rotating within an outer fixed ring, the load comes on one point on the ring, and it is well known that fatigue is the cause of most of the failures. The fixing of the ring allows fatigue to be set up, and will eventually destroy the ring in that particular place. Therefore, I think it is necessary that the ring should be allowed to creep a little so that different points on the ring will be brought under the points of loading.

Mr. John Pugh: The President's recollection of seeing the manufacture of balls at the Rudge Works when he was a student at Mason College (a year before my time) makes me think that some details of the process then employed might be useful.

The Rudge Co., who had been making ball bearings for some years, began to manufacture the balls themselves in about 1880. The two processes referred to by Mr. Legros, namely, turning the blanks from the bar and forging them in strings, were both employed, though the former predominated. The process of finishing was closely analogous to that at present in use, except that the grooved disks had a vertical axis and were only about 12 in. diameter. Balls of moderate accuracy were turned out by interrupting the grinding process by separating the plates and examining the balls, but there was no guarantee or even likelihood that the balls in one groove would at any stage be the same size as the balls in another groove, and there was no method of changing from groove to groove. Also the quantity operated on at one time was extremely small. The real technical advance which was due to the Hoffmann Co. was the employment of a hopper, which operated as a changing box, so that the balls at the end of one circuit entered the hopper and then by the law of probability entered another groove. This, and the increase in the size of the grooved disks, not only enabled a much larger quantity of balls to be operated on at a time, but the very small amount of grinding done at each revolution, and the fact that the balls changed from groove to groove at each revolution, guaranteed that all the balls being operated on in one machine were

at every stage identical in diameter and finish. The fact that this patent was not upheld in the action against the Auto Machinery Co. is an illustration of the difficulty of getting a legal court to appreciate a technical advance of vital importance.

Very broadly, the extraordinary degree of spherity and accuracy obtained in balls can only be obtained by arranging that very little work is done at a time on a ball, and that it is done a vast number of times. To obtain a greater accuracy it would only be necessary to prolong the time and reduce the amount of work done at a time on the ball.

Our own practice in making blanks is to head them up cold. By this process the metal is not tortured, as is the case in the rolling method, where a certain amount of twisting goes on along the axis of the ball. In the heading method the wire is cut into very short lengths and put between two hemispherical dies and squeezed to its spherical shape. It is, of course, necessary to cut the blanks off very accurately, so that when squeezed the ball will just fill the sphere formed by the dies and produce no fin. The forming of the fin shows an external distortion, which does not much matter as it is not costly or difficult to remove, but this is accompanied by an internal distortion of metal flowing towards the fin, which seriously affects the quality of the ball. We believe that it is because we avoid the formation of the fin that we are now obtaining better results from headed up balls than from those turned out from the solid.

There is one other point which Mr. Clarkson has already mentioned, namely, that the bearings on vehicles ought not to be disturbed when wheels are taken off for tyre replacements. With motor cars that is current practice with most systems of detachable wheels, and that, I think, is why the wheel bearings suffer much less than they do on commercial cars. Another reason is that commercial cars are usually fitted with solid tyres, which are much more severe on every part of the chassis, and especially, I think, on the bearings. An illustration of the effect of solid tyres on every part of the chassis is that while we have long since ceased to have any trouble whatever with spokes breaking in wire wheels fitted with pneumatic tyres, we have utterly failed to make a wire wheel stand up with solid tyres, although we have made two or three serious attempts to solve the problem on occasions separated by a number of years. I believe that the experience we are now having with pneumatic tyres on commercial vehicles will prove that they are economical in use even with the

(Mr. John Pugh.)
present chassis, that they will permit re-design of the chassis, giving a still greater economy, that they will have less damaging effects on the roads, and that they will become universal on economic grounds alone; and that on other parts of the chassis it will be found that ball bearings stand up as well as they do on other pneumatic tyred cars.

Mr. G. H. Rodway: The point which appeals to me as a technical man is the application of any formula, or capacity tables of anti-friction bearings, to purposes of selection of bearing sizes. The anti-friction bearing manufacturers may give us certain load-carrying capacities or formulæ, but how are we to apply this information? Are we to make comparisons, and if so in what ratio, with the capacities given or thus obtained, with, for instance, in case of hubs, the loads which the bearings have to carry obtained from the static load on the tyres, or with the stresses brought about by side-slipping, going round corners, etc.? Again, in the case of gear-box bearings, are we to use bearing loads obtained by the maximum torque which the engine develops, or on values derived from an average output? It is obvious to me, that the bearing sizes can only conveniently be arrived at by experience, as there are so many factors affecting their useful life, but the motor car manufacturer's experience is very often limited to a few models, and consequently he cannot have sufficient information accumulated to make the best selections. It is, therefore, my opinion, that it is far better for manufacturers and designers to submit their bearing problems to the makers of anti-friction bearings in order that they may have full economy in every sense in their designs. Anti-friction bearing makers with proper business acumen will see that they are equipped to give their clients the full advantage of their special knowledge and experience.

Dust excluders, referred to by Mr. Clarkson, are very important as determining the useful life of anti-friction bearings, and the Packard people told me while I was in the States in 1920, that they were starting to use cork or cork composition in their front hubs. A useful form of wet and dirt excluder, and incidentally, of course, a grease retainer, although it is, perhaps, slightly more expensive, is an all-metal device. The faces are of zig-zag shape, the object being to give a circuitous route for the dust and dirt, and by the same means retain the lubricant. This device has proved satisfactory for the purpose intended.

On the question of holding outer rings of bearings in their

housing, there is a method used largely in the States by certain roller bearing manufacturers. The housing is split, and between the top and bottom half are placed shims approximately two to three thousandths of an inch thick, the housing being bored out exactly to the size of the outer ring. When assembling, the shims are removed and the cups tightened down. It appears to be rather a crude way to mount ball bearings, but I offer it as a contribution towards the discussion. The shims could, of course, be reduced in thickness during the boring operations so that there will be less liability of serious distortion.

Mr. T. S. Mercer: I should like to ask the author whether he has experienced any trouble through a certain percentage of otherwise perfect ball bearings being noisy in operation. Just prior to the war I was engaged in the experimental department of a firm of motor car manufacturers who were endeavouring to improve the silence of the various units of their car, and we occasionally found a bearing in which neither we nor the makers could find any error, yet which was excessively noisy when running.

This trouble was sufficiently serious to necessitate a machine being designed to enable all ball bearings to be tested for noise before being passed to the assembling shops for use. This machine was of quite a simple nature, being somewhat similar to a small lathe head, the inner race of the bearing to be tested being mounted on the spindle and driven by a belt at a speed somewhat higher than the maximum speed the bearing would be run at in use, the outer race being held stationary. The machine was isolated in a small room, and at first quite a large percentage of bearings were rejected for noise.

The makers were rather surprised at having bearings rejected on no other account than noise, and they sent their representatives to investigate the matter, with the result that, I understand, they subsequently made a similar machine and tested their bearings for noise, after which the percentage rejected by us for noise was reduced from something like 15 per cent to about 1 per cent. It is presumed, however, that this improvement was entirely due to this selective process, and not to any improvements in manufacture.

In view of this experience I would like to know whether Messrs. Rudge-Whitworth have met with any trouble in this connection, and whether they submit their bearings to any test for noise.

(Mr. Heathcote.)

Mr. Heathcote, in replying on the discussion, said: Mr. Cooper referred to the expansion of the outer ring owing to side thrust. That doubtless affects the angle taken up by the ball, but I do not think it makes the bearing less capable of withstanding side thrust. I think it is likely to increase its ability, if only for the reason that it brings the axis of the ball more into line with the direction of the thrust. When properly housed, the rings will not yield very much. The ball is an exceedingly elastic thing, and it will yield in preference to the ring. It is, of course, quite easy to calculate how much the ring does enlarge in diameter. With regard to the nature of the coloured film, I think Mr. Cooper agreed that the corrosion between the shaft and the inner ring, or, perhaps, between the housing and the outer ring, might be due to heat, but he did not think that heat could cause it in the case of the ball and track. Working among things like this, the investigator soon becomes convinced that the causes are the same, and he seeks evidence for it. The particular evidence which I have found is that, after removing the coloured bands from an over-loaded ball race, dark and light bands are produced by dipping in an etching agent, which has the effect of colouring a tempered part darker than a part which is not tempered. This is sufficient proof to my mind, and I do not think there can be any doubt about the coloured films being the same. Furthermore, the depth of red and the depth of the colour produced by etching follows the curve in Fig. 16. That curve is intended to show where the hot parts are, and the agreement in itself is sufficient to my mind to show that heat is most probably the cause of the oxide.

With regard to the question of creep, which was mentioned by Mr. Junner, Mr. North, and Mr. Lawrence, may I ask them if creep is noticed only with the revolving elements? Is that the creep referred to?

Mr. Junner: In my case it was on the element which should be stationary.

Mr. Lawrence: It is both, and chiefly on the revolving element.

Mr North: It was the revolving element in all cases except one, in which the housing was of a relatively soft material.

Mr. Heathcote: I cannot conceive that the non-revolving element should wear very much. Of course, it is quite easy to explain why the revolving element does wear if the fit is not an interference fit.

Mr. Garrard: The elasticity of the housing would account for it.

Mr. Heathcote: The interference must be sufficient. The difference in diameter between the shaft and the ring, the shaft being larger than the ring, is the interference, and it must be sufficient. If the interference is not of sufficient amount to prevent the ring, when loaded and run, from assuming a diameter greater than that of the shaft, the ring and shaft will revolve relatively to one another. If that happens, Prof. Goodman has pointed out that a tremendous pressure, e.g., 5,600 lb., can be developed, which would produce a shear stress of 46,700 lb. per sq. in. on the pin which he employed. The tangential forces are so high because we have here to do with sticking friction and not sliding friction, and a coefficient probably of 0·2 to 0·5; certainly more than 0·1.

Mr. Allan mentioned some investigations which had gone to prove that the flexure was greatest at the end of the major axis, and yet experiments have shown that it is greatest at the end of the minor axis.

Mr. Allan: I referred to the tensile stress at the end of the major axis. The stress had been calculated to be greatest there, and I suggested that the stress may be greater at the minor axis.

Mr. Heathcote: Yes, it is greatest at the end of the minor axis. It is worked out in the paper. I should think it goes to show that the other calculations are not correct if experience goes against them.

Col. Higgs asked whether the 5 per cent ball clearance is satisfactory for all practical purposes. I think there are some practical purposes for which it would be unsatisfactory, but for the majority of practical purposes undoubtedly the present percentage is a very good one.

Mr. Lawrence asked about the gauging of the balls, and how the very fine measurements were made. There is a very simple way of doing it which has just been described in "The Engineer." If a micrometer is clamped to the top of a spring balance and locked so that it just holds a Johannsen gauge, and the gauge is then pulled vertically downwards, the needle of the balance will turn through a certain angle which is a measure of the friction. It is found possible to pass between the same anvil faces a gauge which is one ten-thousandth of an inch larger, the friction force being greater for the one size than for the other. That can be done

(Mr. Heathcote.)
for any size, and with the calibrated spring balance used as an indicator of the size, it is perfectly easy to magnify the one hundred-thousandth of an inch into half an inch. That is an instrument which we use largely, and have been using for some time. That is one way of doing it, but there are others. The principle of the ball gauging machine is the same.

COMMUNICATIONS.

Mr. A. J. Rowledge wrote: I was interested in the data given in the paper with respect to the question of journal bearings taking end thrust. In the King-Bugatti aero engine—of about 400 b.h.p.—built in America, the air-screw shaft is carried in two ball bearings only and driven through spur gearing. The thrust of the air-screw is taken on the radial bearing immediately behind the screw. This bearing contains ten balls $\frac{7}{8}$ in. dia., the races have deep grooves and, I presume, no gap. The engine is described in Sherboudy and Wardrop's book on aero engines, where it is stated to have run a fifty hour test satisfactorily, using a propeller to absorb the power.

The arrangement is particularly good from the user's point of view as it entirely obviates any maladjustment of the bearings besides simplifying the construction.

Mr. A. M. Arter wrote: I would like to give the following additional information with regard to the load-carrying capacity of ball bearings from the point of view of the balls themselves. Prof. Goodman stated* that "*the safe working load for ball bearings* is dependent on the speed of rotation of the balls themselves."

Hertz's theory showed that, for a given load, the maximum stress in the material was smaller the greater the extent of the contact between the balls and the races, and, therefore, it was desirable, in order to maintain the maximum bearing capacity, to make the radius of the groove of the inner ball race 4 to 5 per cent larger, and that of the outer ball race 10 to 15 per cent larger than the radius of the balls.

For a single-row radial bearing the areas of contact were calculated by Mr. G. J. Wells, and are given in full on p. 284 of Vol. VI. of the Proceedings of this Institution. The figures there given confirm Hertz's theory, the small difference in area of 0·00006 sq. in. for the above proportion of ball diameter and inner and outer race track diameters approximating to all intents and purposes to an equal bearing surface in both race tracks for the balls; in which case equal wear of track would be contemplated.

* See Proc. Inst. C. E., Vol. CLXXXIX.

(Mr. A. M. Arter.)

Method of determining the overload of balls.—Prof. Goodman's method* was to examine balls under a microscope, by which he was able to ascertain that if a ball was run with an overload it appeared to be covered with tiny flakes of snow, which are specks where tiny crystals have broken away from the surface of the ball, and that if the test were continued at this load, the balls and races would ultimately fail. Taking the foregoing considerations into account, I drew up the following formulæ for the permissible loads on ball bearings of the best materials and workmanship, and with curved tracks, giving approximately the same area of contact for the inner and outer race tracks of journal bearings and also equal area of contact for thrust washer tracks.

The formulæ will be found to coincide very closely with generally accepted practice for the diameters of balls most generally in use, say up to $\frac{3}{4}$ in. dia., *vide* makers' recommendations with regard to load, revolutions and size of bearings; the formulæ will include, therefore, the usual factor of safety included in makers' tables.

Thrust bearings—

$$W = 700nd^{1\cdot45}\sqrt{\frac{320d}{ND}}$$

Journal bearings—

$$W = 900nd^{1\cdot45}\sqrt[3]{\frac{320d}{ND}}$$

where

W = load in lb.
n = number of balls.
d = diameter of balls in inches.
D = dia. of ball track in inches (thrust bearings, centre to centre of opposite balls; radial bearing, dia. of inner track).
N = revolutions of shaft per minute.

In the case of thrust bearings, all the balls carry the load simultaneously.

In journal bearings, according to Hertz, only one-fifth of the balls take the load at one time. This has been taken into consideration in preparing the formulæ, and they should be used as per the notation.

* See Proc. I.A.E., Vol. VIII., p. 125.

In thrust races, a satisfactory permissible load has been found to vary inversely as the square root of the number of revolutions, and in journal bearings, inversely as the cube root of the revolutions.

I am glad to see that the author states on page 613 that, according to Clerk Maxwell's Theory of Heat (page 265), the length of heat waves is proportional to the square root of the periodic time,

$$\lambda = \frac{1}{\sqrt{N}}$$

or the velocity of propagation will vary as $\sqrt{N}$, and that the stresses must be reduced as the speed is increased.

This, according to the present paper, applies to the ball tracks in the races, and is in agreement (as already mentioned) with the statement that the load on thrust bearings must vary inversely as the square root of the revolutions. An interesting fact with regard to journal bearings, which is a corollary to my formulæ, is that if a journal bearing has the centre race rotated, the load which the balls will stand is much greater than when the other race is rotated at the same revolutions.

This fact can easily be corroborated by taking a journal bearing of sufficient size to see the balls in their cages, and rotating firstly the inner race one revolution and noting by means of a blue pencil mark on the cage the angle of rotation of a ball centre, and then rotating the outer race, keeping the inner race stationary; it will be found that the balls all rotate through a greater angle when the outer race is rotated and the inner stationary than *vice versâ*, according to the proportion which their diameter bears to the diameter of the tracks. From this it will be seen that a journal bearing will not stand the same load when the outer race is rotating as it will when the inner race rotates, at the same revolutions.

An application of this method of rotation is found on all front wheels of motor cars, and this fact, as far as I know, has not been pointed out before, and should be taken into consideration when deciding upon the ball races for front wheels of cars. To make the statement complete, ball bearings for front wheels of cars should have a greater factor of safety, because of the inertia effect on them of bad roads.

Professor J. Goodman wrote: This is a very valuable paper which will doubtless be referred to for many years to come by all

(Professor J. Goodman.)
interested in ball bearings. It contains a vast amount of information which possibly may not be appreciated by all at the present time, but as the subject advances, the value of the author's work will be more and more apparent.

With regard to the fit of a ball bearing in its housing, I have come to the conclusion that the outer ring should be mounted with a soft backing, such as linoleum, wood, vulcanised fibre or other similar material, since bearings so mounted run much more silently than when fitted into a rigid housing. The soft backing acts as a cushion and thereby materially reduces the stresses in the balls when the bearing is subjected to vibration and jars.

Failures with ball bearings are often due to imperfect workmanship when mounting, or to subsequent lack of alinement, but if a soft backing be used, such inaccuracies are of little consequence.

I wish the author had abstained from using absolute units of force, poundals, in a paper intended for the use of engineers, who regard such terms as only suitable for physicists whose problems begin and end on paper.

With regard to the lubrication of ball bearings, there are some instances in which oil or other lubricant may cause trouble. On more than one occasion I have had experience with a ball bearing which ran cool at very high speeds and without giving any trouble when kept perfectly dry and free from oil, but on adding a few drops of a high-class light mineral oil the temperature ran up very rapidly, causing the bearing to smoke to such an extent that the machine had to be stopped. For bearings running at slow and moderate speeds some lubrication is desirable, but mainly for the sake of preventing rust.

I still maintain that it is bad practice to allow journal bearings to take side thrust. The author's treatment of the subject is, in my opinion, defective, since he, and others who have written upon this subject, neglect the very important point that the outer ring cannot remain concentric with the shaft when supporting a radial load, and consequently some of the balls take far more of the end thrust than others, the amount depending entirely upon the amount of radial play in the bearing. There is need for experiment on this point.

Dimensions of the contact surface.—I have recently made some experiments to determine the areas of the surfaces in contact between balls and flat plates, and find that the results very closely

agree with values calculated from Hertz's formula, the experimental values being slightly greater than those calculated. The approach of two flat plates separated by a ball was also measured and compared with Hertz's values. The two are closely in agreement.

On page 610 the author says: "If the balls in a bearing vary in size even ever so little, the larger ones will tend to complete their circuit round the axis of the bearing more quickly than the smaller ones." I do not think this is correct since the "slip" increases as the load increases. It is evident that the load on the larger balls is greater than that on the smaller ones; hence the slip on the larger balls is greater than that on the smaller ones, with the result that they travel less quickly than the smaller balls.

On page 614 expressions are given for the relative load-carrying capacity of bearings at different speeds based on the velocity of propagation of heat waves in the balls, and it is stated that for high speeds it is usual to reduce the load in practice according to the relation given. My own experimental results, however, which were obtained a few years ago, differ materially from the author's expression. I am now carrying out further tests at high speeds, and as far as they have gone the results confirm those which I previously obtained. A comparison is interesting.

Revs. per minute.	Thrust Bearing. Relative Load.		Journal Bearing. Relative Load.	
	Heathcote.	Goodman.	Heathcote.	Goodman
100	1,000	1,000	1,000	1,000
1,000	316	135	465	357
10,000	100	14	215	48

My own tests conclusively show that the "safe loads" given in makers' catalogues for high speeds are far too high. I arrived at the safe loads by running bearings under given conditions and from time to time examining the balls under a microscope; overloading for a given speed is at once evident by the appearance of minute crystalline specks. Balls which show this defect may run for a long time before failure, but in the end they always give

(Professor J. Goodman.)

out, the specks grow larger and finally the balls peel. I am not aware that any maker of ball bearings uses a really satisfactory means of detecting failure in the early stages. If bearings have to run until they fail, much time is wasted.

Gyrostatic action.—The credit for pointing out the gyrostatic action on the balls of a thrust bearing is attributed to Mr. Guest, but Mr. F. W. Lanchester previously called attention to the matter in 1908 in his paper before the Institution of Automobile Engineers on "The Design of the Automobile." *

The value taken for μ, namely, 0·1, is too low—my own tests give a value of about 0·3.

The wear tests given on page 657 are especially interesting to me. I believe I was the first to point out (Proc. Inst. C. E. Vol. CLXXXIX.) that the smaller balls in a bearing wear more rapidly than the larger balls. These figures give the first independent testimony to my statement.

I hope that the author will continue the excellent work he has taken in hand and will give the results to the engineering world.

In further reply to the discussion, and in reply to the written communications, Mr. Heathcote wrote: The results of the falling weight tests described by Mr. Cooper are interesting, but I should not care to infer that they prove that the area of contact in the n bearing is not n^2 times that of a similarly stressed unit bearing. In the main, the experimental results in Fig. 26 lie along four straight lines, i.e., the radial flattening is proportional to the applied dose of energy, but it does not follow that the force due to the blow is proportional to the dose of energy. Furthermore, the lines, when continued, do not all pass through the origin: for instance, that relating to the $\frac{1}{4}$ in. ball cuts the vertical co-ordinate axis at about 4, suggesting, to my mind, that either the amount of the radial flattening or the force of the blow is not quite as plotted. Quite apart from these objections, it may be urged that data derived from measurements of permanent deformations can hardly be relied upon to challenge a relation which is supposed to hold for stresses within the elastic range.

I have, as suggested by Mr. Cooper, worked out the contact areas for a $1\frac{1}{8}$ in. ball on a raceway loaded with 10,000 lb. The track diameter assumed is 7·625 in., and as the form of the

* See Proc. I.A.E., Vol. II., p. 187.

raceway is not stated, the calculations are made for three cases: (1) for the outer ring of a journal bearing, (2) for a thrust washer, and (3) for the inner ring of a journal bearing. The data upon which the calculations are based are:—

Ball, $r = 0{\cdot}5625$ in.

Track assumed concave (outer ring), $r_1 = 3{\cdot}8125$ in.

Track grooves, $r_2 = 0{\cdot}5906$ in.

Load, $P = 10{,}000$ lb.

Elastic constant, $E = 3 \times 10^7$ lb. per sq. in.

Substituting these values in the expression for q on page 600 of the paper, we find $q = 0{\cdot}0832$ in.

Substituting the same values in the expression for cos τ on page 600, we find $\tau = 25\frac{1}{2}$ degrees.

Reference to Figs. 10 and 11 gives, for this value of τ, $\mu = 3{\cdot}25$ and $v = 0{\cdot}45$, whence the ratio of the axes of the contact ellipse is 7·23; the width of the ball track 0·54 in. and the area of contact $= \tau\ (3{\cdot}25)\ (0{\cdot}45)\ (0{\cdot}0832)^2$ sq. in.

$= 0{\cdot}0318$ sq. in.

and average load stress = 140 tons per sq. in. Mr. Cooper finds (by experiment) about 150 tons per sq. in.

If the bearing be a thrust bearing, r_1 is infinity; in which case $q = 0{\cdot}079$ in.; $\tau = 24$ degrees 5 minutes; $\mu = 3{\cdot}3$; $v = 0{\cdot}44$; the ratio of the axes is 7·5; the width of the ball track 0·522 in.; the area of contact 0·0284 sq. in. and the average load stress 157 tons per sq. in.

If the bearing be a journal bearing the inner ring of which has a track dia. of 7·625 in., $r_1 = 3{\cdot}8125$ and $q = 0{\cdot}0755$; $\tau =$ 22 degrees 20 minutes; $\mu = 3{\cdot}6$; $v = 0{\cdot}43$; the ratio of axes is 8·4; the width of ball track 0·544 in.; the area of contact 0·0277 sq. in. and the average load stress 163 tons per sq. in.

All these figures are approximate, and it is interesting to note that those for the outer ring agree very closely with some of Mr. Cooper's experimental determinations.*

Mr. Cooper does not consider it warrantable to draw any deductions from tests the duration of which is only about 65 hours, or that any user would be satisfied with such a run. Users of firearms accept the proof-house test in which two charges of more than the usual weight are fired, and I think a 65 hour test on a

* Since the above calculations were made Mr. Cooper has informed the author that his experiments were made with a ball and an outer ball race.

(Mr. Heathcote.)
bearing under the kind of load users are often warned not to employ shows that they may apply such a load to such bearings without detriment for 65 hours, and, since the bearing was not detrimentally affected in any way, probably for much longer.

The difference in the behaviour of bearings under dead weight (or rotating mass) and spring loading is proof of eccentricity in the bearings tested. Truly concentric bearings will, ceteris paribus, give the same results whether the load be applied by a dead weight, a lever system or springs. This has been debated before (see Proc. Inst. Civ. Engrs. 1911—12. Discussion by Mr. Barrett and Professor Goodman's reply). Surely the truth is that the difference in behaviour under dead weight and spring-applied load, depends upon the fact that the force required to accelerate the parts attached to a slightly eccentric bearing, as the axis of the revolving ring is displaced, is greater for a dead load than for a spring system. In the former the mass accelerated during the displacement is the mass the weight of which is the load applied; in the latter it is only the mass of the spring stretched. The mass of a lever system will probably be intermediate between the two. The total load upon the bearing during the displacement is the applied load plus the force required to produce the mass acceleration, and is, therefore, greater in the case of an eccentric bearing loaded with a dead (or rotating) mass than for the same bearing loaded by levers or by a spring. Whether this appreciably affects the longevity of bearings made to a very high degree of accuracy is very questionable. For such a bearing it would be quite correct to suppose that it "will stand a dead mass loading for the same period that it will if it were loaded to that same extent under a spring." If it is a question as to which method of applying the load is the best, for somewhat eccentric bearings that will depend, of course, upon the conditions under which they are to be used, and as very many are loaded through springs and belts, and comparatively few bear the whole of their load as a dead weight, probably the lever system of applying a test load is the best to choose.

Mr. Parnacott suggests steel washers spot-welded on to ball bearings. Spot-welding hardened ball races is not an operation that appeals to me at first sight. Ball bearing manufacturers generally leave the provision of dirt-excluding devices to the user, who knows, or should know, what is possible and necessary. Devices closely resembling those illustrated in Fig. 28, but not

spot-welded, are in use and give satisfaction. The dirt and wet excluder and oil retainer illustrated in Fig. 30 has the form of a canister lid. This has given excellent results when other mountings have proved unsatisfactory. Other devices are illustrated in Figs. 31 and 32.

I am afraid I cannot follow Mr. Allan, when, after conceding agreement between calculation and experiment as to the dimensions of the contact surface in the Type 345 single-row journal bearing loaded under a test load of three to four times the maximum rated load, he goes on to suggest that the actual stresses do not show the same agreement with those required by theory and calculation.

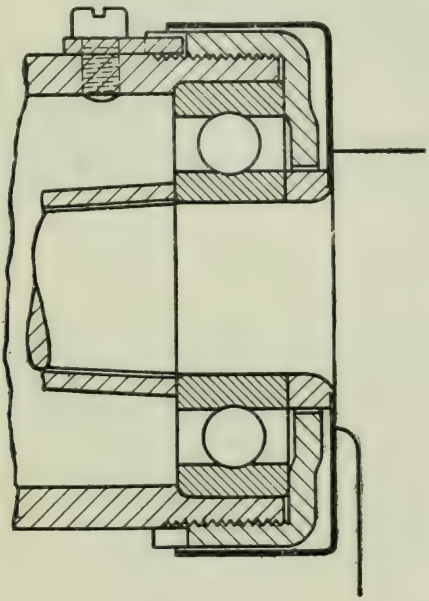


FIG. 30.

Surely the dimensions of the contact surface depend upon the strains and the strains upon the stresses, and it does not appear at all likely that the stresses can be so very different from the requirements of theory if the strains agree so well. Auerbach (Wied. Ann. 43, pp. 61—100, 1891), Stribeck (Zt. d. Ver. deut. Ing. 45, pp. 73—79, 1901), and Schwinning (ibid. pp. 332—336) on the Continent, and Professor Goodman and others in this country have done experimental work the results of which confirm practically all the predictions of Hertz's mathematical investigations. If the results of experiments do not agree with the predictions of Huber and Sundberg, it seems much more likely that

(Mr. Heathcote.)
their predictions are at fault, not that the assumptions underlying Hertz's equations are failing us. The prediction about failure at the end of the major axis is certainly wrong in theory, for the tensile stress parallel to the surface is simply related to the flexure, and, as shown on pp. 597 and 598 of my paper, the latter is much greater at the end of the minor axis. With regard to the duration tests on two types of bearing, while the data given by Mr. Allan

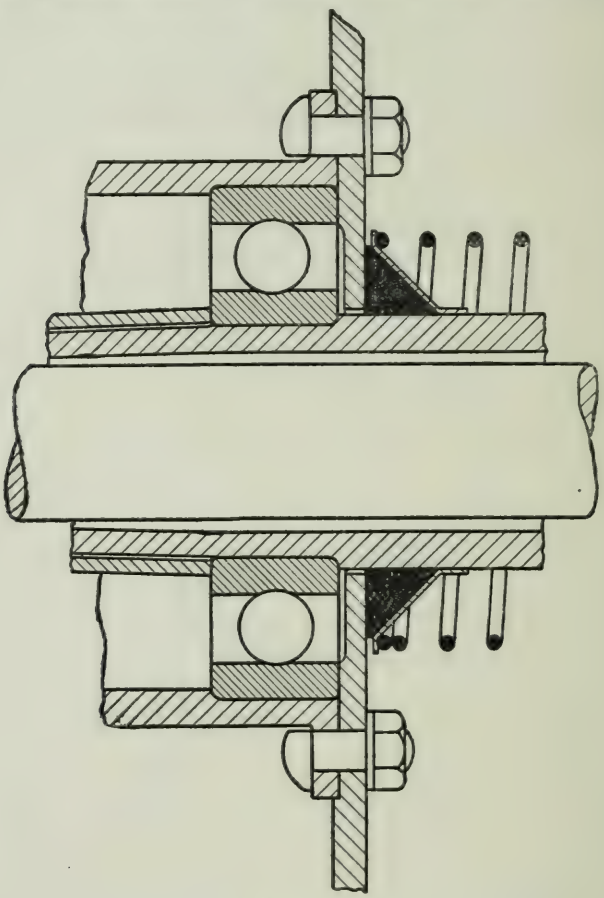


FIG. 31.

are doubtless of value, they do not appear to bear upon the subject of the investigation on pp. 587—591. Tests to confirm this should be under pure axial load without the complication of radial load. That the single-row journal bearing will perform well under considerable side thrust is shown by Mr. Allan's results and the testimony of others who took part in the discussion. In Mr. Allan's tests the bearings without slots lasted longer than those with slots. Other things being the same, we must be prepared

for a slight advantage in the non-slotted bearing, whether the outer race be spherical or an annular groove, but it does not seem correct to say that under axial load the balls are forced into the slot. How can they be? The balls on either side of the slot prevent any such thing happening. As soon as the raceway at the slot yields more than at other contacts the load is taken up by the balls at these contacts. The inference that there were fewer

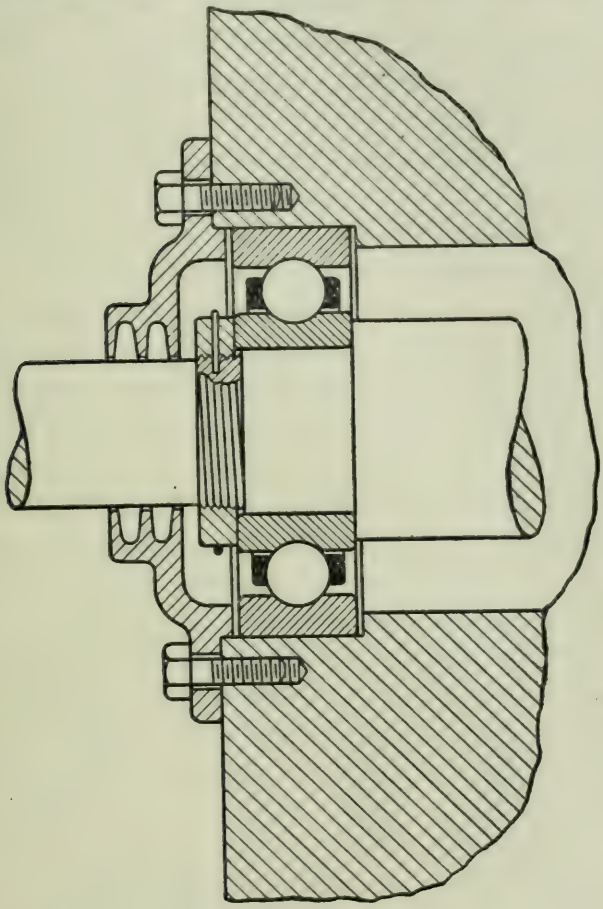


FIG. 32.

balls in the bearing assembled with slots on opposite sides is a natural one, but as a matter of fact the number was the same, a slot of special form being employed. I agree that misalinement of a shaft is harder on an annular ball bearing than on a spherical ball bearing, but I consider that the employment of a larger radius of curvature for the outer race will aggravate, and not tend to cure, that part of the ball loading which is due to the generation of more heat at the inner than at the outer contact surfaces.

(Mr. Heathcote.)

In reference to the question of creep, two cases are to be considered, in both of which the convex surface is assumed slightly smaller *when loaded* than the concave surface with which it pairs, (1) the bearing not locked, (2) the bearing locked. In the former, we have what is practically a roller bearing in which the roller is very nearly as large as the raceway. Now we know how prone even carefully made roller bearings are to develop end thrust, in some cases in very considerable amounts and proportional to the load. In such cases, as Professor Goodman has demonstrated, the roller rolls with its axis not quite parallel to that of the track, applies an end thrust equal to the load on the roller multiplied by the coefficient of sticking friction, then slips back and does the same thing all over again. It seems more than likely, therefore, that the same kind of motion will take place at the revolving element resulting in the wearing away of the softer surface (and to a less extent of the harder one). If the attempt is made to minimise the axial slipping, and the revolving element is locked up against a shoulder, other forces tending to abrade the surfaces put in an appearance, this time tangential and not axial in direction. If the location of the face of the revolving element could be effected without the introduction of any restraint upon the relative angular rotation of the parts in question no tangential forces would be developed; the locking, however, tends to drive both at the same angular velocity—which is impossible if they touch. Unless sufficiently forceful to prevent touching, we must therefore expect locking to induce wear by tangential relative motion. The forces involved are so considerable that I do not believe locking the bearing on its sides will generally suffice to prevent creep, or that it offers any advantage except when there is interference. If I understand the matter correctly, creep will occur only when the loaded ring and shaft or ring and housing separate at some point or points, and any means that will prevent radial separation will obviate creep. As already stated in my verbal reply, an interference fit seems the best means. The amount recommended is 0·0003 in. for small, 0·0005 in. for medium and 0·0007/0·001 in. for large sizes. A corollary to this is that the bearing, before assembling, shall possess sufficient end-play. If ball bearing users will test *assembled* bearings for end-play and noise they will be in a better position to choose the right amount of *initial* end-play.

With regard to interference, the remarks at the top of page 574 really relate to bearings which have little or no initial end-play

fitted to iron or steel members. With soft metals and bearings having rather more than 0·006 in. end-play it is not necessary to so limit the interference.

Colonel Higgs also raises the question whether initial load on the balls really matters in view of the fact that an applied load near to that of the listed capacity of the bearing will relieve some of the balls of all load. Applied load, if sufficient, will relieve the load on some but not all of the balls in one half of the bearing. The ball in line with the load is the first to be relieved of its initial load, then those next to it if the applied load is sufficient; but however great be the applied load, it will not relieve the whole of the initial load on all the balls in that half-circle. Since the load on these affects the load on those in the more loaded half, it is incorrect to assume that "no additional load is put on to the balls by means of the original interference . . ." I have made tests on bearings with little or no end-play (0·002 in.), and have found that they failed after an exceptionally short run. With regard to clearance (difference of curvature of track groove and ball) of the few bearings tried a track with $2\frac{1}{2}$ per cent clearance failed at high speeds, the balls getting very hot. An otherwise similar bearing but with 5 per cent clearance behaved satisfactorily. Mr. Max Lawrence was interested in the accuracy attained in the manufacture of balls and ball races. The former are measured and sorted into three and sometimes more groups. The latter are made a dead fit with the required amount of end-play, which enables 0·00002 in. radial play to be detected. The final operations, namely, light polishing with rouge, assembling each bearing with a few standard balls and testing by hand are performed successively many times until the required fit is attained.

Mr. Rodway's question about the choice of bearings is one that an engineer could answer better than myself, but surely the better way is to calculate the maximum load and to choose a bearing listed to take that load. The Rudge-Whitworth Co. test all bearings of their manufacture for noise, and rectify or reject the noisy ones.

In attempting to arrive at a formula for the load on ball bearings, Mr. Arter has tackled a very difficult subject. In the formulæ he puts forward, the safe load varies as the 0·725 power of the ball diameter for thrust and as the 0·5 (approximately) power for journal bearings. This is very different from the relation generally

(Mr. Heathcote.)
made use of by ball bearing manufacturers justified by many tests and long experience.

I think Mr. Arter is correct in supposing that journal bearings fare better when the inner race is the revolving element. This is to be expected, if only because the stress on the stationary element is then distributed over more metal, but not for the reason that Mr. Arter supposes. The angular velocity of a ball about its axis is the same whether the inner or the outer race be the revolving element provided, of course, that the revs. per minute of the bearing are the same in each case. If the experiment described by Mr. Arter be completed by noting the lengths of track traversed, they will be found to be the same whichever element is rotated. If the experiment be made with three or four florins, one representing the inner ring and a piece of cardboard for the outer ring, it is possible to be convinced that the angular rotation of the balls about their axes, after allowing for the rotation *of* the axes, is the same whichever element is revolved. The argument that carries most weight to my mind is to imagine a ball bearing revolving in space, the rings going at such speeds as to make the ball axes stationary in space. If we now imagine the observer to be rotated about an axis passing through the line of vision and in line with the axis of the bearing, first at an angular velocity equal to that of, say, the outer ring and in the same direction, and then at a velocity equal to that of the outer ring, it is obvious that in both cases when one ring is stationary relatively to the observer the other is revolving at an angular velocity equal to the numerical sum of the angular velocities of the two rings about their common axis, also that the angular velocity of the balls about their axes is the same for each case.

Should this meet the eye of a chemist or physicist interested in atomic structures, it may open a fruitful field for speculation and work to consider a bearing revolving in space so that its ball axes are stationary, and then to imagine it spun about one of these axes at an angular velocity equal but opposite to that of the ball. That ball will then appear quite stationary, and could, if the races were of convenient shape, be poked out of the bearing and replaced by another of different composition. There is no need to tell the chemist or physicist that for balls he can read electrons in the atom or atoms in a molecule.

In deciding the question, raised by Professor Goodman, whether it is the larger or the smaller balls in a journal bearing which

complete their orbit round its axis first, we have, of course, to ascertain the value of Ro (Ro + Ri) (if the outer is the revolving member) for both larger and smaller balls, Ro being the distance from the axis of the bearing to the non-slip bands of the outer raceway and Ri the like distance for the inner. If we imagine the larger ball to be derived from the smaller one by expanding it, it is clear that the non-slip bands move further away from the centre of the ball (see Fig. 9). They also move further round the ball. The former tends to make the larger the faster, the latter tends to make it the slower of the two. If the latter effect predominates, Professor Goodman's opinion is the correct one, but an inspection of the formulæ and of a sketch more or less to size leaves me still of the opinion that in its effect upon Ro and Ri the angular displacement of the non-slip bands round the ball is not likely to predominate over their displacement towards and from the axis of the bearing. If the inner is the revolving member the larger ball will be the one to lag behind.

With regard to the analytical treatment of ball bearings under combined axial and radial loads, I agree that the method adopted omits to take into consideration the fact that in a radially loaded bearing, the unloaded balls of which are clear of one of the races, the application of axial thrust will involve the loaded balls before the unloaded ones. Professor Goodman appears to consider that this will result in some of the balls taking "far more of the end thrust than others," but *will* it? Does it result in anything more than a slight difference in the ball loading, a difference of quite a second order of magnitude? The maximum difference will be the side thrust carried when the unloaded balls are moved up just into contact with both races. The lateral displacement of the two rings required for this is very small. In other words, the angle between the line joining the ball contacts (see Fig. 5) and the plane of the bearing is very small. The horizontal components of the ball reactions will be correspondingly small, and in my opinion negligible compared with the horizontal reaction when all the balls share in taking the side thrust.

Professor Goodman's relative load results are very important from the point of view of the user of such bearings, but before committing myself to the belief that the relation between the speed and permissible load is in accordance with Professor Goodman's figures I would like to know a little more about the bearings in question, for instance, whether the failure was connected with the

(Mr. Heathcote.)
accumulation of heat in the bearings, or with friction between the balls and the cage, or in the oil layer between them, and whether the balls were identical in size. In the absence of information on these points I am inclined to think that perhaps the loading might have been greater had the bearings been made more perfectly.

Professor Goodman very kindly expresses his appreciation of my efforts, but it must not be forgotten that such efforts depend very largely for their success upon previous workers, among whom Professor Goodman himself is by no means the least painstaking and successful.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

April, 1921.

The Twelfth Ordinary General Meeting of the Session was held at the Chamber of Commerce, New Street, Birmingham, on Thursday, April 28th, 1921, at 7 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"Comparative Motor Cycle Performances,"* by D. S. Heather.

There were present 38 Members and visitors.

* This paper was originally read before the Scottish Centre, and is reproduced on p. 839.

THE DISCUSSION.

Mr. R. H. Pearsall, in opening the discussion, said: Figures in a paper are sometimes taken with a grain of salt, and I would put in a word for the reliability of the figures given in this paper. I have some experience of observers and their reliability, and I would like to record my high opinion of the reliability of the author's observations.

The sub-division of the v^2 coefficient in the resistance curve is an important matter for designers of vehicles. We are not allowed to alter bodies very much, but we can alter our mechanical arrangements, and as 60 or 70 per cent of this v^2 resistance is to be attributed to windage, we still have 30 per cent or possibly 40 per cent to deal with. On that point, I would refer members to a paper* read before the Institution of Civil Engineers by Mr. Leeming, in which he works out analytically the resistance encountered by a wheel rolling over a wavy or bumpy road. He shows that the resistance, as far as it can be analysed, depends upon various factors. He shows it to depend upon the wave length of the road, taking a wavy road of ordinary sine wave form as being simpler to examine than a pothole. The greater the length of this wave, other things being equal, the greater he shows the resistance which will be offered to be. The resistance varies again inversely as the radius of the wheel, or, more accurately, it varies both as the square of the length of the wave and inversely as the square of the radius of the wheel. It then varies inversely as the distance which the vehicle has travelled before the wheel has recovered from the bump. It varies naturally with the square of the velocity, and, and this is the point I want to make, it varies directly with the unsprung weight. Now, I submit that one of the reasons why a motor cycle has a greater "windage" than a car is not so much that it exposes relatively a greater amount to the air, but also that normally its chief weight is utterly unsprung. That must mean a very large effect. If the front wheel of a motor cycle, together with its unsprung portion, is compared with the front axle of a Rolls Royce car, I think it

* See Proc. Inst. C. E., 1921, p. 11.

(Mr. R. H. Pearsall.)

will be found that the proportion to the total weight is very much greater. That is why a Rolls Royce, a 3-ton car, is a perfect job in springing, for its axles are *relatively* weightless. With a little car, say the Rover or the Deemster, the axle is found to be a large proportion of the weight of the car, and the v^2 resistance goes up. We have, therefore, to look upon part of this resistance as a factor of the unsprung weight, and a factor, again, of the resilience of the tyres. That, however, is a point on which I have no definite knowledge. Some say that a slack tyre is slower than a hard one, but whether that applies to a dead smooth road, in which case it would have a primâ facie case, I do not know. A soft tyre would suck more than a hard one, but I should like to know whether a soft tyre or a hard one is the best, and by how much, for a car driven over a really bad road. The author will possibly carry out some experiments on that point. It is interesting to note that by merely varying the position, or by the presence or absence, of a wind screen, the author gets 15 per cent difference in the resistance, or, rather, 15 per cent of the v^2 factor difference, especially when the smallness of that part in relation to the whole machine is considered, and it would seem to call for a good deal more study. Also by changing the sidecar, and again that change is very small relatively, he shows a difference of 12 per cent in the speed, which is 24 per cent of the v^2 factor. There is quite clearly a very great deal to be found out yet. It is interesting also to look at the question of tractive effort. Most motor cyclists know that if we change from a solo or a sidecar machine to a small car, we all think it is rather lifeless, and the figures indicate that it is. If we take as a standard figure to start off from, a tractive effort for a motor car of 10 lb. per 100 lb. of the total weight of the vehicle, the sidecar combination rarely falls below that and commonly goes up to 12, and higher on a solo machine. In getting away the car is relatively slow, but when under way it is the faster vehicle. In connection with the question of windage raised by the author, if the vehicle is photographed with a long-distance camera, and the area so obtained is measured by a planimeter, a 6 h.p. B.S.A. sidecar which weighs 1,000 lb. shows 20 sq. ft., whereas a 30 h.p. Daimler, which weighs nearer 3 tons, shows 33 sq. ft., and it has a 4 or $4\frac{1}{2}$ litre capacity engine against a $\frac{3}{4}$ litre engine. Therefore, it might be fairer to compare them on the sq. ft. basis rather than the ton-mileage basis.

Mr. P. J. Joyce: There is only one point that I should like to raise, and that is that the stream-lining of motor cycles should be gone into more fully. Sidecars should be more egg-shaped so that they would not present so much resistance. Also, all pieces that project should be stream-lined, as is done on an aeroplane.

Mr. E. Caldwell: Some time ago I found, to my surprise, that the average wind area for a $1\frac{1}{2}$ litre capacity car is considerably less than that of a motor cycle and sidecar. We were given a figure of 20 sq. ft. for the 6 h.p. B.S.A., but I think it will be found that on the average the small car with wind screen is about 14 or 16 sq. ft.

Major B. W. Shilson: I would like to ask Mr. Pearsall if he could tell us the reason for the unsprung weight of a car having such an effect on the windage. Is it due to the vertical displacement of the car? With regard to the great effect which a small difference in the wind screen has, the Indian punka or fan if fitted with a loose frill at the bottom gives a much greater draught than without the frill, even if the area is increased to make up for that of the frill.

Mr. F. A. S. Acres: The point that I want to raise is in connection with the author's remarks as to the ideal ηp and ideal petrol consumptions, and his statement that the ton-mileage basis is no comparison at all. It seems to me a very poor argument to set up as a test for the motor cycle a low ideal as compared with the ideal for the car, and then say that because we get near that ideal it is a better vehicle, which is what the author has said on pages 866 and 867. He takes the Ricardo ideal engine performance curve and says that with the motor cycle engine we can only expect an ideal ηp of 103 lb., and in the first case we actually get 83, so that that is a better engine than the 20 h.p. Daimler, for which he sets the ideal at 118 and gets 88. It seems to me that the best engine is the engine that gives us the highest value of ηp and the lowest petrol consumption per b.h.p., regardless of any arbitrary limitations. We know air cooling is a limitation in the case of the motor cycle engine. Then, with regard to ton-miles, the motor cycle does, of course, make a poor showing in the same way that the average touring car makes a poor showing compared with the lorry, and the lorry makes a poor show as compared with the petrol rail car. Fifty ton-miles is an extremely good figure for a touring car; a common figure for a lorry is 60,

(Mr. F. A. S. Acres.)
but for petrol rail cars I have seen a record of 260 ton-miles to the gallon. That, in a way, supports the author that the ton-mile basis is, theoretically, hardly a fair comparison, but I would have agreed with him heartily if he had brought it down to the petrol consumption per passenger-mile, or, we will say, the useful load mile.

Mr. T. C. Aveling: I think Mr. Acres was correct when he said the effective weight carried rather than the total weight should be taken as a basis for comparing efficiencies, because a steam wagon will show a very much greater efficiency than a petrol vehicle if the total ton-miles are taken, although the petrol vehicle is really more efficient.

Mr. R. H. Pearsall: Major Shilson asked how the unsprung weight affects the windage. I would refer him to Mr. Leeming's paper already referred to, where it is dealt with fully. Mr. Acres wished to take the passenger-miles per gallon. A sidecar combination, overloaded with three people, will do sixty miles to the gallon, or 180 passenger-miles per gallon: a touring car with seven passengers, also overloaded, will do, say, twenty miles to the gallon, or 140 passenger-miles, and the comparison is 140 to 180 in favour of the sidecar. I have taken them both overloaded, which is fair to them both.

Major R. V. C. Brook: There are many of us who feel that in connection with the motor cycle there is a great field for research which is totally neglected, and that there is still a lot to be done in this direction. I do hope, therefore, that manufacturers will appreciate the figures which the author has given them, and look into things for themselves and see if their designs are correct and make their own comparisons. I have made experiments myself with regard to tractive effort with electric vehicles, and I hope that when the author has completed his other figures, he will make a point of publishing them, and that he will let me have a copy to put before the various road surveyors in this district through my connection with one of the road associations, because I am confident that if the figures are put forward over the whole country, showing the total loss per vehicle, it will awaken the authorities to the value of good roads. I would like to suggest that experiments should be made as to the efficiency of sprung and unsprung frame machines, both as regards petrol consumption and wear and tear, and also with gears as to the speed and power obtainable with varying ratios and the corre-

sponding wear and tear. We all have certain hazy notions that if an engine is run at a certain speed it has a certain amount of wear and tear, but if the speed is increased we shall get more power and possibly the wear might be less. We have not, however, any very definite figures on that point.

Mr. J. F. Berry: It is rather a pity that there has been no direct comparison between motor cycles and light cars of similar engine capacity, because the real fight between motor cycles and cars comes in the 8 h.p. class. We have the 6 h.p. B.S.A. sidecar combination and the small 8 h.p. car, and it would be most interesting if we could have some results of ton-mileage, passenger-mileage, resistance, etc., for these two classes of vehicle, because there is no doubt that the larger h.p. motor cycle has got to fight in the near future against the light car. I certainly think it is not fair to compare a 6 h.p. motor cycle with a 20, 25 or 30 h.p. car.

Mr. E. Caudwell: In connection with the comparison between sprung and unsprung vehicles on bad roads, a car which has a speed considerably below that of a motor cycle always does better on a bad road simply because it is sprung. I have never had the hardihood on bad roads to let my motor cycle out to anywhere near its full capacity.

Major R. V. C. Brook: I should like to ask the author whether he has made any wind-tunnel experiments, or whether it would be possible to make wind-tunnel experiments on scale models in order to get at the actual and comparative resistance of the various types of machine.

The President: About twenty years ago I was interested in the question of train resistance, and carried out some experiments published in a paper by Sir John Aspinall in 1901 before the Institution of Civil Engineers. These showed in a very marked degree the effect of wind at high velocities. I find it a very difficult matter, even when running with a vehicle on a smooth surface, like a rail, to split up the resistance which varied with the speed, and I think the author will find some difficulty when he tries to do this on road conditions. I should like to know how many records he based his results on, and whether he took tests in both directions, as any incline would make a tremendous difference. If high speeds are looked for, undoubtedly some form of stream-lining must be resorted to; this, it has been shown, makes a tremendous difference when approaching velocities such as are met with in aeronautical work. I should like to know if the

(The President.)
author's instrument was self-recording or whether the record had to be read off.

Mr. Heather: It is read.

Mr. Heather, in replying on the discussion, said: In reply to the President, I would say in the first place that all these tests were carried out on days which were substantially windless and when the road surface was in a dry condition, and that is why it has taken a very long time to make the complete tests, and why the tests on splitting up the v^2 term have not been actually completed. The road I used in practically all the tests was a portion of the road between Coventry and Kenilworth, which was made up last year with a perfect Tarmac surface, and there is a flat stretch of nearly three-quarters of a mile between Cannon Hill and Gibbet Hill, and that gave me the opportunity of going down Gibbet Hill on one side and accelerating to the maximum speed, and taking my test on the level before I got to the slope on the other side. The tests were run both ways, and at least two in each direction, but sometimes three were taken in each direction, and the resulting velocity curves were averaged graphically to get a true record. The commencement of the upward slope was perfectly obvious because deceleration increased, and it was quite easy to take the tests only when we were on the level road.

The President: Did you get much variation in your two or three tests?

Mr. Heather: We got very little variation indeed. I think the principal justification for the tests is the fact that they agree so well with the actual engine performances on the bench. The 6 h.p. combination gave me exactly the speed which I got, and practically the same petrol consumption as obtained on the bench with the same carburettor. They agreed so well with the actual road performance that there is little doubt that they are substantially accurate, and I think there is no doubt that for practical purposes they are substantially right. Owing to the fact that we made the tests on windless days, the effect of side wind did not interest us. On the question of streamlining, I am in agreement with the President and others, and certainly if we are to go in for fancy speeds on the track we must streamline, and for very high speeds on the track special machines are necessary to enable the rider to get right down. The average touring machine

does not enable him to do so because the saddle position is not suitable.

The instrument we used made it quite unnecessary to have a recording device, if 5-second intervals are considered sufficiently close, and that has given quite good results. In the trials with a sidecar, I shouted each speed reading to the man in the sidecar, and as the Bonniksen stands still for 5 seconds there is plenty of time to jot it down provided the sidecar is reasonably well sprung. With a solo machine three or four figures have to be memorised and then jotted down, but this is quite easy. The principal virtue of the method is that it requires no special apparatus. That is one of the factors which militate against wind tunnel experiments: they would be rather expensive. Also, it is not much use going into wind tunnel experiments until we have split up this v^2 term into its component factors. We want to know what proportion the wind resistance is, and then wind-tunnel experiments may be useful, and possibly even cheaper than any other method. In attempting to separate the wind effect from other effects, one idea which we have not carried very far was to fit a wind vane on the Bonniksen speedometer spindle, so that instead of reading road speeds we read wind speeds. That gave us one curve for resistance varying with wind speed, and one curve for resistance varying with road speed, and a comparison of the two should enable us to separate one from the other, though there are difficulties in the way.

Mr. Pearsall mentioned the effect of the unsprung wheel. I think there is no question that the unsprung vehicle is very much slower on bad roads, not merely for physical reasons, but also because there is definitely more resistance, and that fact was brought home to us in carrying out some of these experiments on the Birmingham-Coventry road, which is excellent generally, but has one or two nasty waves in it. I think Mr. Pearsall is right in saying that a slack tyre is faster on bad roads. I always ride mine slack for comfort, and I think it gives me greater speed over bad roads, apart from the question of whether I can hold the machine or not, but a hard tyre is definitely faster on a smooth road. The flat wind screen figures to which Mr. Pearsall referred are absolutely certain. Our front screen consisted of a sheet of stamped millboard, and the observer held it in place at a constant angle, though he had great difficulty in doing so at 40 miles an hour. The effect of first driving without any wind screen at all, and then

(Mr. Heather.)

putting up the wind screen, was so great that when actually slowing down the impression was obtained that there was a brake on the wheel. There is no question that the flat screen is very bad.

Mr. Joyce referred to the streamlining question. It is obvious that the motor cycle and sidecar are very bad from this point of view, with all sorts of projections, and the whole outline will have to be smoothed off. As for the windage area of the small car, mentioned by Mr. Caudwell, I have a figure for the 10 h.p. Singer of 18 sq. ft., or less than that of the sidecar outfit, but the real comparison is a trifle smaller than that, because the motor cycle driver stands up so high.

Mr Acres joined issue with me on several points. After all, what we are comparing are definite engines at the moment, and definite vehicles. Mr. Acres wishes to take an engine with a 4 to 1 or 5 to 1 compression ratio, but the average motor cycle engine is a low-compression engine. We must get the utmost out of the particular engine we have got. Although high ideals are very nice, it does not always pay to try to live up to them. The motor cycle beats the car on litres per ton-mile, and also beats the car on a comparison with regard to liveliness. At lower speeds, litres per ton-mile is a reasonable comparison, but not at higher speeds, and therefore it is no use comparing a car at one particular speed with a motor cycle at the same speed, say, 20 miles an hour. What about 30 m.p.h.? The litres per ton-mile then would only give low speed figures.

Major Brooke has appealed to manufacturers to go into the questions raised in the paper, and I would like to endorse what he said. I had hoped that the publication of these figures, for which I have the permission of my Company, would have induced other manufacturers to come forward and tell us a little about what they have done. Our opportunities of obtaining comparative results with other makes are quite small. We can get them, but in most cases we have to rely on outside sources of information. It would certainly be to the advantage of the industry generally if we could get a comparison of the whole range of makes, and I would suggest, in endorsing Major Brooke's appeal, that other makers might give us some idea of what they are doing, so that we can see if these problems, which appear to us to be critical ones, are really so, or whether one particular firm is merely faced by one problem which is peculiar to itself.

With regard to gear-ratios and wear and tear, I am afraid any

tests would involve considerable mileage. My impression is that, taking machines in general, by lowering the gear of one particular machine, there would be less wear and tear. That is only an impression, and I have no knowledge, but as between two engines of the same capacity and different design, I think if one will stand a higher gear than the other, then let us have the higher geared machine. I have no definite figures on the question of the motor cycle combination versus the small car, but with a small car of the 8 h.p. Rover class, or the 10 h.p. Singer, which is the smallest of the old 10 h.p. cars, the resistance of these vehicles is substantially the same as that of a sidecar combination. The petrol consumption of the latter, however, is decidedly lower, and therefore, in any comparison, the motor cycle outfit would show up better. A combination like the 6 h.p. B.S.A. with its windage will do 60 miles per gallon driven at medium speeds. A vehicle like the 8 h.p. Rover or the 10 h.p. Singer will do 40 with substantially the same resistance. Therefore, it cannot be so economical, though I have no definite figures.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

PROCEEDINGS.

May, 1921.

The Thirteenth Ordinary General Meeting of the Session was held at the Institution of Mechanical Engineers, Storey's Gate, St. James's Park, London, S.W.1, on Wednesday, May 11th, 1921, at 8 p.m., Sir Henry Fowler in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following list of Members who had completed their election since the previous Meeting was posted up:—

Associate Members.

William John Hughan (Birmingham).
Arthur Hunter (London).
Thomas Alfred Ince (London).
D'Arcy Vincent Ryan (Sydney).
Horace Terry Satterford (Ramsgate).

Associates.

William Staites Skerrett (Birmingham).
Henry Robert Williamson (London).

Graduates.

Edward Dixon Abbott (Birmingham).
Cedric Arthur Edwards (Birmingham).
Stephen Miller Fulton (London).
Norman Spackman (Birmingham).
Thomas Ernest Waldeck (Loughborough).

PROBATIONARY GRADUATES.

DONALD DUNNETT (Aberdeen).
WILLIAM LAURENCE GAVIN (Bradford).
ARTHUR LESLIE MOLYNEUX (Coventry).
ARTHUR SAMUEL THORP (Beaconsfield).

TRANSFER.

JAMES BELL OSLER (London), Associate Member to Member.

The following paper was then read and discussed:—

"Some Experiments on Supercharging in a High-Speed Engine," by H. R. RICARDO.

There were present 108 Members and visitors.

SOME EXPERIMENTS ON SUPERCHARGING IN A HIGH-SPEED ENGINE.

By HARRY R. RICARDO, B.A.
(Member of Council).

The craving to obtain higher power and greater economy from the internal combustion engine, which is an almost universal ailment, leads the investigator to review from time to time the thermo-dynamic conditions which govern its performance, in the hope of finding some modification of the cycle which will provide one or both of these desirable characteristics.

If we consider for a moment the usual four-stroke cycle, we find that there are only two obvious ways of increasing the power output, namely, by increasing the mean pressure, or by increasing the piston speed. The former does not leave much ground for hope, for the volumetric efficiency is already within one or two per cent of the highest obtainable, and the compression ratio has already been carried to the highest safe limit consistent with the fuels available. Moreover, a little investigation will show that any further increase in compression must result in a very large increase in the maximum pressure, involving the necessity for heavier moving parts; hence such increase of efficiency as might be obtained by increased compression will be largely counterbalanced by enhanced mechanical losses due to the heavier reciprocating masses.

So far as piston speed is concerned, there does not appear to be very much hope in this direction either. Until recently the piston speed was controlled primarily by the inertia of the reciprocating masses, but since the advent of the aluminium piston the controlling factor has been shifted to the valve gear and the provision of adequate valve area. By careful design, the limit can no doubt be still further raised, but only at the expense of greatly increased cost of manufacture. Again, if either the mean pressure or the piston speed could be increased, another controlling factor, which always looms in the background, would very soon come forward and play havoc, namely, the difficulty of dealing with the

intense heat flow which is already a serious problem, even in the smallest engines. This spectre can be propitiated to some extent by the use of aluminium piston and cylinder heads; it crops up again in the exhaust valve, and can be quieted by the use of water cooled exhaust valves, but it cannot be altogether laid by such means, and when staved off in one direction it very soon crops up in another. In whatever way the problem may be viewed, the conclusion is invariably reached that in order to obtain any really substantial gain in power or economy, the question of heat flow must first be attacked, and some means or other must be found to reduce the flame temperature. In this direction lies not only the most hopeful prospect of increasing the power, but also the one chance of a really substantial gain in economy, since the

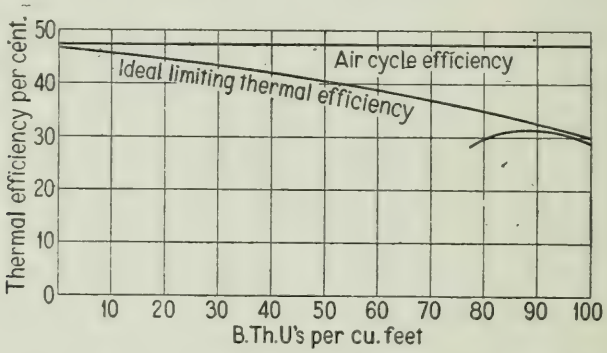


FIG. 1.—Air cycle and $E = 1 - \frac{1}{(r)}\ 0{\cdot}25$ for mixture strength from 0 to 100 B.T.Us. and compression ratio 5:1.

two principal losses, namely, the loss due to direct transfer of heat to the cylinder walls, and the even larger loss due to the increase in the apparent specific heat of the working fluid at high temperatures, are both directly dependent upon the maximum flame temperature.

If we consider the working fluid from the point of view of an air/petrol mixture, we find that the proportion giving complete combustion has a heat value in round numbers of about 100 B.Th.Us. per cu. ft. of mixture. With this mixture density, the combustion of the fuel will raise the temperature of the working fluid to approximately 2500° C., taking into account the increase in the apparent specific heat and dissociation. At this high temperature the loss of heat, and still more the loss due to the increase in the apparent specific heat at high temperatures, are extremely serious.

According to the Air Standard efficiency, so long as the gases are expanded to the same volume as they occupied before compression, the efficiency of the cycle is independent of the maximum temperature, but this holds good only so long as there is no loss of heat, dissociation, or change in the specific heat of the working fluid. When, however, these considerations are taken into account, it will be found that the ideal efficiency falls rapidly as the temperature rises.

In Fig. 1, one curve shows the theoretical efficiency obtainable with a compression ratio of 5 : 1 on the assumption of no heat loss, no dissociation and constant specific heat; this curve is a straight line, and is independent of the flame temperature or heat value of the mixture. The other curve takes into account the loss due to change of specific heat, and also allows a minimum figure for the jacket loss, and it will be observed that as the heat value of the mixture, and therefore the flame temperature, is reduced, the efficiency rises until, at the point of no heat supply, it would become equal to the air cycle efficiency.

Consideration of this curve shows at once the desirability of working with a weak mixture, and therefore a lower flame temperature, but unfortunately petrol vapour has only a very narrow range of burning, and so long as the mixture is homogeneous it is not possible under ordinary conditions to employ a mixture weaker than about 85—90 B.Th.Us. per cu. ft. At this strength the maximum flame temperature is but little lower, since the first effect of reducing mixture strength with petrol is to reduce the proportion of dissociation and so affect the temperature but little. Any attempt to work below this density with a homogeneous charge results in incomplete combustion, and ultimately so slow a rate of burning that flame lingers in the cylinder and ignites the fresh charge on entry, causing the familiar popping back through the carburettor—the well-known indication of an over-weak mixture.

It is, of course, evident that were it found possible to work with a much weaker average mixture density, and so obtain a very substantial improvement in thermal efficiency, the mean pressure would be reduced, though not in direct proportion to the heat value of the mixture, because the higher efficiency obtained would mean that a greater proportion of the available heat would be converted into power.

To appreciate the effect of working with a low mean mixture strength it is interesting to consider what would happen if into a

cylinder containing nothing but pure air, a paper bag were inserted, containing a small proportion of fuel and air of normal mixture strength, and that by some means the contents of this bag were ignited at the end of the compression stroke; the bag would, of course, immediately burst, and the burning mixture would be projected into the large excess of air present in the cylinder, thereby heating the air and raising its pressure. The average temperature of the air under these conditions would of course be much lower than if it were saturated with fuel; on the other hand, if the same quantity of fuel had been allowed to mix with the whole of the air it clearly would not have ignited at all.

Fig. 2 shows the sort of indicator diagram which would be obtained supposing that the contents of the paper bag represented 20 per cent of the total cylinder volume, the remaining 80 per cent

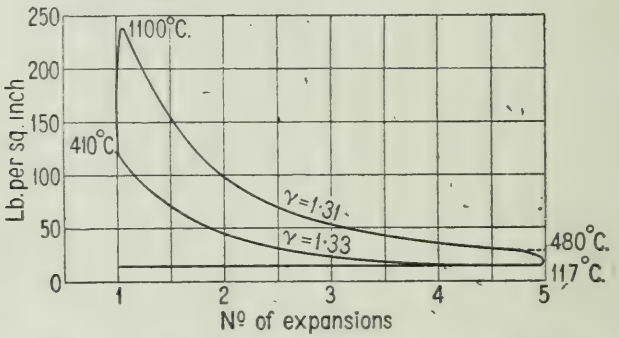


Fig. 2.—Ideal indicator diagram for 20 B.T.Us. per cu. ft.

being either pure air or air and exhaust gas. It will be observed that the maximum flame temperature would in this case be only about 1100° C. With such a maximum temperature the loss of heat to the walls and the loss due to change of specific heat would be very small, and the efficiency would be very high in consequence. The theoretical efficiency would under such conditions be about 44 per cent for a compression ratio of 5 : 1. The author must apologise for using the simile of the paper bag again, but it affords a ready means of illustrating what he is aiming to describe.

In the experiments described later in this paper the author aimed at:—

(1) Reducing the mean mixture strength by stratifying the charge and so obtaining a high thermal efficiency.

(2) Reducing the heat flow by lowering the flame temperature.

(3) Recovering and even increasing the mean pressure by the addition of extra air in the form of a supercharge.

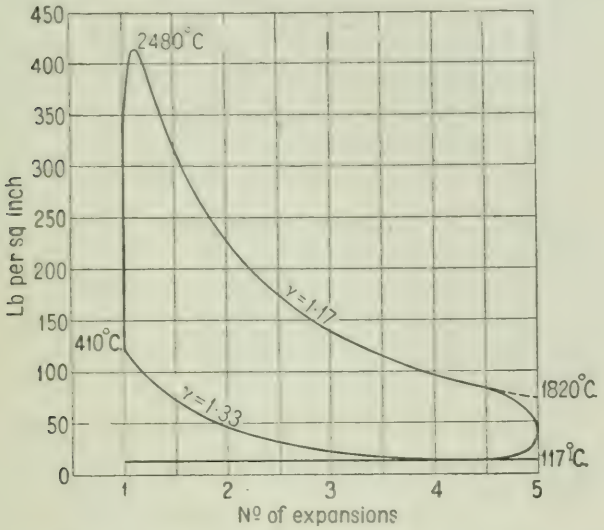


Fig. 3.—Ideal indicator diagram for 90 B.T.Us. per cu. ft.

In Figs. 3, 4 and 5, are shown three indicator diagrams which illustrate the objects in view.

(a) A normal diagram for an economical mixture strength

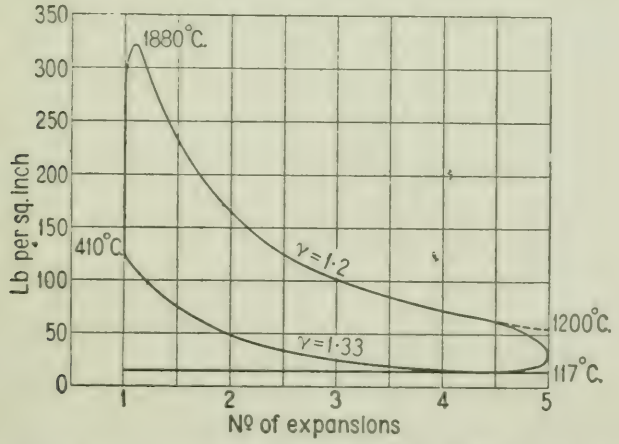


Fig. 4.—Ideal indicator diagram for 66 B.T.Us. per cu. ft.

with a homogeneous charge, namely, about 90 B.Th.Us. per cu. ft. This diagram shows a maximum temperature of about 2500° C., a limiting efficiency of 32·5 per cent,

and a mean effective pressure of 138 lb. per sq. in. indicated.

(b) A diagram for a mean average mixture strength of

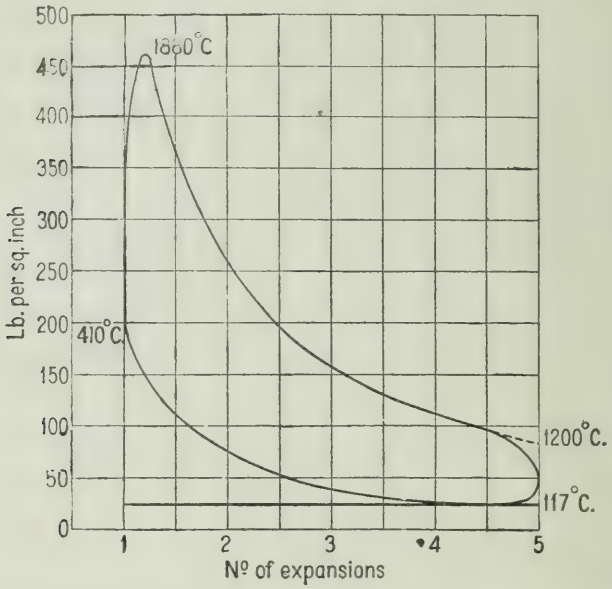


FIG. 5.—Ideal indicator diagram for 66 B.T.Us. per cu. ft. plus 33 per cent supercharge.

66 B.Th.Us. per cu. ft. In this case the maximum temperature will be about 1880° C., the efficiency 35·6 per cent, and the mean effective pressure 113 lb. per sq. in.

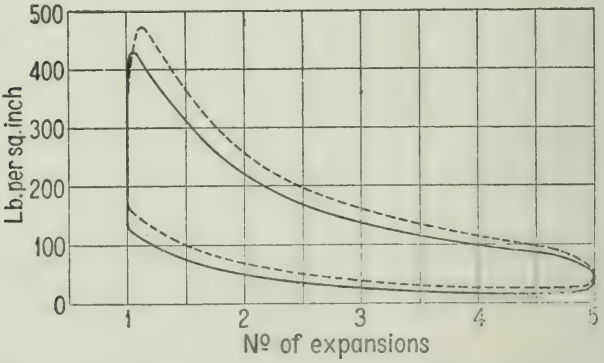


FIG. 5A.—Shows Figs. 3 and 5 superimposed.

(c) A diagram also for a mean average mixture strength also of 66 B.Th.Us. per cu. ft., but in which the weight of air has been increased 33 per cent by means of supercharg-

ing. The effect of the supercharge will be to enlarge the whole diagram by 33 per cent without affecting the temperature and, therefore, the efficiency, but increasing the mean pressure to 150 lb. per sq. in. Under these conditions, not only will the high efficiency theoretically obtainable under the conditions of diagram (b) be maintained, but the mean pressure will be restored and even raised to well above that obtainable under normal conditions of working.

The experiments may be divided into three phases.

(1) An investigation into the efficiency obtainable by varying the mixture strength under normal conditions.

(2) An investigation into the possibilities of reducing the mean mixture strength by means of working with a stratified charge.

(3) Experiments combining the use of a stratified charge with supercharging.

For the first series of tests what was considered in 1913 as a highly efficient single-cylinder vertical engine, direct-coupled to a swinging-field dynamometer, was used. The engine was provided with a Hopkinson optical indicator and a carburettor in which the whole of the fuel was thoroughly vaporised, and in which the control of the fuel supply could be regulated with extreme nicety by means of a very fine needle valve. The accuracy of the apparatus was such that the power could be read with certainty to within ± 0·3 per cent, and the fuel consumption to within ± 0·5 per cent. Very careful determinations were also made of the mechanical and pumping losses, so that the indicated power and efficiency could be arrived at from the brake horse-power, and also checked by indicator diagrams which were taken at frequent intervals.

There is, of course, nothing novel about these tests, but they were carried out with very great care in order to furnish a datum line. A summary of the results obtained is shown in Fig. 6 which explains itself. It will be observed that so soon as the mixture strength is reduced to any substantial extent beyond that at which there is just sufficient air completely to combine the whole of the fuel, incomplete combustion counteracts any gain in efficiency due to the reduced flame temperature. Unfortunately, however, the absolute mixture strength could not be definitely determined because no means were then available for measuring

accurately the air supply; consequently the curves showing the actual performance may be as much as ± 2 per cent out in a horizontal direction.

So far as the object of these tests was concerned, this was of no importance whatever, but the author realises that from other points of view the tests would have been more valuable and interesting had it been possible to provide an accurate determination of the absolute instead of the relative mixture strength. By calculating back from the mean pressure and thermal efficiency actually obtained, however, it is possible to arrive at the heat

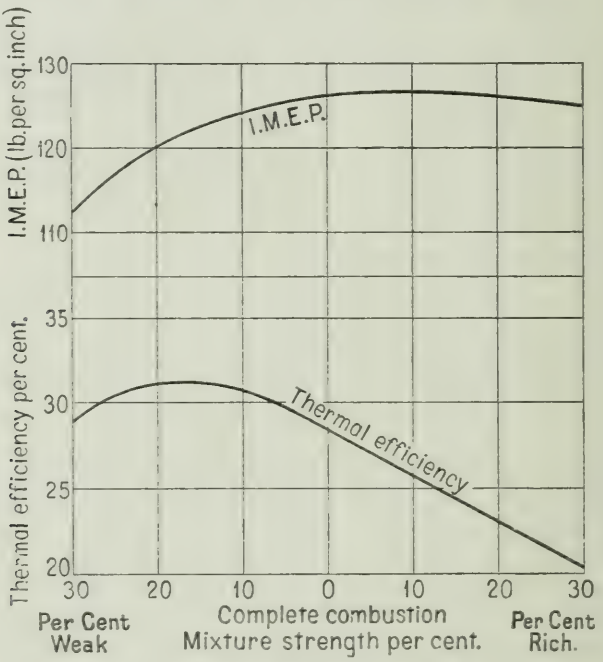


FIG. 6.—Relation between M.E.P. and fuel consumption (homogeneous charge).

value and therefore the strength of the mixture certainly to within ± 5 per cent, and this has been done in preparing the above diagram.*

The second series of tests, which was carried out during 1913—1914, was undertaken with a view to determining:

(1) Whether it was possible by means of stratification to work with a low mean mixture density.

* More recent experiments carried out with a very accurate and sensitive air measuring device indicate that the point of complete combustion should be shifted about 3 to 4 points towards the left, *i.e.*, that the mixture strength at maximum efficiency is not quite so weak as indicated in this diagram.

(2) Whether, if it were possible to do so, the results obtained would be found to be in agreement with those theoretically obtainable, or whether any disturbing factor would crop up to prevent their attainment.

To carry out this series of tests a very old single-cylinder vertical engine of 5 in. bore and 6 in. stroke was resurrected and provided with a new cylinder, cylinder head and valve gear. In view of the fact that the use of a stratified charge and a low mean mixture density would, if successful, result in a very low flame temperature, no water jacketing or other means of cooling was provided for the cylinder or cylinder head. The cylinder head was so formed, and the position and timing of the valves were so arranged, that a rich mixture of fuel and air and a large additional supply of pure air entered the combustion chamber at different points, and were so directed that even after their entry, and in spite of very vigorous turbulence, they would not completely mix, the rich fuel mixture remaining always in the neighbourhood of the sparking plug, while the air remained over the piston.

The power output of the engine was controlled entirely by the quantity of fuel admitted during the suction stroke, no throttle valve or other means of control being provided, so that at all times and under all conditions a full air charge was taken into the cylinder. It was not expected that mixture between the rich working fluid and the air would be entirely eliminated, but it was hoped that a sufficient degree of stratification could be obtained to permit of the average mixture strength of the whole contents of the cylinder being reduced to less than half that obtainable with a homogeneous mixture; the results proved that the degree of stratification actually obtainable was far greater than this, and, in fact, it was found possible to reduce the mean mixture strength from a useful minimum of 85 B.Th.Us. per cu. ft. under ordinary conditions of working, down to as low as 10 B.Th.Us., and yet obtain perfectly regular running.

A Hopkinson optical indicator was fitted in the combustion head, and the indicator diagram thrown on to a glass screen covered with tracing paper was carefully watched; a number of indicator diagrams were photographed, but for the most part the diagrams were traced in pencil, which was very much quicker; at the same time the brake horse-power of the engine was recorded.

These experiments proved more successful than had been hoped for, and the results were in tolerable agreement with the theoretical

figures. The following observations are taken from notes and test reports of 1913—1914:—

(1) The engine could be started at all times, even on the coldest day in midwinter, on the first pull-over.

(2) When running light with an indicated mean pressure of

FIG. 7.—Indicator card with stratified charge.

about 14 lb. per sq. in., the exhaust, though perfectly free, was almost inaudible, sounding rather more like heavy breathing than anything else. Also the cylinder barrel remained quite cool, and it was possible to bear the hand on any part of the cylinder head or exhaust

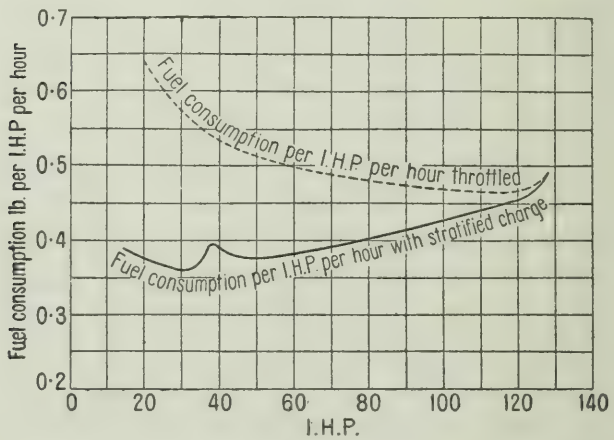


FIG. 8.—Fuel consumption and M.E.P. with stratified charge.

pipe even after an hour's running on no load at 300 to 400 revs. per minute.

(3) So long as the admission of fuel was not altered the speed of the engine remained absolutely uniform and the indicator diagrams extraordinarily consistent. Fig. 7 shows an exposure of 20 seconds, taken with the engine running

light at about 600 revs. per minute, and therefore represents about 100 complete cycles.

(4) By opening the needle valve and increasing the admission of fuel the engine would immediately accelerate even from the slowest speed of 120 revs. per minute.

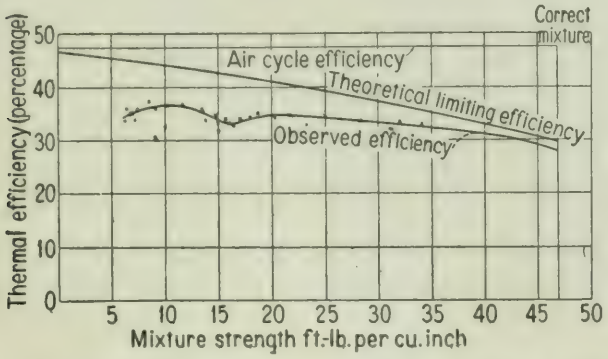


FIG. 9.—Indicated thermal efficiency ideal and actual with stratified charge.

(5) One very noticeable feature was the extraordinary rapidity of ignition, due to the violent turbulence set up. A fixed ignition timing of 3 degrees before dead-centre sufficed for all speeds and loads, and invariably gave a perfectly vertical line of pressure rise on the diagrams.

FIG. 10.—Indicator card with stratified charge.

(6) Some hundreds of readings of fuel consumption were taken at all loads ranging from dead light to full load, and the mean results of these are shown in Fig. 8, which gives the fuel consumption obtained with indicated mean pressures ranging from 14 to 120 lb. per sq. in. Fig. 9

shows the indicated thermal efficiency actually obtained, plotted against that theoretically obtainable, and presents, on the whole, very tolerable agreement. On the light loads there is undoubtedly some loss due to incomplete combustion.

(7) On full load the running of the engine was in every respect normal, but, of course, the absence of any water jacketing prevented the maintenance of full load for more than a few seconds at a time.

(8) Perhaps the most noticeable feature of all was the general sweetness and smoothness of running and the extraordinary responsiveness of the engine. Unfortunately, owing to the poor mechanical design of the engine generally, it was not possible to run it at high speeds, the

Fig. 11.—Indicator card with stratified charge.

extreme safe limit being only about 800 revs. per minute. Tests were made over a speed range of from 150 to 800 revs. per minute, and these showed practically no difference in the indicated fuel consumption, the shape or regularity of the indicator diagrams or the general running.

Subsequent experiments carried out on other engines proved, however, that the results obtained on the first experimental model could not be always repeated. Further bitter experience has proved that the author was quite unusually fortunate in selecting proportions in the first instance, and that much further experimental work is required before the system of controlling on the fuel alone can be so boiled down as to be rendered applicable to any type of engine. Quite recently another and much larger engine has

been built, operating on the same principle, and employing as far as possible the same form and proportions of combustion chamber; this engine, which has been running at the author's

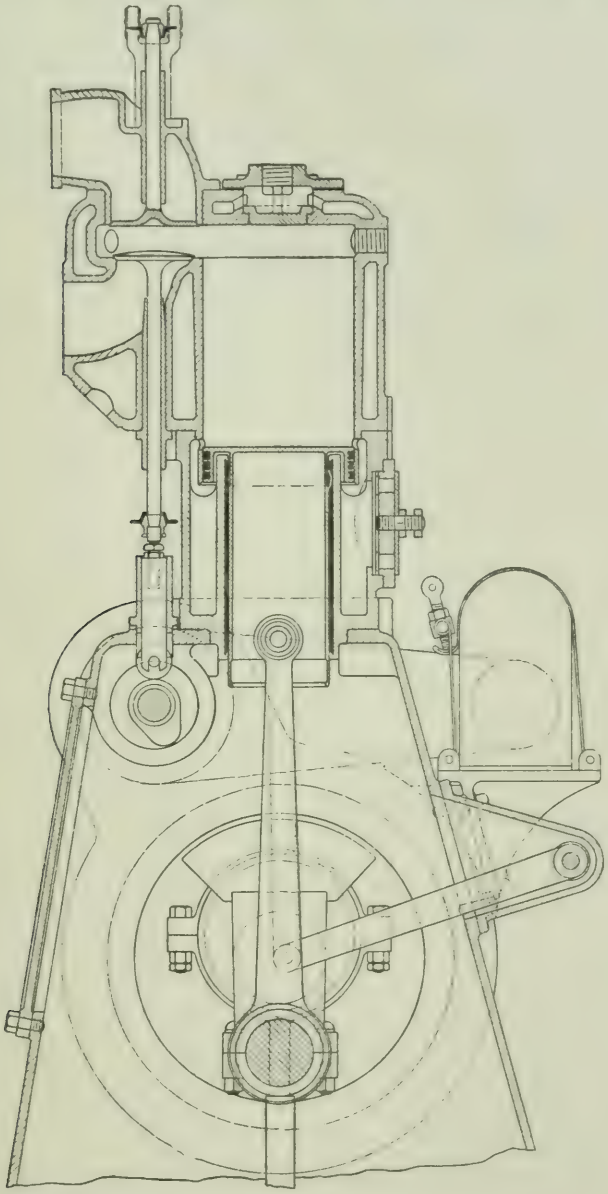


FIG. 12.—Section of supercharging engine.

laboratory for the past twelve months, has proved equally as efficient, but all attempts to adapt the principle of operating by stratification alone over the whole range of load to existing engines have ended more or less in disappointment.

Having established that, under certain conditions at all events, a very substantial degree of stratification could be maintained with, at the same time, sufficient turbulence to ensure rapid combustion (although the two conditions may not at first sight appear compatible), and having also proved satisfactorily that under these conditions a very marked reduction in the maximum temperature and something at least of a corresponding gain in the efficiency could be obtained, the third series of tests was commenced.

For the purposes of these tests, a special experimental engine was designed using a system of supercharging generally similar to that devised by Sir Dugald Clerk. This engine, a section of which is shown in Fig. 12, was completed in 1915; it had a bore and stroke of 110 mm. and 140 mm. respectively, and was designed to run at a normal speed of 1500 revs. per minute. The upper portion of the cylinder was quite normal and of the L-head type, with the inlet valve placed vertically over the exhaust valve, and operated by a push rod and overhead tappet. The piston, however, was formed in two parts, a plain head of aluminium carrying the piston rings, provided with a very short skirt furnished with a number of small oil grooves and a circular crosshead of considerably smaller diameter, the piston and crosshead being connected together by a hollow aluminium trunk. The crosshead portion of the piston, which carried the gudgeon pin, reciprocated in a white-metal-lined guide, which was carried in a separate casting interposed between the cylinder and the crank case.

In its original form the crosshead was of mild steel, and the piston and trunk were of aluminium, the two members being screwed together as shown in Fig. 13. This form of construction, though it cannot be considered mechanically sound, was found very convenient for experimental purposes, since the upper half of the piston could be changed very quickly and easily, in order to vary either the compression or the contour of the head, which latter proved to be very important. In point of fact it has never given any trouble in the experimental unit, where great care was taken in the machining of the threads, but the author would never advocate such a construction other than for experimental work. The piston and crosshead, as a whole, were fairly light, the total weight with rings and gudgeon pin being only 39 oz., which compares quite favourably with that of an aluminium trunk piston of the same size, though aluminium pistons were scarcely known at the time of the experiments.

The cylinder, which was very short, was spigoted into the separate casting carrying the crosshead guide, and the extension of the cylinder skirt was provided with a ring of ports uncovered by the piston at the bottom of its stroke; these were surrounded by a movable sleeve or shutter, so that they could be closed off at will. Surrounding the crosshead guide was an annular chamber having a minimum capacity of about 60 cu. in., but the capacity could be varied over a wide range by means of hollow

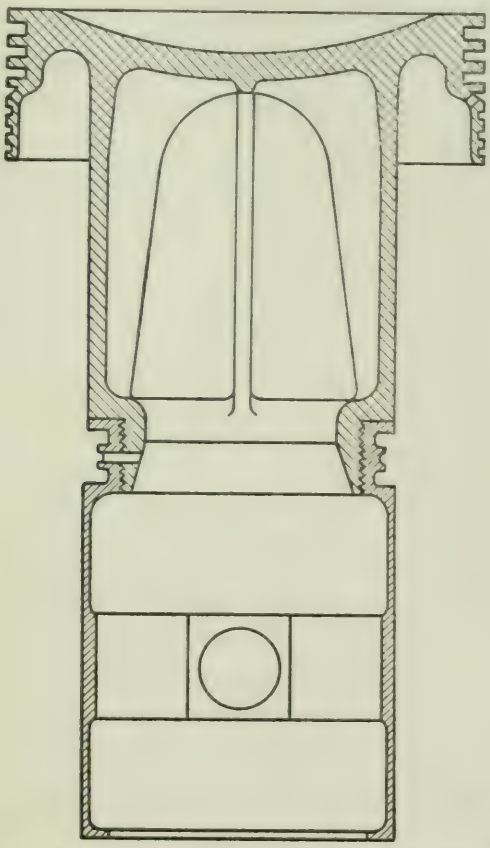


FIG. 13.—Piston of experimental engine.

detachable plugs. A series of very light phosphor bronze flap valves admitted air to the chamber and acted as non-return valves during the compression stroke.

Lubrication of the cylinder walls was provided for in the following manner. When the piston had almost reached the top of its stroke a series of small holes drilled through the wall of the crosshead were uncovered above the top of the guide. Owing to the reduced pressure in the crosshead chamber at this point

as compared with the pressure in the crank case, a small quantity of oil-laden air passed out through these holes, with the result that the oil was deposited on the walls of the cylinder where it was picked up by the short skirt of the piston and distributed evenly over the inner surface.

In its original form the cycle of operations was as follows:—

Commencing at the beginning of the suction stroke, the piston, as it descended, drew from the carburettor a charge of petrol vapour, and air of normal proportions. Towards the end of the stroke the ports round the lower end of the cylinder were uncovered and a charge of air compressed in the crosshead chamber to a pressure of about 12 lb. per sq. in., entered the cylinder.

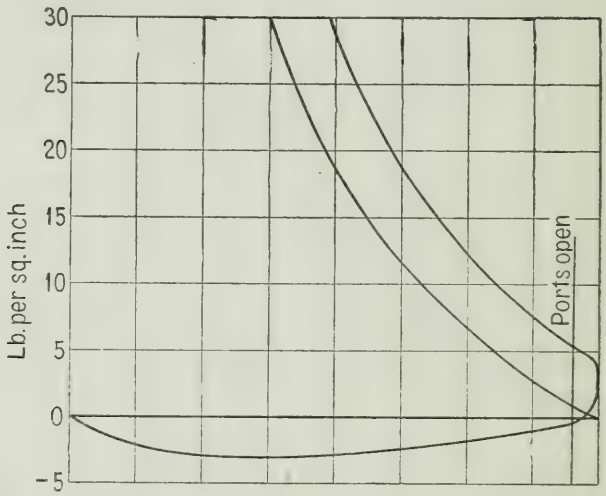


Fig. 14.—Ideal light spring card showing admission of supercharge.

Almost simultaneously with the opening of the ports the main inlet valve was closed. The supplementary air from the crosshead chamber raised the pressure of the cylinder contents to about 5 lb. per sq. in. above atmospheric pressure, and at the same time formed a layer above the piston, which remained in a more or less stratified condition throughout the compression stroke. Fig. 14 represents an ideal indicator diagram, showing the cycle in the cylinder during the suction stroke and the admission of the supercharge air, and Fig. 15 shows an actual indicator diagram taken from this engine. During the upward stroke the contents of the cylinder were compressed, the combustible mixture remaining more or less at the top, and the pure air forming

a layer over the piston. Meanwhile, another charge of air was drawn into the crosshead chamber.

At the end of the suction stroke the contents of the cylinder consisted of about 70 per cent of combustible mixture of normal strength, namely, about 95 B.Th.Us. per cu. ft., and about 30 per cent of air, the former being concentrated more or less in the valve pocket, and in the neighbourhood of the sparking plug, and the latter forming a layer over the piston. The mean mixture density of the whole of the cylinder content was therefore only about 66 B.Th.Us. per cu. ft., a density which, under ordinary circumstances, would not have been inflammable. On ignition, the combustible mixture in the neighbourhood of the sparking plug ignited with great rapidity, causing further violent turbulence in the combustion chamber, so that the air and the

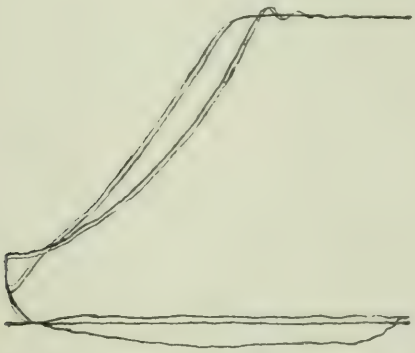


FIG. 15.—Indicator diagram showing admission of supercharge.

burning mixture were intimately mixed, but not until combustion was well under way.

Towards the end of the expansion stroke, the exhaust valve was opened in the usual manner and, shortly afterwards, the ports in the cylinder walls were again uncovered, and the cylinder swept through and partially scavenged by the second charge of air compressed in the crosshead chamber; this second charge again entered above the piston in a more or less stratified form, the gas left immediately above the piston being mostly air, while that nearer the top of the cylinder consisted mainly of exhaust products. On the upward stroke of the piston, the residual exhaust gases and part of the air were expelled through the exhaust, leaving the clearance space partially full of air. There was thus provided alternately a supercharge of air at the end of each suction stroke,

and a scavenging charge at the end of each firing stroke, so that not only did the cylinder receive an additional charge of air over and above the charge drawn in from the carburettor, but a further supply of air was provided by the scavenging charge, which had the effect of replacing the residual exhaust gases in the combustion chamber with, at all events, a considerable proportion of pure air.

It will be seen therefore that, on paper at all events, the temperature of the cycle was reduced in two directions:—

(1) By lowering the flame temperature;

(2) By scavenging during the exhaust stroke, while at the same time the weight of air was increased approximately 40 per cent, about 33 per cent by the supercharge and possibly 7 per cent by the scavenging.

It is obvious that the engine could be operated on either of two systems. Either a very rich charge could be admitted through the carburettor, providing sufficient fuel to combine with the whole of the air, in which case the mean pressure should be increased

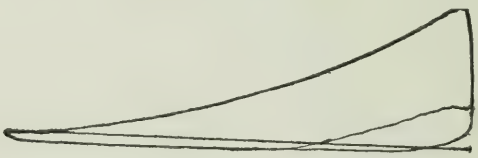


Fig. 16.—Indicator diagram from crosshead chamber (motoring test).

by 40 per cent, while the temperature of the cycle would still remain less than normal, thanks to the cooling effect of the scavenging charge; or a normal mixture could be admitted through the carburettor, in which case the air acted merely as a diluent, lowering the flame temperature and so increasing the efficiency of the cycle.

Before attempting any actual tests the engine, which was direct coupled to a swinging-field electric dynamometer, was motored at its normal speed of 1500 revs. per minute, and indicator diagrams were taken both from the crosshead chamber and the cylinder, the indicator being operated by linkage direct from the crosshead, in order to avoid any possible errors in synchronism.

Fig 16 shows a sample of the indicator diagrams obtained from the crosshead chamber after the timing of the valves had been adjusted to give what appeared from the indicator diagrams to be the

best possible results. In its original form the ports opened and closed 28 degrees before and after the bottom dead-centre, and the inlet valve closed 10 degrees before the bottom dead-centre; the motoring experiments, however, showed that it was unnecessary to close the inlet valve until after the bottom centre was reached, since owing to the time lag there was found to be no tendency to blow back into the carburettor at speeds above 1000 revs. per minute.

A very careful analysis was made both of the frictional losses and of the power absorbed in pumping both in the cylinder and crosshead chamber. These were found to be as follows, expressed in terms of mean pressure:—

Total frictional losses, including windage losses	11·5 lb. per sq. in.
Fluid pumping losses in cylinder	3·5 lb. per sq. in.
Fluid pumping losses in crosshead chamber	4·5 lb. per sq. in.
Total friction and fluid pumping losses when running normally	15 lb. per sq. in.
„ supercharging	19·5 lb. per sq. in.

Having arrived at what appeared to be the best possible timing from the point of view of volumetric efficiency, the actual tests were started. A Claudel carburettor was used, fitted with a needle valve in the jet, in order to adjust the mixture strength. The first results proved in some respects rather disappointing.

The main object aimed at, namely, the ability to work with a low mean mixture density, proved successful from the start, and it was found that the engine would run perfectly steadily with full supercharge even when the mixture admitted through the carburettor was rather on the weak side for normal running without supercharge. In spite of the extra work done in pumping on the underside of the piston, both the brake horse-power and consumption were improved by about 5 per cent when supercharging, the gain in indicated efficiency being nearly 10 per cent. which was in very close agreement with the theoretical gain. So long as the supercharge air was used only as a diluent and no attempt was made to increase the mean pressure beyond the increase due to the improved efficiency, all went well, but so soon as an attempt was made to raise the mean pressure to any substantial extent by enriching the mixture admitted through the carburettor, troubles began.

Although the flame temperature was lowered very considerably, as was proved by the gain in efficiency, the average temperature of the cycle was not apparently reduced to the extent that had been anticipated. When running normally with the most economical mixture the brake mean pressure obtained was 106 lb. per sq. in., corresponding to an indicated mean pressure of 121 lb. per sq. in.; when supercharging with the same gross fuel consumption the brake mean pressure rose to 112 lb. per sq. in. and the indicated mean pressure to 131·5 lb. per sq. in. When, however, any attempt was made to increase the mean pressure by enriching the mixture, severe detonation was set up which soon resulted in pre-ignition.

At this time very little was known about the phenomenon of detonation, which was supposed to be merely a premonitory symptom of pre-ignition and not, as is now realised, a perfectly distinct phenomenon, often the cause, but not the symptom of pre-ignition. At that period, however, the author, in common with most others, supposed that detonation and pre-ignition were kindred phenomena and that they were dependent solely upon the temperature of compression. This theory, however, did not appear to fit the case in point, for it was clear that the compression temperature must depend upon the ratio of compression and the initial temperature of the gases in the cylinder; since, however, the compression ratio in the cylinder was the same whether running normally or supercharging, and since the temperature of the supercharge was not high, while the effect of the scavenging was to replace the highly heated products of combustion remaining in the cylinder at the commencement of the suction with a certain proportion of comparatively cool air, it was difficult to believe that the compression temperature could be higher when supercharging than when running normally.

Lastly, the possibility that any uncooled part within the cylinder, such as the sparking plug points or the exhaust valve head, were reaching a higher temperature when supercharging was disposed of. A water cooled sparking plug was made up, and observation through the exhaust port showed that the exhaust valve, which was at a bright red heat when running normally, fell to a dull red when supercharging. Finally a stream of cold water was directed continuously against the underside of the exhaust valve head. Neither of these expedients proved of much avail. Although the author's belief that detonation and

pre-ignition were closely allied and dependent upon temperature had by this time received a considerable shaking, he nevertheless decided to make every possible endeavour to reduce the compression temperature.

The direct passage of air from the crosshead chamber to the cylinder was cut off by a diaphragm, and the air was forced to travel through an intercooler consisting of a nest of small tubes cooled with a liberal supply of cold water, in order to ensure that most of the heat of compression in the crosshead chamber and any conducted heat from the piston was removed before the supercharge entered the cylinder. This intercooler, which was destined to play an important part in the later experiments, had at this stage no effect whatever. Next, in order to increase the effect of the scavenging charge, the exhaust valve was set to open some 66 degrees early, or 10 degrees earlier than before, when, contrary to the author's expectation, the detonation and pre-ignition became far worse, so much so in fact that the engine would not run for any length of time on the supercharge at all, even with the weakest mixtures. This was somewhat of a surprise, and pointed to a new line of investigation altogether. The indicator was again fitted on the intercooler and diagrams were observed while running; it was found that when the exhaust valve opened 56 degrees early, the exhaust pressure in the cylinder at the moment when the ports were opened was slightly in excess of that in the intercooler, with the result that a very small quantity of exhaust gas passed out of the cylinder into the cooler and then back into the cylinder, followed by the air scavenge charge. When, however, the exhaust valve was set to open earlier this did not occur, the air entering the cylinder straightway.

As the result of this experiment, the author came to the conclusion that the pure air scavenging charge, instead of flushing out and cooling the cylinder, tended to combine with the products of combustion, some of which were then not completely burned down to CO_2, and so connived at and even assisted in the continuance of combustion during the exhaust stroke. The effect of passing some of the products of combustion out of the cylinder, cooling them below the ignition temperature and then re-admitting them, might be to prevent or, at all events, to greatly reduce the after-burning, and also absorb the heat liberated by re-association.

The next experiment tried was to open the exhaust valve 52 degrees early, with which setting the pressure in the cylinder was approximately 36 lb. per sq. in. when the ports were opened, with the result that a very substantial proportion of exhaust products passed through the intercooler.

Fig. 17 shows an indicator diagram taken from the intercooler under these conditions. With this setting it was found possible to increase the mean pressure to about 128 lb. per sq. in. on the brake, or 147·5 lb. per sq. in. indicated without detonation. This result appeared clearly to indicate that the lowering of the temperature brought about by eliminating the after-burning during the exhaust stroke had reduced the tendency to detonation and pre-ignition. A number of measurements were made of the heat carried away by the cooling water, both when running normally and when supercharging, and it was found that the

FIG. 17.—Indicator diagram taken from crosshead chamber when running.

proportion of heat so carried away was about 6 per cent less when supercharging.

During the same period, namely, the latter part of 1915, the author was carrying out another series of tests on the use of paraffin, and great difficulty was experienced with detonation. The experiment was then tried of admitting along with the paraffin, vapour and air a small proportion of cooled exhaust gases, as is done in the case of coke oven gas-engines; this proved so completely successful in eliminating detonation, and with it all tendency to pre-ignition, that the experiment was tried of admitting a small proportion of cooled exhaust gas into the cylinder of the supercharging engine along with the fuel and air through the main inlet valve. This had the immediate effect of entirely eliminating all tendency to detonate, and it was found possible, by merely enriching the mixture, to raise the brake mean pressure to 140 lb. per sq. in. and the indicated to 159·5 lb. per sq. in., which was the highest limit attainable, the whole of

the supercharge air being then saturated with fuel. The quantity of exhaust gas admitted in this manner was extremely small, and yet its influence was remarkable. The effect of this experiment was to raise doubts in the author's mind as to whether the after-burning during the exhaust stroke had really been the cause of the troublesome detonation. The exhaust cam was therefore set back to open the valve 66 degrees early, and a further test was made with cooled exhaust gas admitted through the carburettor, when it was found that the mean pressure could still be raised until the whole of the supercharge was saturated without being troubled with detonation, though a somewhat greater quantity of exhaust gas was required under these conditions. This indicated that the general rise of temperature due to after-burning was not largely responsible for the detonation, and it seemed probable that the most significant effect of opening the exhaust valve late was that a proportion of the exhaust gases passing through the intercooler did not re-enter the cylinder during the scavenging period, but remained in the crosshead chamber, and was re-admitted along with the supercharge.

As it might be rather inconvenient to admit exhaust gas through the carburettor in the case of a multi-cylinder aero engine, the method of admitting it along with the supercharge, and at the same time scavenging with cooled exhaust gas, was far more attractive. Accordingly, the exhaust valve was set to open at 48 degrees early, so that the pressure in the cylinder was still about 50 lb. per sq. in. when the ports were uncovered, and probably nearly one third of the total exhaust products passed through the intercooler, for the maximum pressure in the crosshead chamber rose momentarily from 10 to 28 lb. per sq. in. Under these circumstances the scavenging charge consisted mostly of cooled exhaust gas with a relatively small proportion of air, while the supercharge consisted mainly of air, but with a small proportion of cooled exhaust gas.

With this setting the engine behaved satisfactorily in every respect, and both objects were attained, for on the one hand the engine could be run on a low temperature cycle, using the supercharge as a diluent, and so attain a high thermal efficiency, and, on the other hand, the supercharge could, when desired, be used as an addition to the active working fluid, and a very high power output could be obtained, and, of course, any compromise was available between these two limits.

In Fig. 18 is shown a typical indicator diagram taken with the engine running normally and when supercharging—the fuel consumption and speed being the same in both cases.

Having achieved the main objects the next step was to pay attention to the frillings, and a number of minor improvements were made.

(1) It was found, as might be expected, that the relatively early closing of the inlet valve caused some loss of volumetric efficiency, though this was compensated for to some extent when supercharging, for the lower the pressure in the cylinder the greater the pressure-difference between that and the crosshead chamber, and therefore the greater the proportion of supercharge which entered. The inlet valve was already very large and the acceleration

FIG. 18.—Indicator diagram normal running and supercharging from working cylinder.

of the valve gear was very nearly up to the limit, so that little could be done in that direction; the author, however, adopted the expedient of recessing the seating of the inlet valve, so that it formed a mask. The valve then acted as a piston valve, giving a very quick cut off when entering the recess or mask, after which it could be closed on to its seating at leisure; since the pressure-difference on either side of the valve was relatively small during the period of entry of the supercharge and the early part of the compression stroke, the leakage between the valve and the mask was very slight. Fig. 19 shows the valve-opening diagram in relation to piston displacement (a) as originally used, (b) with the inlet valve masked to the extent of 0·060 in. This simple expedient* proved

* Patent No. 125659.

successful, and has been used in all the Tank engines and other engines the author has since designed, whether

FIG. 19.—Valve opening diagram with masked valve.

supercharged or not. The effect in the case of the engine in question was to increase the brake mean pressure when running normally from 106 to 109 lb. per sq. in.

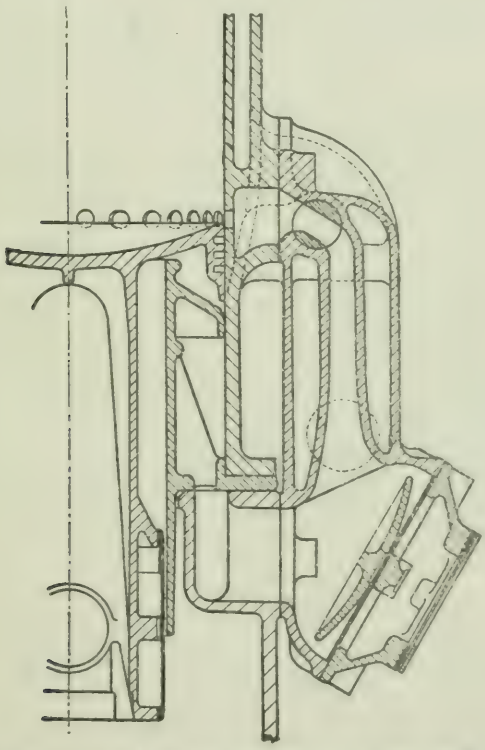


Fig. 20.—Section of crosshead chamber and intercooler.

at 1500 revs. per minute, and when supercharging from 146 to 147·5 lb. per sq. in.

(2) The intercooler, which originally consisted of a nest of very small tubes, was changed to a single flattened tube.

The small tubes with their many soldered joints were a source of weakness, and since the function of the cooler had changed from that of cooling an already fairly cool air

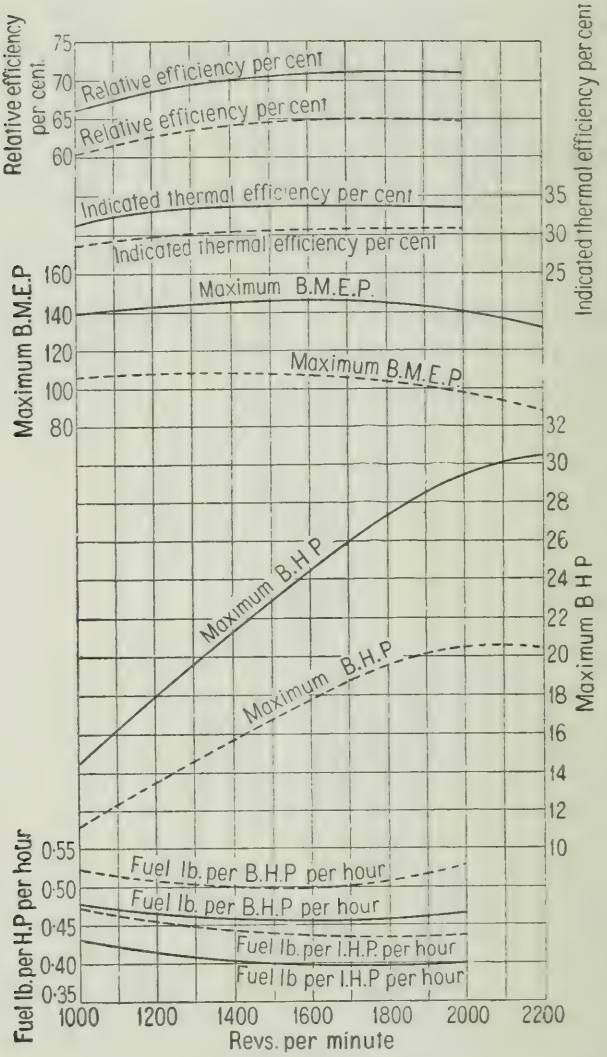


FIG. 21.—Summary curves of performance, full lines supercharging, dotted lines normal.

supercharge to that of dealing with a relatively small quantity of very hot exhaust gas, it was obvious that a greatly reduced cooling surface would suffice.

(3) It was found that the degree of stratification could be further extended and the economy and economical range of the engine could be increased by checking the rate of entry of the supercharge into the cylinder, and also

by making the top of the piston concave, which further encouraged the supercharge air to form a layer over the piston. In order to check the rate of entry a baffle was provided in the crosshead chamber near the entry to the intercooler. Ultimately the arrangement assumed the form shown in Fig. 20.

(4) The effect of cooled exhaust gases in checking detonation was so impressive that it was decided to raise the compression ratio from 4·7 to 5·1 : 1; still higher compression ratios up to 6 : 1 were tried and, although the economy was improved in proportion to the compression, it was then found necessary to add so much inert gas to the supercharge that the maximum power output was decreased.

With these modifications, the experiments so far as this engine was concerned were completed. The results obtained are shown in Fig. 21 and may be summarised as follows:—

NORMAL RUNNING.

Experimental supercharging engine: 110 mm. bore × 150 mm. stroke. Compression ratio 5·0 : 1.

Revs. per minute.	Maximum B.H.P.	Maximum B.M.E.P.	B.M.E.P. at most economical load.	Fuel, lb per B.H.P. per hour.	Fuel, lb. per I.H.P. per hour.	Ind. thermal efficiency per cent.	Relative efficiency per cent.
1000	11·2	106	103—105	0·525	0·475	28·6	60·3
1200	13·5	108	104—106	0·510	0·455	29·9	63·0
1400	15·9	109	105—107	0·501	0·443	30·7	64·6
1600	17·9	108	104—106	0·502	0·44	30·9	65·0
1800	19·7	105	101—103	0·515	0·438	31·0	65·3
2000	20·6	99	95— 97	0·535	0·440	30·9	65·0
2200	20·5	90	..	..	..	..	..

SUPERCHARGING.

Revs. per minute.	Maximum B.H.P.	Maximum B.M.E.P.	B.M.E.P. at most economical load.	Fuel, lb. per B.H.P. per hour.	Fuel, lb. per I.H.P. per hour.	Ind. thermal efficiency per cent.	Relative efficiency per cent.
1000	14·7	140	110—135	0·483	0·434	31·4	66·2
1200	18·0	143	112—137	0·465	0·416	32·7	68·8
1400	21·2	146	115—140	0·461	0·408	33·4	70·3
1600	24·6	148	115—142	0·459	0·402	33·8	71·3
1800	27·5	146	115—140	0·461	0·400	34·0	71·6
2000	29·6	141	112—136	0·470	0·405	33·7	71·0
2200	30·5	133	..	..	..	..	..

It may be argued that the normal power and efficiency of the engine were, compared with present-day practice, rather poor, but it must be remembered that the engine was designed nearly seven years ago, and also that the combustion head with valves in a side pocket was not of the form to give either the best power or economy.

There is, however, every reason to suppose that the percentage of improvement over normal running, both as regards power and economy, by the employment of a stratified supercharge will hold good whatever the normal performance may be.

The main object of all these experiments had been the application of the system to aircraft, the principal advantages being:—

(1) A very much greater gain in power in proportion to the increase of weight, the increase of weight being approximately 10 per cent, while the gain in power with fully saturated supercharge was nearly 40 per cent.

(2) The method provides automatically a means of compensation for altitude. If on the ground the carburettor is set to give a normal weak mixture, then the whole of the supercharge air is acting merely as a diluent, giving improved economy and a slight increase in power. As the machine rises the flow of petrol through the jet decreases as the square root of the density, while the weight of air decreases directly as the density; hence the mixture supplied by the carburettor gradually becomes richer and more and more of the supercharge is used as active working fluid, until at about 15,000 ft. the whole of the supercharge air is saturated. From ground level, therefore, to 15,000 ft. the power of the engine will decrease only as the square root of the density instead of directly as the density, as in the case of a normal engine, while throughout the whole of this range there will always be an excess of air present in the cylinder sufficient to ensure complete combustion, and the maintenance of conditions ensuring maximum economy.

(3) In a multiple cylinder engine any lack of uniformity in distribution is corrected, since if any one cylinder receives a richer mixture than the others, instead of the fuel being wasted, more of the supercharge air in that cylinder is carburetted and the power output increased.

All the experiments referred to above were carried out during

the years 1915 and 1916. During 1916, when the results had reached a reasonably satisfactory stage, an order was placed by the Naval Air Service with Messrs. Peter Brotherhood, Ltd., to build a twelve-cylinder supercharging engine of 600 b.h.p. At that date this was, to say the least of it, an extremely ambitious undertaking. Messrs. Peter Brotherhood, Ltd., however, actually built such an engine, though progress was necessarily very slow; all official interest in the engine was lost and the engine had become thoroughly out of date long before its completion. On test, this engine actually developed over 700 b.h.p., but the main crank case casting proved defective and developed serious cracks.

Towards the end of 1916 the author was requested to design a complete range of engines for Tanks, and the urgency and responsibility of this work compelled him to devote the whole of his time and thought to it throughout practically the whole of 1917 and 1918, so that the supercharging experiments had to be dropped for the time being.

It will be apparent from the above description of the experiments that although the system of supercharging with a stratified charge can be made to give the performance that might be expected from purely theoretical considerations, yet it is rather sensitive; much for instance depends upon the opening diagram of the exhaust valve and the closing diagram of the inlet valve, on the capacity of the intercooler and crosshead chamber, and on the position and area of the baffle, etc.

Thoughout 1917 and 1918 the engine was entirely monopolised for carrying out tests relating to the tank engines, and since the Armistice it has been devoted to carrying out tests on fuels, for which purpose the use of supercharging proved invaluable. A great deal of information has been obtained during the four years which have elapsed since the experiments were broken off, and many of the problems which puzzled the author at that time have now been cleared up. With the knowledge now available as to the phenomena of detonation, as to the important influence of combustion chamber design, and as to the treatment and influence of different fuels, very great improvements could undoubtedly be made. It is hoped shortly to build a larger and more up-to-date experimental engine on generally similar lines but embodying all the most recent information. The experiments described above occupied nearly two years of almost continuous research; apart from the

information gained on the subject of supercharging, the experiments provided a liberal education on the wider subject of high speed internal combustion engines.

In conclusion the author would like to express his thanks to Mr. C. W. Bryant and to all the technical staff of Messrs. Peter Brotherhood, Ltd., for their whole-hearted encouragement and assistance, and also to Mr. Le Mesurier and Mr. Everett, of Messrs. Armstrong, Whitworth & Co., Ltd., for their enthusiastic co-operation in the design and construction of an experimental aero engine embodying the principles outlined above.

THE DISCUSSION.

Dr. W. R. Ormandy, in opening the discussion, said: In the opening part of the paper the author refers to the loss which is brought about by the apparent increase in specific heat, and the absolute dissociation that takes place in the cylinder of an ordinary petrol engine running on the usual type of mixture, but I would point out that there are apparent specific heats, instantaneous specific heats and average specific heats. The author apparently complains that the increase in the particular specific heat and the increase in dissociation which occur with rich mixtures inevitably lead to a reduction of temperature and reduced efficiency, and yet we were told that the main object of the experiments was to endeavour to reduce the temperature in the cylinder in order that the efficiency might be increased. It seems to me that if the instantaneous specific heat increases, as we know it does, when the temperature increases, say, from 1,500° to 2,500° C.—because the increase in specific heat is comparatively slight below 1,500° C.—then the whole question turns upon whether the added heat energy that has had to be put into that fluid to raise its temperature from 1,500 to 2,500° is given out again as it cools down from 2,500° to 1,500°, assuming that the gases are at 1,500° as they leave the exhaust valve. Similarly, if dissociation does take place, as we know it must at these high temperatures, it seems to me that the dissociation merely results in the production of something of the nature of a Diesel effect in the engine, that is to say, that the oxygen endeavours to combine with the carbon and hydrogen to form CO_2 and water, and a certain proportion of carbon and hydrogen are present as carbon monoxide and hydrogen, which cannot combine at the maximum temperature, and have to wait until the temperature has been reduced, and, consequently, give out their heat a little lower down on the stroke. It is obvious that the higher temperature attained without dissociation should lead to slightly higher efficiency, but against this has to be balanced the much greater loss of energy due both to radiation and conduction, the first varying as a high power of the temperature. Therefore, I would suggest that we really are working, to a little

(Dr. W. R. Ormandy.)
extent, in a vicious circle by dealing alone with pure theory. It is eminently desirable that all doubt should be removed by measurements of the temperature of the gases leaving the cylinder and by analyses of the gases at the end of the explosion stroke. I know that it is a very difficult problem, because the moment an exhaust valve opens the gases expand suddenly—but I do believe, on the other hand, that the even more important question as to the degree of dissociation would be settled by exhaust gas analyses. Knowing the author's experimental skill, I think that it is not at all beyond his powers to design an arrangement of a half-time valve which would enable samples to be drawn from the cylinder during the explosion stroke, because, although it may be said that the combustion goes on after the gases have left the cylinder, it is possible to stop that combination, because it has proved possible to take samples of gases from the great electric arcs that are used in the production of nitrous oxide fumes with the arc method of making nitric acid by using water cooled copper capillaries; these analyses have shown in that case that the composition of the gas is practically that which theory would lead us to expect.

I would like to make the suggestion that we consider stratification from another hypothetical standpoint, not using the paper bag referred to by the author. Let us suppose that the ideally carburetted charge, whether it amount to 10, 20, 40 or any other per cent of the total cylinder volume, be capable of existence as a thin layer at the head of the cylinder, where it may exist in any required degree of turbulence, and that underneath this, separated as it were by a plane surface, we have the necessary amount of air and/or exhaust gas to complete the charge, and that in this lower layer also any required degree of turbulence may be attained. I believe that if we could get that, which we may call ideal stratification, accompanied by ideal turbulence, we should not get the results the author has got with his engine, and I believe he has got them because he has not got ideal stratification. It is almost impossible, I think, to conceive 10 per cent of the possible ideal perfect charge getting into the cylinder and being present as a very thin layer at the top, and being in contact with the mass of metal at the top, and during this very rapid combustion, burning at the very high temperature which it would attain under these conditions, and give the efficiency which he has obviously got. It seems to me that what really does happen is that from the rich

entrant charge of gas we have a charge which swirls round in various ways, and the maximum result is attained when the mixture is gradually diluted in such a way that the high temperature obtained at the commencement of the explosion is sufficient to cause ignition and rapid combustion in the weaker mixtures that lie below. Professor Dixon has made experiments on these lines, but not with petrol. Imagine that we have a tube divided into two portions, the first of which, provided with sparking points, can be filled with a theoretically correct explosive mixture, and the second portion of which can be filled with a weak mixture containing any required percentage of the ideal mixture. It has been shown that if the two portions of the tube be opened to one another and the perfect mixture be fired electrically, then the combustion wave will travel through and burn up mixtures contained in the second portion of the tube which are too weak to burn under ordinary conditions. It would be of real value to have further experiments carried out along these lines, and one reason why I wish to mention this is to emphasise a point which I am always anxious to push home, namely, the close inter-relation between what we may call pure theoretical research and its immediate bearing on practical problems. It would be very interesting to know how weak a mixture we can actually burn, given a certain impetus in the explosion which has been fired into it to set it off. The velocity with which this explosion or combustion wave proceeds from the starting point varies, and it goes more and more rapidly. If the first part of the tube be long enough, it will finish almost instantaneously, because we shall get an explosive wave.

What velocity must be attained in the rich mixture near the sparking plug in order that we may burn a 50 per cent mixture, and how far can we go with dilution of this sort and still get complete combustion? Then we should know that we had to get that mixture, not exactly stratified, but graduated, in such a way that at all points there would be a flame velocity and temperature sufficient to burn the fuel at that particular point.

With reference to the question of detonation, the author spoke of the tremendous effect brought about by allowing a comparatively small percentage of exhaust gas to be present with the mixture that went in from the carburettor. Before the meeting I asked whether, if nitrogen had been introduced instead of exhaust gases, the result would have been the same, but the

(Dr. W. R. Ormandy.)
author told me that the results would be more pronounced if exhaust gases were used. This is probably because they contain a large amount of carbon dioxide and water, which are tri-atomic gases having high specific heats. If that be the case, we may assume that the reason they produce that very pronounced effect is that they reduce the temperature and slow down combustion immediately surrounding the point where the explosion starts. We know in the case of paraffin, for instance, that the time occupied before we reach the point where there is a very sudden increase in pressure is exceedingly short.

I would like to point to some experiments in an entirely different field which appear to have a bearing on this, but before doing so I would like to refer further to the author's really most important and interesting discovery that there are certain types of fuels the detonation of which is brought about readily by a comparatively small increase in pressure. He has found, I believe, other fuels the detonation of which is less affected by pressure, but which is brought about very readily by a slight increase in temperature. In other words, certain fuels are sensitive to temperature, and others are sensitive to alterations of pressure. The whole of the author's work during the last few years has been, I take it, directed towards an endeavour to correlate the chemical nature of the fuel with its behaviour in an engine, and the next stage, the stage we must aim for—I will not say the ideal stage—is to find out what physical property of the fuel it is that causes it to behave in this manner. We want to take the discovery back a stage further, and I would like to refer to certain experiments done by Moore on the spontaneous ignition temperature of fuels. Moore has found that if into a crucible contained in an iron block, into which crucible oxygen or air is passing, a drop of fuel be allowed to fall, then at a certain temperature of the block a slight explosion will take place, and the lowest temperature at which it will take place is called the spontaneous ignition temperature. When oxygen is employed, this point is definite to within about 3° C., and in the case of petrol it is something of the order of 280° C. Now, if the same experiment is carried out with air instead of oxygen, it will be, say, 380°. We may regard the effect of using pure oxygen as being equivalent to using air under a pressure of something like four atmospheres. The oxygen atoms or molecules are much closer together; they are about four times as near in pure oxygen, at any rate, as they

are in air at normal pressure. But Moore has also found that there are certain fuels which have almost the same spontaneous ignition temperature in air as they have in oxygen. In other words, these particular fuels would appear not to be sensitive to variation in pressure. Further, he has found that by starting with pure oxygen the spontaneous ignition temperature is, say, 280°, but if a mixture of 50 per cent oxygen and nitrogen is used the ignition temperature is altered. It seems to me that we have got here two entirely different trains of thought, both dealing with the one subject of the spontaneous ignition temperature, which is, after all, nothing more or less than the detonation temperature when it takes place in a cylinder under pressure, and they suggest that there is a correlation. It would be desirable to have a range of experiments carried out, and instead of using oxygen mixed with nitrogen, to take oxygen mixed with exhaust gases, and plot a curve showing the behaviour of petrol and benzol, the behaviour of which latter fuel is different from that of petrol. Benzol has a spontaneous ignition temperature in oxygen of 620°, and that, of course, is borne out by the author's discovery that the compression to which benzol can be used is 7 to 1, or slightly over, and with many petrols only 4 to 1 or 4·6 to 1. It would be interesting to carry out a range of experiments using petrol and benzol, and employing oxygen with varying percentages of exhaust gases, and plotting out a curve of the temperatures at which spontaneous ignition takes place, and seeing what relation it bears to the behaviour of similar fuels in a supercharged engine, which is a very valuable means for examining into these problems of combustion, which, although at first sight appearing to be purely of theoretical interest, are, after all, fundamental, in that it is by knowledge of them that we shall more rapidly advance in the designing of engines which are more suitable for their purpose than the present type. I must, in conclusion, take the opportunity of expressing my thanks personally to the author for giving us this paper. I submit that it is not possible to get all the good that is buried therein without reading the further work that he has done, though this has as yet only been published in abstract in a monthly journal, but it will, I hope, be eventually published in complete form, together with the work which he has in hand for the Empire Motor Fuel Committee, and such other work as has a strong bearing on it, such as whatever may be done in the direction which I have indicated.

(Mr. A. A. Remington.)

Mr. A. A. Remington: I hope we shall have more papers of this sort in the near future dealing with the problems facing us in our endeavours to improve the internal combustion engine as applied to motor cars, because, after all, the small high-speed internal combustion engine is the heart of our industry. During the earlier part of the paper the author made a very great point of telling us that the experiments were made with a homogeneous mixture. I am sorry to say that in motor cars running on the road that is an ideal mixture that we very rarely attain, and, therefore, a percentage has to be taken off all these results for the fact of the mixture being non-homogeneous. This is a varying percentage, varying very considerably in many cases, and I think it would be of great interest if the author would indicate the means by which he made his mixtures homogeneous, the means he took to ascertain that they really were homogeneous, and also the means he took to ascertain the exact strength of the mixtures, which he variously gives us in terms of B.Th.Us. per cu. ft., and ft.-lb. per cu. in., and give us some idea of the probable accuracy of the determinations. In dealing with research work of this sort, the accuracy of the determinations is a very important factor as affecting the exact position and, sometimes, the form of the curves, and I notice that all the curves in the paper are nicely smoothed out, so that a little information on the degree of accuracy of the exact points from which these smoothed-out curves were made would add greatly to the value of the paper.

I remember a good many years ago reading an account of the experiments that Dr. Dugald Clerk made on a National gas engine with a supercharging device, or, rather, a mixture-density increasing device, in which he passed exhaust gas out on the exhaust stroke, cooled it to a considerable extent, and passed it in again on the compression stroke, and thereby increased the density of the mixture and obtained improved economy. That, I take it, is a forerunner of what the author has been describing to us to-night in a rather different form, and doubtless the author is thoroughly familiar with Dr. Dugald Clerk's experiments. I remember thinking, when reading of these experiments, whether it was worth while trying a similar sort of device in connection with a motor car engine, and whether the extremely high speed of the engine would upset the effect. Later, I believe it was in 1909, I had a great deal to do with the testing of an engine that was known under the name of Kessler, the name of the inventor, who was an American. In this engine the crank chamber was used as a com-

pressor, and air compressed in the crank chamber could be admitted into the cylinder towards the end of either the working stroke, to augment the charge, or the end of the exhaust stroke, to scavenge, or both. Tests were made in all three ways, and we found that the greatest effect was obtained by using the whole of the air so compressed, which, after all, was very much less than the author obtains, as we did not get anything like 5 lb. per sq. in. pressure in the crank chamber, to augment the charge, and found that scavenging was a very doubtful advantage. This engine had not got a very high volumetric efficiency, and even with all the air used to augment the charge, the mean effective pressure was only what could be obtained to-day out of an engine of quite ordinary design, namely, about 97 to 100 lb. per sq. in. on the brake. Another point on which I should like more information is why such a small percentage of exhaust gas that has been cooled and added to the vastly greater percentage of nitrogen mixed with the charge of oxygen in the cylinder should have the effect of preventing the detonation which occurs when the small amount of cooled exhaust gas is not admitted. Information as to the actual amount of augmentation of the charge with exhaust gas, and the temperature of that exhaust gas, might throw more light on the subject. The charge presumably consists of approximately four-fifths nitrogen. What is the percentage of exhaust gas added? Is it merely the extra percentage of inert gas that has the effect, and, therefore, more nitrogen would do the same, or is there some peculiar influence in cooled exhaust gas as compared with nitrogen? In the early part of the paper it seemed fairly clear to me that the cooling reduced the after-burning, but the author rather upset that later on when he suggested that it was really CO_2 plus water rather than cooling that had the effect, and when he dealt with the matter in connection with paraffin it left me still more mystified, and I hope he can clear up the point.

Mr. W. S. Renwick: Under certain conditions, perhaps rather in more old-fashioned engines than the author has been dealing with, exhaust valves can be very badly burned by running on a weak mixture. We used to have a theory that this burning was caused by passing the oxygen, which must be present in the products of combustion of a weak mixture, over the highly heated exhaust valve. Some of the author's mixtures must have been quite weak enough to supply enough oxygen for burning the valves, and at the same time strong enough to get the valves hot.

(Mr. W. S. Renwick.)

In view of the author's success with running on mixtures of all sorts and conditions, presumably his valves did not burn, so I would like to ask if we have got to give up this theory? and if so, whether the author can give us another in its place?

Mr. A. E. Berriman: I should like to ask the author whether it is before or after ignition that turbulence of the charge assumes the greatest importance. I would also like to say that I regard the author's endeavour to find a practical method of securing higher economy at partial load by means of stratification as an enterprise of the greatest value. The paper itself I consider to be the best we have had for many years, and I feel that we shall look back on the present occasion in time to come as one of our Institution's historical nights.

Mr. E. B. Wood: I am particularly interested in this paper from the view that it may give a new point of attack on the paraffin or low grade fuel problem. With regard to what Dr. Ormandy has said, I have often noticed during carburettor experiments that a taper will light a mixture which cannot be lighted by a spark under compression. It might be said that the fuel wanted some time to evaporate on starting, but it does not seem to be that. A very weak mixture could be lighted in that way, and it was useful as a sort of rough check to find out what the mixture really was. I am not, however, quite satisfied that this detonation or spontaneous ignition question is not one purely of time and temperature, the exhaust gas tending to increase the time required for the fuel to ignite or burn at a certain temperature, and, as Dr. Ormandy has said, the ignition temperature is a range rather than a definite temperature, depending on the proportion of diluent with the oxygen. I think Professor Bone* found that hydrocarbon would combine with oxygen at comparatively low temperatures, such as 250° to 300° C. The temperature rise caused by compression is theoretically the same, of course, whatever the initial pressure of the charge, but the initial temperature of the charge has a very great influence on this naturally, and, as far as I know, this temperature is to a great extent guesswork. It is an exceedingly difficult thing to measure, and will naturally vary with the amount of fuel burned. The heating effect on the charge, passing over an inlet valve which may be at a temperature up to 600° C.,† must be considerable. In some ways the results seem to

* See Proc. I.A.E., Vol. I., p. 21.

† See Proc. I.A.E., Vol. XIV., p. 32.

suggest that the absence of detonation in the lower compression engine is due almost as much to the increased proportion of exhaust products in the charge as to the lower compression ratio. I am rather surprised, however, that so eminent an authority as the author makes the unqualified statement that the volumetric efficiency is within 1 or 2 per cent of that possible, when he admits that he has made no direct experiments, and a simple calculation shows that by reducing the initial temperature, or, rather, the temperature at the beginning of compression, from 117°, which the author gives in his diagrams, to, say, 60°, the volumetric efficiency was improved somewhere in the neighbourhood of 14 per cent.

Mr. J. S. Wilkinson: Can the author give any information as to the thermal efficiency of his supercharging engine compared with that of a normal petrol engine at light loads, that is, loads of from 0·3 to 0·8 full load? As most petrol engines are working between these limits for the greater part of their life, it would be of considerable interest to have the relative values of the brake thermal efficiencies for these conditions, perhaps, in the form of curves.

Mr. W. A. Tookey: The author's work has been connected with petrol engines mainly and similar liquid fuels. My work, on the other hand, has been mostly in connection with gas, which is usually thought to be a very much inferior fuel, but I think the results that we get with gas are indicative of what we might expect from petrol if it were possible to use petrol in rather weaker mixture strengths. I am pleased to note that the author, I think for the first time in any scientific research, has expressed his values of mixture strength as so many B.Th.Us. per cu. ft. That is a standard basis by means of which internal combustion engineers can co-relate the performances of liquid fuel engines with gas engines, and, to my mind, it is a very distinct step forward, and one upon which this Institution is to be congratulated. I have expressed the mixture strength of gas engines in terms of B.Th.Us. per cu. ft. for several years past, and I have now a lot of useful information on that basis. I can, however, never obtain the rich mixture strengths with gas which the author can obtain with petrol. He speaks of the order of 85 to 90 B.Th.Us. per cu. ft., and I take it that in so speaking he is referring to the strength of the mixture as admitted, *i.e.*, in terms of volumetric displacement of the piston. I think it would be very convenient if, as an addendum to his paper, he could also express that in terms

(Mr. W. A. Tookey.)

of the total cylinder volume, because then we get, I suggest, a still better scientific basis, in that we rule out the compression ratio. In his 5 to 1 volume ratio engine we should have a decreased mixture strength, not of 85 to 90, but four-fifths of that, say, of 68 to 72 B.Th.Us. per cu. ft. It is interesting to realise that Dr. Watson, when he read a paper* before this Institution many years ago on tests on a single-cylinder Daimler engine, gave a lot of useful test information, and I remember then calculating the petrol mixtures he was using in terms of B.Th.Us. per cu. ft., and, if my memory serves me rightly, Dr. Watson's results were round about 66 B.Th.Us. per cu. ft., taking the total cylinder volume into account, and not the piston displacement volume only, as the author has done. I made a very interesting set of experiments some four or five years ago in connection with mixture strengths of town gas and air, which is, I think, rather valuable. The experiments were carried out on a three-cylinder high-speed gas engine (8 in. diameter, 7 in. stroke running at 600 revs. per minute) working on town gas running at no load, and the mixture strength on the basis of total cylinder volume came out at about 28·5 B.Th.Us. per cu. ft. For the sake of the experiments we cut out one cylinder of the three, when the consumption of the engine immediately went down, and the amount of gas supplied to the engine gave a mixture strength of 26·5 B.Th.Us. in each of the two cylinders. We then proceeded to cut out the second cylinder, thus leaving two pistons merely pumping up and down. The total consumption again decreased, and the mixture strength supplied to the one cylinder was 31·8 B.Th.Us. per cu. ft. of total cylinder volume. Hence, taking the mixture strengths as 1·0 for one cylinder, the gas consumption had to be increased to 1·67 for the two cylinders, and 2·7 for the three cylinders. That, I think, shows that there is a certain critical strength which must be worked to in order to secure regular ignition, and that critical strength in a gas engine is very much less than the author has noted in his petrol engine. In my experience, the weakest mixture strength that has been used in connection with a gas engine is in the neighbourhood of 15 B.Th.Us. per cu. ft., but for high-compression, "cold starting," liquid fuel engines, and also for two-cycle crank-case-compression liquid fuel engines, the mixture strength at "no load" and normal running speeds has been proved to be as low as 8 to 9 B.Th.Us. per cu. ft.

* See Proc. I.A.E., Vol. VII., p. 35.

The PRESIDENT: I am sure all will agree with the remark made by Mr. Berriman that this is a paper which will become historical. It is a matter of great regret to me that, when I was Superintendent of the Royal Aircraft Factory at Farnborough only two of the very few engines we built were on the principle described in the paper, and I trust we did something to forward this very important question. We are very much indebted to the author for having placed before us so lucidly so important a subject, and I hope that when some of these further experiments are carried out he will again give us the benefit of the work he has done, which is leading, undoubtedly, to a great advance in automobile work in this country.

Mr. RICARDO, in replying on the discussion, said:—Dr. Ormandy opened with a collection of posers, some of which are very difficult to answer, but all of which are very much to the point. He raised first the question as to the assumptions I made with regard to apparent specific heat and dissociation. I should like to say that I used the words apparent specific heat rather advisedly, because the term covers the two. With regard to dissociation, he pointed out, very rightly, that if the expansion is carried down to a certain point, re-association will take place, and that there will be no loss. That is quite true, but unfortunately we do not carry the expansion down to anywhere near that point. We found on an experimental variable-compression engine that on every fuel any increase in compression ratio was followed by a very considerable increase in efficiency, very much greater than the Air Cycle would indicate, and I think there is no doubt that that is due to the fact that as the expansion ratio was increased and the final combustion temperature reduced the recovery due to re-association was increased.

Dr. ORMANDY: But analysis will prove it.

Mr. RICARDO: Yes, but analysis, to within the limits of accuracy called for, is, I think, a very difficult thing to carry out. I feel that Dr. Ormandy has been suggesting lines of research, with which I heartily agree for the most part, but I think that there is already a great deal of evidence in support of Tizard and Pye's deductions; for instance, with the variable compression engine just referred to, and when working on a compression ratio of 4 to 1, with all other conditions as nearly identical as possible, we get an efficiency relative to the Air Cycle of

(Mr. Ricardo.)
about 64 per cent. Working on exactly the same mixture strength and altering nothing but the compression, which can be done in a few seconds we get, with a 7 to 1 ratio, a relative efficiency of 69 per cent. That is a very marked increase, and it is very difficult to see any other explanation for it than the, at least, plausible one, that the losses are due to dissociation, and that they therefore diminish as the expansion is increased. With regard to the question of stratification, Dr. Ormandy raises a very interesting point, and suggests that what is going on in the supercharged engine is substantially what he has defined. I think it is highly probable that is the case. In the supercharged engine we were stratifying to a very limited extent. We were only extending the range on the weak side from 85 B.Th.Us. down to 60 B.Th.Us., and I think it is quite possible that what he described would happen. In the case of the other engine, in which I tried to investigate the effect of stratification over a wide range, I do not think it could happen to anything like the same extent. In this particular engine the rich combustible mixture was virtually in a cartridge which was exploded, so that the process of combustion came very near to the paper bag analogy. I quite agree that turbulence was created largely after the commencement of ignition, but there was also a very considerable degree of turbulence before; no doubt the immediate effect of ignition was to bring about a regular and thorough stir-up due to the explosion in the cartridge. The question of nitrogen instead of CO_2 as a diluent is one which we are hoping to investigate, and I do not feel competent to say very much as yet, except that there are marked indications that exhaust products are much more effective than nitrogen. It may be, partly, that their specific heat is higher, but this does not seem to explain it entirely. We found, for instance, that when CO_2 is admitted to an air petrol mixture of normal density the throttle can be opened wide, and that mixture can be steadily drowned by the addition of CO_2 until the engine is running, still with wide open throttle, on no load. I have actually had an engine running idle perfectly steadily at about 1,000 revs. per minute with wide open throttle on full mixture strength, admitting CO_2 to the carburettor. There are various possible theories that can be suggested, but I do not think any one of them is sufficiently certain. There appears to be a good deal of evidence in support of the theory that the CO_2 slows down the rate of burning to such a degree that, in spite of ample turbulence, com-

bustion is nowhere near complete by the end of the exhaust stroke. Experiments on these lines are somewhat involved, and it is dangerous to draw too many deductions from them. Dr. Ormandy also raised the very interesting point, namely, the difference in the observed ignition temperature of certain fuels in oyxgen and in air. I certainly agree that that is very important, and I agree that it suggests a line of research to be followed. We should certainly investigate detonation both in an actual engine and under adiabatic compression with samples of fuel which show large and small divergencies in this respect.

Mr. Remington raised the question as to how I obtained a homogeneous charge, and whether it was really homogeneous. I am afraid that, perhaps, my paper was rather misleading. I spoke of a homogeneous charge not as indicating that it was truly and strictly homogeneous, but rather to distinguish it from a stratified charge in which there was deliberate separation. I did the best I could to get a homogeneous charge, in the later experiments at all events, by actually boiling the fuel and passing the vapour and air through a number of hot perforated plates; in point of fact, however, I found in the case of a single-cylinder engine no perceptible difference as regards homogeneity compared with the results obtained when working with an ordinary Claudel carburettor. Mr. Remington next enquired as to the accuracy of the determinations. Here again, I wrote this paper recently on work done several years ago. The accuracy of the determinations as regards measurement of power output and fuel consumption was very close; I think I can say it was certainly within $\pm$ half of 1 per cent. The accuracy with regard to air measurement, on which I based the mixture strengths used, was deduced from indicator diagrams and was only an approximation, but since these experiments were carried out I have had opportunities of making very accurate measurements of air consumption with a special direct-reading air measuring device, and I have gone over these figures and checked them with more accurate readings taken subsequently, and on the whole I find they are pretty nearly correct. These readings can only affect the figures for mixture strength: all other readings may be taken as accurate to within about half of 1 per cent. I was very interested in what Mr. Remington had to say about the Kessler engine with scavenging and supercharging, and particularly his remark that the scavenging was of very little value. We tried some experiments on scaveng-

(Mr. Ricardo.)
ing without supercharging, and I can entirely endorse his experience. In point of fact, the effect of scavenging out the residual exhaust products is greatly to increase the tendency to detonate, and therefore to necessitate the use of a lower compression. A great many patents have been taken out for scavenging the exhaust products, but the one thing required is to keep them in and keep them cool, and I generally wish I had more of them, at all events in high-compression engines. It is very difficult as regards these supercharging experiments to give any idea as to the quantity of exhaust gas used, because I had no basis on which to judge, but I have since done a lot of experimenting on another engine with variable compression in which cooled exhaust gases were sometimes added, and I can give at least some approximate figures. Speaking from memory, a fuel that detonates normally with 4·8 to 1 compression will run on a 6 to 1 compression with about 4 per cent of the volume of air displaced by additional exhaust products; when running with a compression of 7·5 to 1 it required about 25 per cent. Judging from what I have found since, I should say that in the supercharging engine the proportion of exhaust products over and above the natural residual products in the cylinder was from 4 to 5 per cent of the swept volume.

Mr. Renwick raised the question of the effect of burning exhaust valves with weak mixtures. I think that that really only applies when we are working with what I have been calling a homogeneous mixture which is too weak, so that the combustion is very slow and is continuing during the exhaust period, i.e., combustion and re-association are going on very actively during the period the valve is open. When working on a stratified charge, so that combustion is as rapid and complete as when working normally with a strong mixture, then my experience has been that the exhaust valves keep remarkably cool, in fact I have found that on the stratified charge engine a polished steel exhaust valve $2\frac{1}{4}$ in. diameter just reached a pale straw colour after two hours running at a mean pressure of about 30 lb. per sq. in.

Mr. Berriman raised the point as to whether the turbulence took place after ignition or before. As far as my experience goes, in the case of the first engine certainly, the bulk of the turbulence was created during the suction stroke, though it was no doubt largely augmented by the ignition of the first portion

of the charge, but I am by no means so sure that the same applies to the supercharged engine. In fact, I got better and better results on that engine by taking actual steps to check the turbulence in the air admitted as a supercharge.

Mr. BERRIMAN: Is a pocket necessary?

Mr. RICARDO: No, it is not necessary, and on another engine we have built we dispensed with it. I am quite inclined to the theory that the stratification is not very complete in the supercharged engine, and that it operates very much on the lines Dr. Ormandy has suggested.

Mr. Wood raised the point that I had given a definite figure for volumetric efficiency, and said it could not be improved by more than 2 or 3 per cent. That is based on the assumption that the temperature at the end of the suction stroke is 117° C., and Mr. Wood says, "Why not reduce that to 60°, and you will get a great increase in volumetric efficiency." How will you do it? The temperature I have taken for the entering gas is about the minimum at which petrol will remain as a vapour, and I have then allowed for heat taken up from the cylinder walls, etc., and by admixture with the residual exhaust products in the cylinder. If he can produce a lower temperature, and therefore a higher volumetric efficiency, so much the better; nothing would be more welcome. I do not agree that it is difficult to ascertain the temperature at the end of the suction stroke when means are available for determining the absolute pressure at this point and the volumetric efficiency.

Mr. Wilkinson raised the question of running the supercharging engine on light loads. Generally speaking, for running on really light loads, it was found advisable to cut off the supercharging, otherwise there was a considerable tendency for the engine to be drowned out. For any load from about 90 lb. per sq. in. brake mean pressure up to 140 lb. the consumption was better than the best that could be obtained when running normally, but below about 80 to 90 lb. per sq. in. mean pressure it was always advisable to cut off the supercharge. The best consumption per brake horse-power was obtained when running at from 115 to 135 lb. per sq. in. brake mean effective pressure, but it varied very little over a much wider range.

Mr. Tookey raised some interesting points with regard to mix-

(Mr. Ricardo.)
ture density. The figures which I gave for mixture density represent the actual proportions of fuel and air, or the heat value of the mixtures of fuel and air entering the cylinder, not the heat value of the mixture inside, including the residual exhaust gases, but even taking the heat value of the mixture within the cylinder I cannot agree that any homogeneous charge of 66 B.Th.Us. per cu. ft. could have been used. The very minimum that could be got to ignite under the best conditions is between 80 and 85, that is, of the air and fuel before entry. The proportion by weight of residual exhaust gases in an engine with a 5 to 1 compression is only about 7 per cent, so that that will reduce the minimum figure from 85 to possibly 78, which I think is the extreme limit at which it is possible to work with petrol. With gas, of course, the mixture is inherently very much weaker in heat value, because a considerable portion of it is inert. Even with town gas, for instance, the gas/air ratio is about 10 to 12 per cent, while with petrol it is only about 2 per cent, so that in the former case the proportion of oxygen present, which really controls the available heat energy of the mixture, is about 10 per cent less than in the case when a petrol mixture is used. Also with town gas, owing presumably to its large inert content, or possibly to its relatively large hydrogen content, a very much wider range of mixture strength can be worked with on the weak side. I have found I can work on ordinary town gas from a mixture of 75 B.Th.Us. per cu. ft. down to about 45 or 50. That is a very wide range on the weak side compared with petrol. I have not seen any figures so low as those which Mr. Tookey speaks of, but those he quoted in the case of the experiments on a 3-cylinder engine at very light loads are rather in agreement with the point I mentioned before, namely, that the mixture can be drowned down with inert gas to quite an extraordinary degree; the point I should like to enquire about in connection with these experiments is what was the thermal efficiency of the engine when running under these conditions, because drowning in that way allows tricks to be played with the mixture strength, but does not give any advantage in the way of thermal efficiency.

Dr. Ormandy's points are mostly, I think, directions in which further research can be carried out. I am grateful to him indeed for suggesting them, and I shall have great pleasure in attacking them as far as I can.

In conclusion I would like to say that I am afraid the paper is rather ancient history. I had hoped to carry out a number of further experiments on the same subject, but unfortunately I have been so full up with a lot of other experimental work, chiefly on fuels, that I have not been able to get the supercharging engine going again.

COMMUNICATION.

Mr. E. G. Ritchie wrote: The author confines himself entirely to the influence of supercharging upon the power output and efficiency of an engine operating under ground-density conditions, and I propose to discuss the various experimental results obtained from this point of view only. The question of the supercharging of aero engines opens up a much wider field of controversy. All the results obtained by the author are discussed by him with reference to stratification, and in this respect it is doubtful whether stratification to the extent implied in the paper does take place except at low engine speed. In the preliminary tests outlined on page 724 et seq., it is stated that the engine could be run quite regularly on a mixture strength corresponding to a calorific value of 10 B.Th.Us. per cu. ft. of charge. This corresponds to an air petrol ratio in the neighbourhood of 150 to 1. In the normal engine, mixtures weaker than about 16 to 1 give irregular running, and further reduction in mixture strength results in slow burning and back firing. The operation of the engine on a mixture strength of 150 to 1 therefore demands the separation of the charge in such a manner that approximately 90 per cent of the air is quite inert so far as the combustion process is concerned. Details of the cylinder design are not given, but the results obtained clearly indicate a state of affairs corresponding to extreme pocketing, and while this apparently permits of the engine being run on an abnormally weak mixture when developing a very low mean pressure, such pocketing will undoubtedly have a detrimental influence upon power output and efficiency on full throttle or at high speed. That such is the case is clearly indicated in Figs. 8 and 9, which show that the consumption increases and the efficiency diminishes rapidly with increase in mean effective pressure.

In Fig. 8 curves are given with a view to showing the superiority of power control by regulation of the petrol supply and stratifi-

cation over control by throttling. I submit that the two curves are not strictly comparable. The consumption-power curve when throttling is dependent to a large extent upon the characteristic of the carburettor, and while the particular carburettor employed shows to disadvantage, another carburettor might have given results equal to or better than those obtained by direct control of the jet.

On page 727 it is claimed that the results obtained demonstrate the value of stratification in so far as it permits of the development of higher thermal efficiencies. Examination of the plotted points of Fig. 9 indicates that with a mixture strength of approximately 10 ft -lb. per cu. in., efficiencies as low as 0·305 and as high as 0·370 were observed. This represents a greater difference than was observed over the whole range of mixture strengths tested, as shown by the mean curve of Fig. 9.

From Figs. 8 and 9, the relationship between indicated mean effective pressure and efficiency is approximately as follows:—

I. M. E. P.	40	50	60	70	80
Thermal Efficiency	0·345	0·340	0·335	0·330	0·325

From the above, it will be observed that the variation in thermal efficiency with mean effective pressure is not of large magnitude, and might indeed be accounted for by change in any one or more of the many experimental variables. The engine upon which the tests were carried out was unjacketed, and it is suggested that the decrease in efficiency with increasing mixture strength and increasing power output is due in large measure to lack of cooling. Had the cylinder been jacketed, it is highly probable that the efficiency would have increased with increase in power output. The highest efficiency recorded is approximately 37 per cent with the engine developing an indicated mean effective pressure of 30 lb. per sq. in., while with a mean effective pressure of 80 lb. per sq. in. the thermal efficiency was 33 per cent. Results which I recently obtained on a normal engine having a bore of 100 mm. and a stroke of 140 mm. are given in Table I. These were obtained with a compression ratio of 5·1 : 1.

(Mr. E. G. Ritchie.)

TABLE I.

Conditions.	R.P.M.	Mean Effective Pressure.		Petrol Consumption.		Thermal Efficiency per cent.	
		Indicated.	Brake.	lb. per I.H.P. per hr.	lb. per B.H.P. per hr.	On I.H.P.	On B.H.P.
Maximum load Mixture.	1600	144·5	122·7	0·406	0·477	33·7	28·7
	1800	142·1	118·0	0·414	0·499	33·1	27·5
	2000	137·6	110·3	0·374	0·468	36·6	29·3
Most economical Mixture.	1600	137·3	116·6	0·383	0·450	35·8	30·4
	1800	136·2	113·2	0·381	0·459	36·0	29·9
	2000	135·2	108·0	0·360	0·450	38·1	30·5

The above figures indicate that without additional mechanism the normal engine is capable of developing an efficiency as high as was observed on the author's engine, and this on full throttle and at maximum speed.

In the second series of tests a side-valve cylinder was employed, and it is stated on page 725 that by supercharging, the power of the engine was increased by 5 per cent and the petrol consumption lessened by a like amount. The author points out that any attempt made to increase the mean effective pressure by increasing the petrol flow resulted in severe detonation. This would appear to point to the conclusion that the above figures substantially represent the maximum possible improvement obtainable by supercharging in this particular engine. The normal side-valve cylinder suffers from the disability of low volumetric efficiency, pocketing and faulty turbulence, and it is highly probable that with the supercharger in operation the effect of these disabilities would be lessened. The improvement attributed to supercharging may therefore in some measure be due to inherent faults in the engine upon which the tests were carried out.

Comparison of the performance of side-valve and overhead valve engines indicates that the latter are capable of developing from 10 per cent to 40 per cent more power than the former on a 10 per cent lower petrol consumption. The possible improvement in power and efficiency obtainable by altering the valve disposi-

tion is therefore apparently much greater than was obtained by supercharging.

Tests carried out by me on the possibilities of supercharging showed that in a normal overhead valve engine adjusted with reference to maximum economy under full-throttle ground-density conditions, no improvement in efficiency was obtainable by direct supercharging.

The results given in the second Table on page 743 do not fairly represent the effect of supercharging *per se*, but include the effect of adding cooled exhaust gas to the charge as a diluent. As recognised by the author, this has a marked influence in checking detonation, and thus would permit of the use of much higher compression than is normally possible. In the normal overhead valve engine, brake mean effective pressures as high as 138 lb. per sq. in. at 1700 revs. per minute have been observed, and while this is lower than the figures given by the author, it is highly probable that the admission of a small quantity of cooled exhaust gas with the charge in a well-designed overhead valve engine would permit of the development of much higher mean pressures than that given above. The thermal efficiencies given in the second Table on page 743 of the paper are no better than those given in Table I.

As applied to aircraft, supercharging has a distinct function—the maintenance of power with diminishing density. As a means of improving the power and efficiency of a well-designed petrol engine on the ground, the value of supercharging is doubtful.

In reply to the written communication Mr. Ricardo wrote:—I find it very difficult to frame a reply to Mr. Ritchie's contribution, because I cannot find that it contains any consecutive argument, and it appears to consist solely of destructive criticism and doubts as to the accuracy of the figures. He opens his remarks by saying that I confine myself solely to ground-level conditions. If he will read my paper a little more carefully he will find that I stated very definitely that "the main object of all these experiments has been the application of the system to aircraft," and that I discussed at some length the objects aimed at in relation to high-altitude work.

He next points out that, owing to extreme pocketing, the efficiency of the stratified charge engine diminishes with increase of mean pressure. The gist of the whole paper was that with a

(Mr. Ricardo.)
stratified charge the efficiency increases with diminution of mean pressure, which is exactly the same thing. At high mean pressures, the efficiency obtained was within 3 or 4 per cent of the highest attainable with the compression ratio used, so that the so-called "pocketing" was clearly not having the destructive effect which Mr. Ritchie implies. In the next paragraph he attributes the gain in efficiency on light loads to carburettor characteristic. Surely Mr. Ritchie is aware that under no circumstances whatever can a throttle-controlled engine show an appreciably higher thermal efficiency on light loads than on full load; this is not only demonstrable on theoretical grounds, but is common practical knowledge to everyone who uses a throttle-controlled engine. It is, I think, almost needless for me to say that all proper precautions were taken to eliminate any errors due to carburettor characteristic. The particular tests shown in Figs. 8 and 9 were all taken without a carburettor at all, but with the fuel completely volatilised, while in the throttle-controlled tests, at every load and throttle opening the whole range of mixture strength was explored, and the most economical mixture found in every instance. When running on the stratified charge this, of course, was unnecessary, since the power output was then governed by the supply of fuel alone, full air being admitted to the cylinder at all times.

Referring to Fig. 9, Mr. Ritchie points out that efficiencies as low as 30·5 per cent, and as high as 37·0 per cent, were recorded with a mean mixture strength of 10 ft.-lb. per cu. in. When reading the paper I pointed out that when the mean mixture strength was reduced to 10 ft.-lb. per cu. in. the stratification was liable to become rather uncertain, and that this was most marked at about 16 ft.-lb. per cu. in., which was a critical point. I certainly found, however, no such variations as he refers to, nor on again examining my notes and figures can I find any justification for this statement in the test results from which Fig. 9 was compiled—the extreme variation in efficiency with a mixture strength of about 10 ft.-lb. per cu. in. over a large number of tests was from 35·8 per cent to 37·7 per cent. I cannot conceive how he arrived at the figure of 30·5 per cent unless it was from some accidental mark, due to a printing error, in his proof copy of the paper.

In the next paragraph, quoting my figures, he points out that the gain in efficiency over the relatively narrow range from 40 to 80 lb. per sq. in. is only from 32·5 per cent to 34·5 per cent, a

gain of 6 per cent; had the engine been throttle-governed there would, over the same range of load, have been a loss in efficiency of nearly 6 per cent, so that the relative gain even over this range is nearly 12 per cent. In any case, why take so narrow a range? In any efficient engine with a compression ratio of 5 to 1 the useful range of indicated mean pressure is generally from 15 to 135 lb. per sq. in., or just three times the range he takes. It would be as useful and almost as sensible to argue that in the case of a steam turbine it would not be worth while to run condensing, because the gain in efficiency with a vacuum ranging from 0 to 10 in. of mercury is relatively small. In this connection also I must protest most emphatically against his suggestion that the variation in efficiency as between the use of a stratified charge and throttle-control could come within the range of experimental error.

Mr. Ritchie next proceeds to quote the results of tests which he has carried out recently, and in which he claims to have obtained an indicated thermal efficiency of 38·3 per cent with a mean pressure of 116·6 lb. per sq. in. and a compression ratio of 5·1 to 1. I am afraid that several of Mr. Ritchie's experimental errors must have run amuck here, for the figure he quotes can only be described as fantastic. For a compression ratio of 5·1 to 1 the Air Cycle efficiency is approximately 48 per cent. With a mean pressure of 116·6 lb. per sq. in., the maximum flame temperature will be in the neighbourhood of 2400° C., and the ideal efficiency obtainable with the most economical mixture strength, assuming no loss of heat to the cylinder walls and 100 per cent diagram factor, will be 38·4 per cent. Mr. Ritchie would do well to read carefully a paper delivered by Messrs. Tizard and Pye before the British Association at Cardiff in August, 1920, in which the limits of thermal efficiency for an air/petrol mixture are discussed with great thoroughness.

To allege that he has himself obtained a thermal efficiency within one-tenth of 1 per cent of that theoretically obtainable from the cycle he is using is outrageous—in effect he asks us to believe that he has succeeded in suppressing all heat loss to the cylinder walls, and that with a flame temperature in his cylinder of well over 2000° C.! The days are long since gone by when the limits of thermal efficiency of a petrol engine are controlled only by the credulity of the observer.

In the next paragraph he suggests that supercharging merely had the effect of correcting the loss in the cylinder due to the use

(Mr. Ricardo.)
of side valves situated in a pocket; to this I can only reply that in the larger experimental engine built by Messrs. Peter Brotherhood the valves were not placed in a side pocket, and yet the same gain in power and efficiency was observed.

In the next paragraph he makes the specific statement that overhead valves give from 10 to 40 per cent more power and 10 per cent higher efficiency than side valves—this is arguing from the particular to the general with a vengeance.

He proceeds to state that in tests he has made he has found no gain in efficiency from "direct supercharging." By direct supercharging is usually meant the wholesale raising of the pressure of the cycle. If this is what he means, he might well have spared himself the trouble of carrying out the tests at all, for no one would suppose that direct supercharging could, by any stretch of imagination, raise the thermal efficiency of the cycle; why should it? Direct supercharging has been adopted for increasing the power output of an engine, but not even its most enthusiastic advocates have suggested that it could raise the efficiency.

Lastly, he refers to the fact that the presence of cooled exhaust products permits of the use of a higher compression—of course it does—but I have specifically stated that the results given on page 743 were obtained with a compression ratio of 5 to 1.

SCOTTISH CENTRE.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

SCOTTISH CENTRE.

The First Ordinary General Meeting of the Session of the Scottish Centre was held at the Royal Technical College, Glasgow, on Monday, 1st November, 1920, at 7.30 p.m., Mr. D. Keachie (Chairman of the Scottish Centre) being in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

Sir Henry Fowler, K.B.E., then delivered his Presidential Address.*

There were present 50 Members and visitors.

* The Presidential Address was originally delivered in London and is reproduced on page 6, *ante*.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

SCOTTISH CENTRE.

The Second Ordinary General Meeting of the Session was held in the Royal Technical College, Glasgow, on Monday, 20th December, 1920, at 7.30 p.m., Mr. D. Keachie being in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The result of the ballot for the election of the Scottish Committee for the Session 1921/22 was announced, the new Committee being:—

Mr. D. Keachie.
Mr. J. F. Henderson.
Dr. T. Blackwood Murray.
Mr. T. C. Pullinger.
Mr. W. J. Thomson.
Mr. G. H. Cutbush.
Mr. G. E. McCaw.
Mr. J. Richardson.
Mr. J. Watt.
Mr. J. D. Parkes.
Mr. R. J. Smith.

The following paper was then read and discussed:—

"Roads and Vehicle Maintenance," by
Col. R. E. Crompton, C.B.*

There were present 56 Members and visitors.

* This paper was originally read in London and is reproduced on page 167, *ante*.

THE DISCUSSION.

Mr. D. Drummond, in opening the discussion, said: This is the first time that, as a professional man, I have come face to face with men who are as desirous of improving the roads as the Road Authorities are. This Institution, I take it, is anxious to build vehicles which will do the least possible damage to the roads. Now, it is a fact that when the automobile industry started, the roads were not fitted in the slightest degree for the traffic which came upon them. I think it is to the credit of the road authorities of this country that they did not lose faith, but took their courage in both hands and met what appeared to be an unsurmountable difficulty, unsurmountable in that we had no knowledge to go upon and we had to feel our way. If it had been a private concern, motor vehicles would not have been allowed out until roads were constructed to carry them. But the owners of the vehicles were never asked to stay their hands, and the road authorities went on, and in the main have been very successful in carrying the traffic. In the vicinity of Glasgow on the main roads the mechanical traffic is to-day about 91 per cent of the whole measured by numbers. I cannot give the percentage in tonnage. Yet in 1912, only eight years ago, there were no mechanical commercial vehicles passing between Paisley and Glasgow. As a consequence of this rapid progress, the roads running south into Ayrshire went all to pieces. We are now at the stage when it might be said that the whole traffic is going to be carried by mechanically-propelled vehicles, and the horse will only be used for short distances, for the farmer passing from his farm to the station, or from one field to another, or for similar short distance work. At a distance of eight miles from Glasgow there is not a single through wagon of any kind hauled by a horse—it is wholly mechanically-propelled traffic. I believe the road engineer has sufficient knowledge, if he gets the money, to give a road which will carry all the traffic. The author has epitomised a great deal of what he has heard at many conferences during the past twelve years. I hope we shall hear more to-night about the mechanical action of vehicles upon the road surface. Nine years ago, I was very much of the opinion that the vehicle was built for the convenience of the man who

drove it, and the men who were going to load and unload it. That is to say, the wheels were low and of such a diameter that they became a sort of knocking machine on the roads and knocked the roads to pieces. In France some of the wheels are higher than a man and this makes all the difference in the world, and it needs no mathematics to demonstrate it. If a steam engine drawing a heavily loaded trailer comes to a bridge which will not clear the load with ordinary wheels, they jack up the trailer and put on small wheels, taking them off again after they get through the bridge. The low wheel is inclined to push and plough up the road surface, and I have repaired the roads repeatedly on account of that very thing. Undoubtedly any increase in wheel diameter at once very largely solves the difficulty of giving a surface without risk of this deformation. I think deformation depends upon two things, a wheel of sufficient diameter and proper materials laid upon the road surface. Nine years ago, when the subject was being discussed with a view to legislation, the width of the tyre was increased but the diameter was left the same. This has the result that owing to the camber of the road the bearing is wholly on the inner 9 in. or 10 in. of the tyre, the outer edge of the wheel rarely carries weight. There is only a certain definite width that can be effective on a cambered road. After that the diameter must be increased, and that is really what has troubled the road engineer more than anything. I am happy to see the steel shod wheel gradually disappearing, and I think we shall go a stage further and have pneumatic shod wheels which do not do the same damage as the solid rubber tyre. Another point is that if I were a contractor I would not have such heavy vehicles; I would go three journeys instead of two and carry a lighter load. We are all too anxious to load up a huge machine, yet our roads would be spared and probably we should accomplish as much at the end of a week and earn the same amount of money by using the lighter vehicle.

Mr. G. E. McCaw: The author has put forward the theory, apparently substantiated by observations on his part, of wave motion as applied to road surfaces. This is a popular theory at the present time regarding all phenomena, but I must say I cannot see it as applied to roads. I have not studied intensively the action of a wheel rolling on a hard surface, but watching the action caused by an ordinary motor vehicle going through a pool of water or through mud, we certainly get a wave motion, but not

in the longitudinal direction indicated by the author. The action is more like the wave from the bow of a barge. These waves can be measured either in an experimental tank, or in smooth water, but when two boats are passing one another, the bow waves merge and give a very different effect, an immeasurable effect, and I maintain that in roads there is a wave action on solid, or semi-solid, material which does not come back to its ordinary level. One vehicle may raise a wave action which is disturbed by the vehicle which follows or passes in the opposite direction. I have examined ordinary main arterial roads near where I reside, both with tarred surfaces and ordinary macadam, and I cannot find any series of regular waves, or regular hollows in the road. Every hollow seems to vary in length from the preceding one, and my impression is that it is more from the effect of bottoming which continues to sink in places, giving hollows which ultimately, if not attended to, become pot-holes.

The author favours the kerbing or filling in of side ditches to prevent the road spreading in a transverse direction. This can, however, only be successful if the kerbing is backed by solid material. Kerbing a road over a moor or soft sand, for example, can never be permanently effective. We have a concrete example in the case of the West Highland Railway, where great difficulty was experienced in constructing the line across Rannoch Moor. No firm bottoming could be got until the ingenious device was adopted of laying on the soft peat a thick layer of brushwood, upon which the permanent way was built, making one of the most satisfactory stretches on the line.

We have also in Scotland the example of the very common dry stone dyke, where the whole success depends on the interlocking of one stone relative to the other, and I feel that in considering roads we have got to go deeper than the surface to see that we have a sufficient and properly interlocked bottoming. It may be hopelessly expensive to take up and replace the bottoming of present roads, but this must be faced in all new arterial roadways. Scarifying the surface of the roads to knit the new surface to the old road so as to prevent it being spun out by the action of vehicles does not appear to be carried out so extensively as was done in the past, but this I maintain is essential to give a decent life to any new surface made.

I would submit to road engineers the possibility, when laying a new surface to the present roads, of using some form of expanded

metal, as we do with reinforced concrete. In such cases the largest stones would be removed after scarifying, then the expanded metal laid, and on the top of that the new surface, all bound with some bituminous material. This would give a road bound near the surface, and would prevent either lateral or transverse spreading much more effectively than either side or central kerbing.

Mr. J. Watt: In his Presidential Address to the Institution of Engineers and Shipbuilders in 1919, Dr. Blackwood Murray pointed out that in the case of a vehicle travelling over a rough road, of the total stress coming upon the axle body only about 8 per cent arises from the direct load imposed by the springs, the remainder being wholly due to the inertia effect of the axle bumping over the uneven surface. It is therefore very evident that if we could get really smooth roads to run on, the design could be simplified very considerably, and thus cheaper and lighter vehicles could be built.

The author has pointed out how the extension of the surface can be prevented by properly packing and supporting the foundation of a road. It has occurred to me that the tractive effort exercised by the wheel on the surface of the road may also have some effect in the formation of pot-holes. Take the case of a fairly heavy char-a-banc travelling up an incline of, say, 1 in 25 or 1 in 30. There is probably a propelling force of about 800 lb. as an average which must be resisted in a horizontal direction by the upper layers of the road surface. As the author points out, the wheel is progressing by a series of bounces, and actually, as the vehicle travels, the propelling force may vary from zero up to probably three or four times the average, depending wholly upon the proportion of the pump period during which the tyre is out of contact with the road. Looking into the matter roughly with sketches, I have come to the conclusion that if the tyre just leaves the road and no more, the propelling force is increased by about 40 per cent. If it leaves the road for a quarter of the distance traversed during the bump cycle the force is doubled, and if for half the distance four times the average propelling force is necessary. Apart from the vertical force, there may thus be a periodic horizontal force of anything up to 200 or 250 lb. over each inch of tyre width due simply to propulsion, and unless the upper layers of the road surface are sufficiently strong to resist this tension, I can imagine that they might rupture and thus give rise to pot-holes later on.

Apart altogether from the question of road surface stress, this also means that much higher factors of safety have to be allowed in the vehicle transmission than would be necessary if the tyre were in contact with the road all the time. Much useful information could be obtained in this direction by taking simple pictures of the ultra-rapid type, which would show the relative movements of the wheels, springs and frame. If once we could get accurate data to go upon, we could tackle the problem much more satisfactorily. Springing at all times is a very difficult problem, and one perhaps usually tackled from the point of view that roughly a 4 in. deflection is required on the rear spring to give comfortable riding, and probably $2\frac{1}{2}$ in. or 3 in. on the front spring.

Mr. Hy. Elvidge: On page 173 the author advocates the repair of roads by filling up the holes to the old level and putting on a fresh coating one stone thick. Is that method in constant use now, or is it a theory put forward by the author for future use? In regard to the suggestion of using two small lorries in place of one big one, two points struck me. One was the question of the first cost of the two machines with reference to their running costs. I do not think the running cost of two 30-cwt. lorries will be equal to the running cost of a 3-tonner. That, of course, raises the question of relative speeds. Mr. Drummond advocated making three journeys instead of two, or, in other words, six instead of four. I think eight journeys with 30-cwt. lorries instead of four with a 3-tonner would be a cheaper method in the end. The idea of running a car over the road in the dark with lamps on the car at different points and taking photographic records did not measure the undulations of the road. The undulations of the road were known beforehand; it measured the relative positions of the different parts of the car, and indicated the efficiency of the front and rear springs respectively. I would like to ask whether submerged kerbs in herring bone form, the apex of the herring bone lying in the direction of the oncoming traffic, have ever been tried, and can the author give us any information, practical or theoretical, as to the behaviour of such herring bone kerbs? I would also like to support Mr. McCaw's suggestion to use expanded metal as a road binder.

Mr. W. Daniels: In regard to the wave formation on roads, may I point out that even in the case of railroads and tramway rails, where there is a considerable resistance to deformation, we

get what is known as corrugation. I would like to ask whether the action producing this corrugation on steel rails is of the same nature as that which produces it on the roads, and if so, it seems to me that we are tackling an exceedingly big problem in trying to eliminate wave formation on ordinary roads, even if we reinforce them with expanding metal as suggested by Mr. McCaw.

The CHAIRMAN: As this is the last official meeting at which Mr. McCaw will act as Hon. Secretary, I feel that we cannot pass to the next business without reference to his services to this Institution. Mr. McCaw has been Secretary since the formation of the Scottish Centre. It was largely due to his efforts that the Centre was organised and kept together. The Council have on many occasions felt the value of his work, and it has always been a pleasure to work in office with him. His courteous manner and willingness to do everything for the Institution has gained him many friends, and it is with regret that we have to record his resignation. The mantle of office has luckily fallen on Mr. Cutbush, who, I am sure, will make a worthy successor.

I would ask you, gentlemen, to accord to Mr. McCaw a very hearty vote of thanks for the work he has done for the Institution.

I am sure we all feel indebted to the author for coming North to deliver what is such a valuable addition to the science of roadmaking.

To an Institution such as this, the need for good roads and their proper upkeep is of paramount importance, and roads outside of towns and cities are receiving, or should be receiving, more attention than ever before, owing to the extension of motor traffic, and to the economy of this method of transportation. Given the support of urban and suburban authorities, and a reasonable amount of expenditure to keep the roads in good condition, there is no reason why motor transport should not take the place of railway transport for at least medium and short journeys.

The author has very clearly pointed out to us the value of smooth surfaces, and of continuous kerbing at the sides of the road to prevent waving, but on wide roads we are still troubled with the corrugations which go so far to shorten the life of commercial vehicles. He makes out that the sides of the roads are much more worn than the centre, but we must remember that motor vehicles use the sides of the road and often leave the centre untouched. I would like him to explain how these kerbs are set in without reinforcing them at the back. My own opinion

is that they will slip away unless there is proper reinforcement at the back of the kerb to sustain the pressure.

Without presuming technical knowledge on roadmaking, I think a good deal more could be done by the application of glass, which has been suggested in some quarters. I am also very much surprised that nothing has been said about concrete or reinforced concrete roads. In America they seem to have solved this problem of concrete and reinforced concrete, notwithstanding the fact that the author is rather sceptical of what has been said and done there. The most successful road in America is laid with 9 ft. concrete slabs, covered with a coating of $\frac{3}{8}$ in. tar and metal, which is renewed from time to time as it wears off. I would like the author to give us some idea of any progress that has been made in the art of making such roads.

It is surprising that some form of steel plate about 18 in. or 20 in. wide has not been tried on roads to form tracks, similar to those used for horse haulage on inclines in cities. Such a line should not be difficult to construct. Its economy is obvious, and the tractive effort would be smaller on a motor vehicle under those conditions.

As showing the advantage of smooth roads, the following figures are taken from a test to show the pull in pounds per ton required to move a load over the roadways specified:—concrete surface, 27·6; concrete base with bitumastic top dressing, 49·2; water-bound macadam, 64·3; gravel, 77·1. Those tests were taken on level roads in good condition. The petrol consumption varied proportionally from 11 miles per gallon on concrete to 9·3 miles per gallon on gravel. Those figures point to the advantage and economy of smooth roads, and we know from experience they add to the life of the vehicle. I agree with the suggestion of the author that machinery should, wherever possible, be introduced in roadmaking and repairing to reduce costs. In repairing roads, the tarring and chipping, especially in populous districts, is usually a source of annoyance and discomfort owing to the difficulty of keeping vehicles and pedestrians off the road between the operations of tarring and chipping. The Kilmarnock district in Scotland have got over this difficulty by having a tipping wagon filled with chips attached to a tractor which hauls a 320 gallon tar tank. A flexible pipe is led from the tank to the rear of the tipping wagon. A "V" shaped trough is placed under the back door of the wagon, which can be regulated for various sizes of

chips. This trough is provided with a spiked shaft driven off the rear wheel to prevent choking. By this means the chips are spread uniformly immediately after the road is tarred, at a speed of four miles per hour, and from 700 to 800 superficial yards can be spread with tar and chips in one operation.

This is an instance where a tipping wagon is of invaluable service to municipal authorities, and the chips are spread more uniformly than can possibly be done by hand, besides being obviously much more economical.

Mr. DRUMMOND: In reference to the use of reinforced concrete, I might say that we have a scheme of what is known as "wheel-ways" on reinforced concrete. It is the invention of Mr. James Chapman, of Forfar. A year ago he went to the Road Board and then he came to me, and we made a bargain to try his experiment, but on account of the passing of the Road Board to the Ministry of Transport the experiment was not made in the summer of 1920 as suggested. But the whole machinery is ready, and the Renfrewshire authorities hope that next summer they will be able to give this scheme a trial. If the suggested wheel-ways should prove a success, they might be more cheaply installed than reinforcing the whole roadway. They are 21 in. broad, and a motor car or motor lorry will run on these wheel-ways until another vehicle comes along, when they give passing room and come on to the wheel-way again. Where they were passing through a busy centre two lines would be necessary, but in the country only one. That is one of the solutions in the air, and we must wait and see what it comes to.

Colonel CROMPTON, in replying on the discussion, said: As Mr. Drummond is to some extent joint author of my paper, as regards the road portion, his remarks may be taken rather as amplifying than criticising what I have said, but when he tells us of the introduction by Mr. James Chapman, of Forfar, of reinforced concrete wheel-ways, he introduces new and interesting matter, but personally I doubt whether any system of wheel-ways which differentiate the surface of the road will have wide application. I think we must all agree that it is better to make the entire width of the road as homogeneous as possible, otherwise we shall have irregular use of the surface and irregular wear always follows on irregular use.

Mr. Watt quoted Dr. Blackwood Murray's Address to the Institution of Engineers and Shipbuilders in 1919, wherein he

points out that of the total stress coming upon the axle body only about 8 per cent arises from the direct load, the remainder being due to the inertia effect of the axle bumping over the uneven road surface. Dr. Murray herein really emphasises all that I have said as to the importance of the impact forces which we wish to study further so that we may deal with them. Mr. Watt, however, falls into the very prevalent error of over-estimating the effect of the tractive forces exerted tangentially to the road surface when a self-propelled vehicle is mounting up an incline. Road surveyors, because they have seen a wheel occasionally slipping, imagine that the forces which cause that wheel to slip are very great, whereas to the best of my belief these tangential forces which tend to move the road material *backwards* are not nearly so important and are not so great as those rolling forces which push the same material *forward*. Of course, it is a question of the grade of incline; on very steep inclines the tangential tractive force may be enough to do damage, but here again the damage is of another class, and although it may cause the sheering away of a portion of the upper surface of the road, it does not deform it to nearly the same extent as the rolling forces do.

I cannot follow Mr. McCaw in his idea that wave formation can be studied by watching a wheel roll through a pool of water. He thinks that kerbing a road over a moor or soft sand can never be permanently effective, and he gives an example of the West Highland Railway. Apart from it being obvious that his illustration does not apply to roads, the answer is that Mr. Drummond has shown us that it is just on these soft moorland roads that kerbing has been most effective and has produced most satisfactory results. Of course the reason is evident—without kerbs the wheel travelling near the edge of the unsupported haunch can push out the stones one by one, and the restraining power of these is small, but when the kerb is used, even when there is only soft material at the back of the kerb, the wheel cannot push out the stones one by one, but the thrust is resisted by the kerb itself, which, being of considerable length, has a greatly increased power of resistance. I agree, however, with Mr. McCaw when he says that the interlocking that we require and the resistance to such movement extends to a considerable distance below the upper surface. He recommends the use of expanded metal or reinforcement; this is, of course, sound engineering but expensive.

In reply to Mr. Elvidge, I would say that my conclusion that

it is better practice to repair the old surface to a true contour before putting a new coating of even thickness over the entire width is based on past experience as well as on theory. It is abundantly evident that the tendency for the traffic to form waves is increased when the new layer is several stones thick, whereas when it is one stone thick only it can be resisted by efficient wedging at the time of rolling; here practice and theory support one another. He also raised an interesting question involving the carrying of loads by a greater number of lighter vehicles. I think he will find that this is a commercial question depending very largely on the relation of the wages paid to the driver and his assistant to the total load carried. From the wages point of view, the larger the loads the lower will be the nett cost per ton carried. Again, he points out that the taking of photographic records by cinematograph really does not measure the undulations of the road, but only measures the efficiency of the front and rear springs. To this I agree, but it is exactly the information that we require to enable us to study the impact effect of different classes of vehicles on different classes of roads already waved or deformed.

I have to thank Mr. Daniels, who corroborates all that I have said, to the effect that the corrugations observed on rails and tramway surfaces obey the same laws and probably are due to the same causes as the waves on the roads.

The Chairman points out that in America they have made very perfect roads by using reinforced concrete, but have found it necessary to cover these with an elastic coating of bituminous material which is renewable. I agree that with concrete roads this is the correct way to keep them in repair to protect them from cracking from impact forces, but in the United Kingdom I say that in the majority of cases our country road crusts are good enough to act as the concrete on which all elastic renewable cover can be laid. The figures that he gives as the tractive forces required on roads are corroborative of figures prepared early in the last century by General Morin in France, which experiments undoubtedly caused the French Government to commence the great road improvements in that country which resulted in the "Grandes Routes Nationales" which, up to the coming of automobiles, were the pride of France, but which failed after a few years when subjected to high speed automobile traffic conditions. The Chairman also referred to the very excellent ex-

periments that have been carried out in Scotland with a tipping wagon filled with chips and fitted with a distributing means by which an even layer of chips can be pushed out on to the road. I can see that this invention is of extreme importance and is likely to be developed into one of the best methods of using machinery for resurfacing all kinds of secondary roads.

In concluding the discussion I have mainly to point out that road improvement is chiefly a question of the money available. It has been said, I think, by Lord Montague and other authorities, that the annual sum that will be required during the next few years will be about 32 millions for the United Kingdom, of which the larger half will be required for the secondary roads. I have had this figure in my mind, and have considered how far the burden on the shoulders of the long-suffering ratepayer can be lightened by the scientific treatment of the roads themselves, and by similar scientific consideration of the action on these roads of high-speed modern vehicle traffic. I have been many years at work on this question; when a young officer, I was employed by the Indian Government to run road trains on the splendid Grand Trunk road of India, and I did much of my preliminary experimental work for these trains in Edinburgh and Glasgow. It was due to a Scotchman, Mr. R. W. Thomson, that the great experiment was tried and was followed by his splendid invention of rubber tyres. At that time I considered what Mr. Drummond has said as to the advantages of wheels of large diameter, and I was extravagant enough, supported by Mr. Thomson, to have my engines built with wheels over 6 ft. diameter, provided with huge rubber tyres 16 in. wide by $4\frac{3}{4}$ in. thick. The Indian Government constituted me the road authority over the roads on which I had to work with my trains, and I think I was then in the peculiar position of being, like the railway authority, responsible both for the roadway and the vehicles. I was then able to take trains carrying 40 tons of paying load; this was done fifty years ago, and nothing on so large a scale has yet been done in this country although possibly it may be done in Scotland in a year or two.

I have already dealt with the commercial question, showing that the use of two vehicles instead of one is largely a question of wages; every item of labour cost has now to be most carefully examined. Personally, from the road point of view, I should like to see traffic carried by an increased number of smaller vehicles. One method of doing this is by developing the road train ques-

tion; I considered this matter several years ago when I was engineer to the Renard Steam Train Co.; I then pointed out that with six vehicles, each provided with six wheels, all of them drivers, a total of thirty-six wheels in the train, there would result a train the gross weight of which would be 144 tons, although each wheel only carried four tons, and that if the motive power was electric, obtained from a trolley wire or an electrical conductor at the side of the road, the weight of the motive power would form such a small part of the gross weight of the train that upwards of 80 and possibly 100 tons of paying load could be carried in such a train, employing only two men, a driver and a guard, in charge, and thus wages would be reduced to a minimum per ton of freight carried. I still think that if road locomotion is to be developed to the utmost, this is the line that engineers will have to follow. At that time I also considered the question of wireless transmission of electricity, so as to do away with the trolley pole and to take the power by induction from wires fixed near the side of the road, but after a great deal of thought and correspondence with the greatest electrical authorities in the world, I had reluctantly to abandon the idea as impossible.

I understand that the reason why the police in London have refused to sanction the use of the giant pneumatic tyres in the London streets is that the bursting of such large tyres and the high air pressure carried in them would be such a serious matter, alarming to the public and quite possibly causing the vehicle to overturn.

In conclusion, the following references may be useful to those who desire to know what has already been brought forward by the author and others on the matters raised in this paper and discussion:—

Proc. Inst. C. E. Vol. CCVII., pages 123—221.

Proc. I. Mech. E. 1913, Parts III. and IV., pages 1253—1328.

Proc. I. A. E. Vol. VII., pages 403—485.

THE INSTITUTION OF AUTOMOBILE ENGINEERS.

SCOTTISH CENTRE.

The Third Ordinary General Meeting of the Session was held at the Royal Technical College, Glasgow, on Monday, January 17th, 1921, at 7.30 p.m., Mr. D. Keachie being in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"Suggested Lines of Development in Transmission,"*

by Capt. S. Bramley-Moore.

There were present 52 Members and visitors.

* This paper was originally read in London, and is reproduced on page 261, *ante*.

THE DISCUSSION.

Mr N. O. Fulton, in opening the discussion, said: One of the difficulties in connection with the use of cone clutches arose from the fact that leather is a very variable quantity, with the result that on the same make of vehicle some clutches were fierce, while others were quite nice to operate. I still think that that is a difficulty with this form of clutch, and I fear that there will still be a danger of variation with other fabrics now being used. The cone clutch will not give an easier engagement than the plate clutch; therefore, it becomes a question of cost, which is seriously affected by the provision of two cones to get a nice easy engagement. In some of the designs showing a double clutch the cone that finally takes the drive has a small diameter with a very slow taper, and I think that there is a risk that this will stick. In Fig. 4 the dash-pot arrangement to prevent too rapid engagement, would prove a very serious drawback when attempting to jump the car out of a hole or ditch—in fact, it would render it impossible to do so.

Mr. F. Henderson: I have tried the double springs suggested by the author to allow the pressure to rise and the clutch to engage, gradually, but could not get satisfactory results. It seems that if the clutch is fierce, there is a certain definite pressure at which it takes up and it does not matter whether the pressure is put on suddenly or gradually. The author states that the pressure on a cone clutch is very much less than that on a plate clutch. This is quite true, but the cone clutch has to be moved considerably further in order to get it clear. The pressure on any kind of clutch depends entirely on the amount of movement, and the amount of pressure that can comfortably be exerted, on the clutch-pedal. If, therefore, the cone clutch has to be moved further to get it to disengage, it means that the pressure on it must be much less for the same pressure on the pedal. The author also stated, in connection with the wear on a disk clutch, that the portion which was running fastest will wear most. I contend, however, that when that portion wears a little, the pressure on it will be reduced, and, therefore, the part which is running slowest will wear more, and so on, so that the clutch actually wears evenly all over.

Mr. W. L. Spence: I find myself in complete disagreement with

(Mr. W. L. Spence.)
the author on the subject of cone clutches, since he proposes greatly to complicate a simple and effective piece of mechanism for an infinitesimal, if not for a purely imaginary, advantage. Sound engineering practice is to simplify and simplify again, short only of the point where efficiency is endangered. I also disagree with him in regard to universal joint action; the Oldham coupling cannot be modified as proposed to run successfully at high speed: it would set up intolerable vibration.

For more than a quarter of a century, actually since 1894, I have been a worm gear advocate, and I have at the present time over 120 sets of duplex worm gears running underground in collieries, driving coal-face conveyors under the most adverse conditions, but still with complete success, and as I have personally designed all this worm gearing I claim to speak with some little knowledge of the subject. That being so, I cannot but be amazed at the author's flippant and inadequate treatment, as set forth on pages 290 and 291 of the paper. On these lines, truly, in the author's own words, "The proposition is really very simple if attacked in a systematic manner."

In actual fact, however, worm gear is most worthy of study, from the Geometry (Halsey, of the American Machinist, is good) to Bruce (Proc. Inst. Mech. E., January, 1906), which paper is a classic. Bruce was the first to publish an analysis of the rating of worm gears; he showed that the duty depended on the pressure-sustaining area of contact between the worm and the wheel, and that this varied as the diameter of the worm and as the square root of the diameter of the wheel. Bruce's basis, but with constants modified on experience, has been used throughout my own work with uniform success. After Bruce, there are Lanchester's invaluable papers before this Institution.* I regret to have to say that, in my opinion, there is nothing of the least value in the author's contribution on worm gear either from the design, constructional or application points of view. While thus out of sympathy with the F.J. gear, it is interesting to recall that the duplex thrustless worm gear was, on the large scale at any rate, the production of Frank J. Sprague, of America. The duplex equalised worm gear is not nearly so well known as it should be. It is particularly applicable for large reductions of, say, ten to one or more for lorries and on other vehicles for reducing centre

* See Proc. I.A.E., Vol. VII., p. 115, and Vol. XI., p. 79.

distances and increasing ground clearances. It does away entirely with ball thrust bearings and eliminates all fine and costly adjustments.

Mr. J. W. Mills: I rather gather that the clutch and gear-box designs put forward by the author are more of academic interest than of practical value. I think Mr. Fulton hit the right nail on the head when he said that the biggest factor in the success of the cone clutch is the condition of the covering leather or other fabric. My own experience has been principally with leather, and the results have been very doubtful. It is impossible to get two samples really alike, and even the same sample once it has been used varies. I think the author's intention was that by utilising the multiple cone clutch the gear-box difficulty could be minimised. Personally, I prefer to put up with a few more gear wheels and a bigger gear-box and to eliminate the possibility of clutch trouble. I think that the general trend of the industry to avoid epicyclic gears is due to the difficulty of lubrication and the multiplication of parts. I quite agree that the design of universal joint shown would fulfil the purpose set out, but I do not agree that in practice it is a good thing. With a joint of the author's type, in which the shafts may be out of line longitudinally and angularly, the movements of the main parts of the joint must be greater than they are in the case of the parts where two joints are employed. I think the universal joint is one of the parts which gives most trouble through wear, and before adopting the author's joint, which seems to me to be liable to greater wear, I think it wants very serious consideration of its other advantages. Coming to the subject of worm gears, there is no doubt at all about the quality of the F.J. gears, but I was sorry to see that the Lanchester Globoid worm was ignored. I am quite prepared to admit, apart from mathematical argument and purely from the wear point of view, that the F.J. thread is superior to the old $14\frac{1}{2}$ degree rack-thread, but I am not prepared to say it is always the case with all rack-threads. If we confine ourselves to the angles of teeth previously in use, then my experience is that the F.J. has better wearing qualities. I should like to have heard something put forward in favour of the Globoid as against the F.J. thread.

Mr. J. Richardson: In connection with the universal coupling put forward by the author as being truly universal in action, we have in marine practice at the present time certain problems which seem to call for a similar solution, and, in some cases, a coupling

(Mr. J. Richardson.)
known as the "Fairfield-Robinson" has been adopted. Has the author considered this type of coupling? Has it been used for automobile work, and what have the results been?

I would also like to ask whether double-helical gearing has actually been tried out in practice in automobile gear-boxes, as it seems to me that for such work it is a refinement that is hardly justified.

I think that, as Mr. Mills said, the double cone design of clutch has been put forward by the author more to make possible amended designs of change-speed mechanisms, than from any desire to improve the performance of the clutch itself.

Mr. J. Watt: Geometrical analysis shows, I think, that the worm with a tooth of hyperbolic form in the plane of the axis has certain advantages over the ordinary rack or straight flanked profile. Nevertheless, before deciding to adopt a worm of any sort for any particular axle, the designer must consider whether this type of gear is fundamentally right to meet the case. In order to obtain high mechanical efficiency under certain specified conditions as regards velocity-ratio and overall dimensions, there is a tendency on the part of worm designers to curtail the root diameter of the worm to such a degree that, running in first speed, the worm as a shaft is found to deflect to an undesirable extent. The effect of such deflection is disastrous as it immediately sets up a lateral displacement of the balls in the bearing races, which puts excessive loads on them, with ensuing rapid wear and slackness. Another point is that a perfectly rigid carrying member is essential in any axle, and with certain designs it is impossible with a worm to place the beam with its greatest dimension vertically and at the same time retain reasonable ground clearance.

The solution of the problem in many cases may lie in the adoption of a bevel drive, and under these circumstances I would be glad to have the author's opinion on spiral bevel gears, which for certain duties appear to offer some advantage. Of course, an objection immediately advanced against the spiral bevel is that there is considerable axial thrust on the pinion, but it must be remembered that there is probably a greater thrust along the corresponding worm shaft. As regards velocity-ratio, I understand that in America there have been run for purely experimental purposes, not actually in practice, spiral bevel gears with as few as four, or even three, teeth in the pinion, and I believe that nine or ten teeth is accepted as quite a reasonable figure. Thus it is

possible to obtain a velocity-ratio of anything up to six or seven to one, which is all that is required in most cases.

I would further like to ask the author if he could give any hints in connection with the calculation of the strength of spiral teeth. The Gleason Company has given me the following formula, regarding which he might care to make some remarks:—

$$S = \frac{6\ WLC}{AT^2\ (C - 1/2A)}.$$

where

S = Stress in lb. per sq. in.
W = P. Cos pressure angle, Fig. 70.
P = Load in lb. on point of tooth.
L = Effective height of tooth as cantilever.
C = Cone distance in inches.
A = Width of face in inches.
T = Effective thickness of tooth in inches.

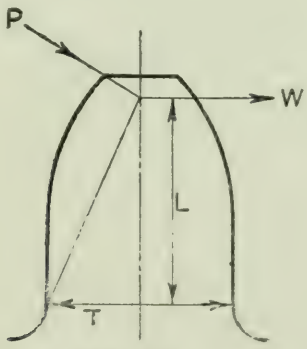


Fig. 70.

Would the author also give some idea of the pressure angle of the F.J. thread, which would appear to be greater than that of most other worms, and, that being the case, the radial load on the worm shaft would be greater, tending to increase the normal load on the ball bearings, to say nothing of the abnormal load imposed on the balls if the shaft deflects excessively in use.

Mr. J. D. Parkes: In my opinion, the oil dash-pot, shown in Fig. 4, would be an immense nuisance if quick gear-changing on hills was required, as it would probably cause the car to lose a lot of way and possibly the driver might miss the gear altogether. The author states that the wear on the friction fabrics of an ordinary plate clutch is uneven owing to the different rubbing speeds. This is not in accordance with general experience, as the

(Mr. J. D. Parkes.)
friction fabrics wear uniformly over their face width, and I do not think that the variation in rubbing speed between the inner and the outer radius is sufficient to cause any more rapid wear than with a cone clutch.

There are two important subjects not mentioned in the paper which might well have been included: one is the question of hydraulic clutches and gear-boxes, and the other that of electric transmission—both of which offer very interesting fields for investigation. I had the opportunity recently of trying a new hydraulic clutch on the road on a heavy car, fully loaded, and found it a revelation as regards sweetness and general comfort. It was further possible to slip this clutch almost indefinitely without seriously overheating the oil.

The author makes a great point of the constant angular velocity of the worm wheel, which he claims is one of the main advantages of the F.J. worm. I would be glad to know what is the variation in angular velocity of the worm wheel, obtained with other types of worm gear. If this variation is small, the F.J. worm can claim no very great advantage, because, with the normal arrangement of cardan shaft, with a universal joint at either end, even if the engine is running at constant speed, there is a calculable variation in angular velocity of either the worm or the bevel pinion. It is only in certain definite cases and conditions of loading that a constant angular velocity is transmitted. My point is, that if ordinary worm gears give approximately accurate results, a refinement such as is claimed for the F.J. worm in this question of constant velocity is of little practical importance.

I would like the author to give us his opinion on the use of worm gears for heavy commercial vehicles, where extremely large torques have to be transmitted through the back-axle, and where, therefore, the pressure between the worm and the wheel is very high. Is it his experience that in these cases there is a danger of oil being squeezed out from between the surfaces, with resultant scoring of the worm and wheel?

The Chairman: I do not quite agree with the author's statements regarding clutches. The ordinary cone clutch does not usually lend itself to softness, specially in starting up a car, and his double cone clutch, while perhaps sweeter in action, will be more expensive to manufacture, and certainly will cost more to keep in repair. Where, as in Fig. 4, he introduces an automatic device for giving more perfect manipulation of the clutch, we are

again faced with a more expensive proposition with extra moving parts. No doubt a lot of trouble has been experienced with plate clutches running in an oil bath, and here a real problem is evident, in the difficulty of getting oil that will retain the same consistency under all conditions of temperature. The most satisfactory clutch to my mind is the single plate dry disk clutch, which is fundamentally better than a cone clutch because of its lower rotating momentum. With proper design a clutch of this type can be made to work smoothly and satisfactorily, and will be found to act more consistently at all temperatures and can be manufactured as economically as any other type. There is no oil required, and it does not need to be enclosed.

With regard to the gear-boxes shown in Figs. 10 and 11, while we have a train of wheels always in mesh, with the exception, of course, of the reverse wheels, the author has not been able to get away from clutches, and shows three internal clutches on the main shaft and one on the counter-shaft, with an ordinary clash gear for the reverse. Internal clutches are probably as harmful to the life of a gear-box in a careless person's hands as any other, and examination of many vehicles returned for overhaul, or from the war, has proved that given a well designed box, properly machined, with the gears hardened accurately to specification, very little damage in many cases could be detected in the gears. With regard to war vehicles, taking into consideration the number of drivers who were required on short notice, and the doubtful adaptability of many of those to the requirements, it is surprising that any mechanical device stood up at all under such abnormal conditions. My own opinion is that for a clean, straightforward job, simple to manufacture and able to stand up to any amount of abuse, there is nothing better for any pleasure or commercial vehicle than a gear-box of the constant mesh type, in which the various speeds are engaged by means of dog clutches.

While on the subject of gearing, will the author give us his opinion of $14\frac{1}{2}$ degree gear teeth as against 20 degree stub teeth? Some months ago, at a conference of gear cutters in America, an interesting point was brought out in discussion, namely, that while in 1919 most American manufacturers were turning over to the stub tooth, in the year 1920 an expression of opinion from all users of gears in America showed 87 per cent in favour of the $14\frac{1}{2}$ degree tooth. Does the author consider any factors, such as wear and noise, are more common to the one than the other?

(Mr. D. Keachie.)

In Figs. 47 to 50 the author gives us diagrams of zones of contact in the ordinary and in the F.J. worm and wheel. He does not mention the Lanchester type of worm, and I would be glad if he could give us any points relating to this type.

Capt. Bramley-Moore, in replying on the discussion, said: I ought to mention that one of the objects of this paper was to promote discussion, and I have deliberately made certain dogmatic statements to provoke controversy. Mr. Fulton spoke about leather being a variable and unreliable quantity, and other speakers asked what I had done in that direction. I once spent a great deal of time experimenting with the leather cone clutch. I sometimes used very weak springs, and perhaps one day I would get such excellent results that I could almost bang the clutch in and still get a smooth start. On taking the car out a few days later, however, I could not get the same result at all. I put it down at the time to the leather being unsuitable. I don't think that leather, as far as I have experimented with it, is to be relied upon. I think it better to use metal-to-metal clutches. I must say that my experiments with leather clutches were unsatisfactory because the results were indefinite. All I proved one day was lost the next.

Mr. Henderson referred to the wear on the disk clutch, and considers that there is even wear all over. I suggest that if the velocity were equal all over, the wear would be equal all over, but because the rubbing speed varies the rate of wear will fluctuate.

Mr. Mills thinks that the gear-box problem was largely responsible for the introduction of the double-cone clutches shown in this paper. This supposition is quite correct. I may remark that the variations in different gear-boxes are very interesting, some changing with great facility whilst others are extremely bad. I am not going to mention the names, but there are one or two makes of gear-box which present considerable difficulty when changing speed, whilst others are just the reverse. I think there is a great future before the epicyclic gear. There are difficulties, but these difficulties, such as lubrication, can be overcome. I am now making practical experiments with epicyclic gears, and hope to publish the results in due course.

Mr. Spence, as an early advocate of worm gear, is to be congratulated upon having the necessary foresight to realise the future of this type of gear. It is a pity, however, that a man with his experience and personal interest in this type of gear should so vehemently attack new developments, especially when such de-

velopments have definitely proved their unquestioned value after thorough road tests on many thousands of gears apart from the efficiency tests of the National Physical Laboratory. Although he states that "there is nothing of the least value in the author's contribution on worm gear either from the design, constructional or application points of view," yet he can bring forward no single flaw or even hint of error in the geometry of the F.J. worm gear as published in my paper. Abstract condemnation is worse than useless unless backed by concrete evidence. The latter is entirely lacking, and for the simple reason that just as two and two make four so the geometry of the F.J. worm gear is just as definite and just as concrete. It would be more edifying if he could point out some error in these calculations. This he cannot do. These calculations, which open up an entirely new point of view, guarantee certain results, and these results have been fully borne out in actual practice.

His remarks regarding the useful formulæ shown on page 290 are of the same character and value as his other observations. My paper is not a treatise on gearing, and horse-power calculations do not fall within its scope. My paper being presented to automobile engineers, I naturally took an appropriate example, and on page 290 I illustrate a quick, simple and useful method of calculating the highest lead angle which can be used on the back-axle of a commercial vehicle with a gear-ratio of 10 to 1 where the limiting factor is the diameter of the worm wheel, in the particular case chosen 16 inches. I would inform Mr. Spence that problems of this nature are continually met with in automobile design, and that the formulæ given have proved, to actual gear manufacturers, most useful.

In reply to Mr. Richardson, I am not acquainted with the "Fairfield-Robinson" coupling to which he refers. Experiments have been made with double-helical gears for automobile work, but I know of no box on the market using this type of gear as yet. Single-helical gears were used with complete success on the Valveless two-cycle 25 H.P. engine, the gears connecting two crankshafts rotating in opposite directions.

Mr. Watt spoke about the bending of the worm shaft under heavy load. In this connection the F.J. worm is stronger than any other type (lead angles being equal) owing to the fact that it has no dedendum. Hence there is no cutting away of the worm shaft below the pitch line, except for the usual clearance.

(Capt. Bramley-Moore.)

With spiral bevel gears fewer teeth can, of course, be employed, but it must be remembered that a pinion with only a few teeth gives rise to a high rubbing velocity and, therefore, rapid wear. Spiral bevel gears with a few teeth can be generated very rapidly, and the question of rapid production is often one of the chief considerations with American manufacturers.

The question of the strength of the spiral bevel is being gone into by Mr. Bostock at the present moment, and the formulæ will be available in due course. Mr. Watt asks about the pressure angle of the F.J. worm gear. It has no pressure angle in the ordinary sense. It is built up, with varying angles. He suggests that it gives a greater thrust. So does a 20 degree tooth compared to a $14\frac{1}{2}$ degree tooth, but the 20 degree tooth is an absolutely sound proposition with many advantages. The extra thrust, if any, is not of great importance.

Mr. Parkes asked about the difference between the angular velocity of the F.J. worm and that of the ordinary type. It is not a question of actually seeing any difference in angular velocity, but rather that a smooth action is being given on the teeth of the gear instead of a hammering one. The difference may be of the slightest nature, but it sets up a hammering action on the teeth of the gear, and it is known that the F.J. will bear a heavier load than the ordinary type and that that hammering action is eliminated. As to his question in regard to the oil being squeezed out, it is extremely important to use special oil for worm gears. Mineral oils are unsuitable, and the use of such oil will probably result in the scoring referred to.

Mr. Keachie has asked me to express an opinion regarding a comparison between the $14\frac{1}{2}$ and 20 degree pressure angle. The latter provides three advantages: it gives a stronger tooth, there is less sliding and greater tolerance may be allowed in manufacture. The $14\frac{1}{2}$ degree angle, on the other hand, gives less thrust on the gears and is possibly a little quieter in action. The whole question is a matter of compromise, and a correct choice depends upon individual requirements. It is true that I have not mentioned the Lanchester worm gear in my paper. I think that the Lanchester gear has given most excellent results, and Dr. Lanchester is to be congratulated upon introducing a gear from which such splendid results have been obtained.

As regards the theoretical aspect of this gear, it seems to possess one or two drawbacks. The lead angle of a worm is dependent

upon the pitch diameter, and if the pitch diameter be increased then the lead angle will be decreased. In the Hindley worm the diameter gradually increases from the middle to the two ends, thus the lead angle relative to the wheel axis, is at the extremities of the worm smaller than at the middle, yet these varying lead angles are meshing with one and the same wheel, hence any form of tooth which might have good bearing surface at the centre of the worm has contact at the ends only in the form of interference. It seems probable that at the ends of the worm there is contact at the corners of the teeth only, whilst in the middle of the worm there is contact at the centre of the tooth only. The amount of interference can be reduced by making the wheel face narrow and the worm short, but can never be entirely eliminated. This procedure further reduces the amount of available contact, thus defeating the very object for which the hollow-faced worm was designed. In Dr. Lanchester's own words: "The Hindley system is the only one which actually obliterates the tooth form from consideration."

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

SCOTTISH CENTRE.

The Fourth Ordinary General Meeting of the Session was held at the Royal Technical College, Glasgow, on Monday, February 21st, 1921, at 7.30 p.m., Mr. D. Keachie being in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"The Design and Manufacture of Pneumatic Motor Tyres,"*

by Colin Macbeth.

There were present 32 Members and visitors.

* This paper was originally read in London, and is reproduced on page 379, *ante*.

THE DISCUSSION.

Mr. J. F. Henderson, in opening the discussion, said: I think that we take the pneumatic tyre more or less for granted. We grumble when it gives trouble, but if we consider the usage we give it we should say that it is wonderful that it stands up at all, especially when we remember that the back tyres are transmitting power as well.

I should like to ask whether running a tyre alternately in one direction, and then in another, makes any difference in the life of the tyre? I should also like to know, in connection with the use of twin tyres or large single tyres, the relative damage to a tyre caused by running it flat as compared with running it over-loaded. The opponents of twin tyres point out that if one tyre is punctured the other tyre has to take the whole load, but I have never heard anyone on the other side argue that a single tyre which is punctured has to run on the rim. It may be only for a few yards, but I am convinced that this does a tyre a great deal more harm than running it for several miles over-loaded, as happens with a twin tyre where one of the twins is punctured or soft. On the other hand, the chance of a twin tyre ever running on the rim is small, and may never occur in the whole life of the tyre, and I am sure this has quite a lot to do with the success of twin tyres.

Mr. Macdonald: Regarding the question of deflation of tyres, I understand one of the author's competitors has a method whereby when one of the tyres goes down below a certain pressure it releases a little plunger which fires off a cartridge and makes an explosion. I think that there is room for such an alarm on the ordinary pneumatic tyre. It is probably a question of getting a chassis made which would take a universal type. If a standard jack could be made at the same time it would be very helpful.

Mr. Cree: I am glad to see that the question of the sizes of the tyres fitted on light cars has been receiving attention so far as chassis makers are concerned. At the recent Glasgow show there were very few cars fitted with tyres less than 710 by 90 which fit a 90 mm. rim. This is a much more satisfactory tyre than the 700 by 80 (which fits a 65 mm. rim) for preventing rolling at high speeds, the factor which determines this being the relation of a line joining the road contact point of the tyre to a line drawn

(Mr. Cree.)
between the inner edges of the clinch of the rim, the latter line in the two rims being respectively 38·5 and 51·5 mm. long. With regard to the relation of the inner sectional area of the clinch of the rim to the bead of the tyre itself, the author showed the effect of making clinches either too deep or too shallow, and there is a considerable difference naturally between a 90 mm. rim and a 65 mm. rim. The distance between a line drawn between the inside edges of the clinch of the rim and the base of the rim is 10 mm. in the 65 mm. rim and 12 mm. in the 90 mm. rim. People sometimes go on alternating the size of the tyres. They found that a certain tyre will go on a certain rim, and so far as the length of the tyre is concerned it is so, but it may be found that the bead is not the proper size for the rim into which it is to fit. The bead of a 90 mm. tyre would not be a proper fit for a 65 mm. rim. That point might be elaborated by the author to show pictorially what is the actual effect of putting a tyre made for a larger rim into one which is too small for it.

Mr. D. Keachie: I was very glad the author made reference to the Straight Side tyre, for I was of opinion that we were getting a little bit behind in that respect. American cars, with the exception of the Ford, are almost exclusively fitted with Straight Side tyres, and the Straight Side tyre is established in that country for its ease of manipulation and excellent service on the road.

The Michelin Co. now supply a clinched tyre without security bolts, which is doing very good work and seems to point to security bolts as being unnecessary. The tyre can be removed and put on the wheel without any fear of nipping the inner tube, and can be manipulated by a novice without any fear. The tyre, owing to the absence of security bolts, can be operated on with the inner tube encased within the walls, and the tyre levers in no instance are ever in touch with the tube. This to the average car owner is an important consideration. He is not, as a rule, an expert on tyres, and the fixing of a tyre is always attended with the fear of a nip after a puncture.

In the old days of clinched solid tyres, nothing else was supposed to be able to take the drive until the advent of the band tyre, which quickly superseded all others, until now the band tyre is practically universal. The same, in some measure, applies to the use of security bolts.

Greatly increased comfort can be obtained by lowering the tyre pressure, though under-inflation is directly opposed to all in-

structions from tyre makers; while it may be harmful to heavy cars, I think it is generally conceded that on light vehicles the rapid depreciation threatened by tyre makers does not occur. Tyres should be constructed to resist a good percentage of flexing due to under-inflated tyres, and if tyre makers still insist on catalogue pressures, with threats of damaged tyres if under-inflated, the need for further development along this line is evident for the production of a tyre, cord or otherwise, to resist undue destruction, if under-inflated.

Mr. Macbeth, in replying on the discussion, said:

As regards the twin and the single, I think if one of the twins were punctured the run could be completed on the other if it were not overloaded, and so long as the puncture was attended to at the other end I don't know that very serious damage would be done. It depends on the vehicle.

Wrong Rims.—In reply to Mr. Cree regarding the fitting of tyres on to rims to which they are not suited, this is more often done than is realised, and with deplorable results. For example, a 700 by 85 or 710 by 90 tyre will be fitted as an oversize on to a 700 by 80 rim. Now reference to tyre catalogues shows that the 700 by 80 is actually an oversize tyre fitting a 650 by 65 rim. The dealer or the user, as the case may be, finds that he can actually fit the 90 mm. section tyre, but does not notice that the large beads are really so wide that the toes overlap when fitted. Such a practice causes tube nipping, blowing off, bead failure and excessive rolling. Usually the customer blames the tyre manufacturer, and unless the fact of wrong fitting is discovered, the tyre manufacturer generally accepts the blame.

Tyres for Light Cars.—Mr. Cree's remarks regarding fitting larger section tyres to light cars are very much to the point. The results of fitting comparatively light large-section tyres to small cars and consequently using lower inflation pressures are astonishing, enabling these cars to run with comfort at speed over rough surfaces. It is not fully realised that there is room for plenty of improvement yet, and it is well to consider still further increases in air-containing capacity at the expense of some increase in first cost but with the advantage of reduced running cost.

Effect of Under-inflation.—Mr. Keachie's remarks on this point I appreciate, and would say that under-inflation can only be stated to occur when the particular tyres referred to are "over-flexed"—thus the inflation pressure should be suited to the load. I think

(Mr. Macbeth.)
that many small cars are relatively more generously tyred than the larger and heavier cars. Motorists know well that it pays handsomely to fit larger tyres than those originally fitted to certain cars, whereas in other cases where generous tyring has been practised, the fitting of a larger section may be a disadvantage, as, for instance, in increased road resistance and reduced gripping powers.

Reversal of Direction of Rotation.—Mr. Henderson's point is interesting, and I know of no conclusive data proving that a tyre does better if always run one way. I believe that greater tyre mileage would be obtained if the torque could be always in one direction. We know, however, that such is not the case, as brake application probably puts a greater local torque on tyres than any driving effort. Thus, I think we can only be safe if we design our tyres to run equally well if reversed periodically from side to side, and modern tyres allow this to be done without obvious failure due to such a practice. The adoption of a more cushioned drive on the chassis would undoubtedly reduce the punishment of tyres when hardly used.

Large Single v. *Twin Tyres.*—Twin tyres are throughout their life extremely liable to have one of each pair taking the bulk of the load all the time or for a considerable proportion of the time. Assuming that deflation does not take place, I believe there is a greater liability to failure of twin tyres due to overloading than is the case with "big" single tyres. If a tyre is run flat it will be ruined, whereas if it is run under-inflated it does not ruin it right away. Five hundred miles with 20 lb. too little would not be a very serious matter, but in 1,000 miles it would begin to separate the plies, and tread separation would start and it would begin to break up. It would certainly tend that way, because they get very hot. If they are run under-inflated power is wasted and the tyre gets hotter.

In the case of deflation, if the driver does not notice the occurrence, the "big" single is liable to serious damage; the twin set, in the case of one tyre being deflated, has twice the normal load on the other tyre and nothing occurs to tell the driver that one tyre is bearing this extra load. This being so, there is a great danger that, in the case of the twin set, two tyres are ruined. There is a great need for a reliable deflation alarm which must operate when any tyre of a set is deflated or seriously under-inflated. The type of deflation alarm mentioned by Mr. McDonald

is novel to me, and I would think that one not requiring the use of a cartridge which has to be renewed is desirable. A suitable marketable alarm would be one which could be attached to some standardised part which would be the same on all makes of car —the only standardised part which suggests itself is the rim, though even at this point non-standardisation of rim attachments and felloe design make difficulties.

THE INSTITUTION OF AUTOMOBILE ENGINEERS.

SCOTTISH CENTRE.

The Fifth Ordinary General Meeting of the Session was held at the Royal Technical College, Glasgow, on Monday, 21st March, 1921, at 7.30 p.m., Mr. D. Keachie being in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"Producer Gas Practice," by JOHN STEWART.

There were present 53 Members and visitors.

PRODUCER GAS PRACTICE.

By JOHN STEWART.

If an air-tight vertical tube of any convenient size be lined with fire-brick, and be provided with a gas outlet at the top, an air inlet at the bottom and a fuel bed of sufficient depth, we have the elementary requirements for the production of air producer gas if the means of air blast is such as to cause a sufficiently high temperature at the base of the fuel bed. With such an arrangement of parts, a fire is kindled at the base, and a fan may be employed to urge combustion to the proper temperature. Gradually fuel is added as combustion proceeds, and if the gas outlet is kept open, it is found that a stage of depth of fuel bed is reached at which the gas at the outlet is combustible, supposing that the combustion zone temperature is sufficiently urged. It is immaterial whether the urging is produced by blast, i.e., pressure, or by draught, i.e., suction—air gas is the product. The original gas producers designed by Bischop and Siemens employed chimney draught.

From such small beginnings, dating back to 1839, when Bischop devised an apparatus almost as simple in character, except that a grate was employed to support the fuel and a hopper provided to feed the coal, the art of producer gas manufacture has advanced until it has reached the stage that it is now recognised to be the most efficient system of power generation from coal in powers of moderate capacity.

In an air producer as described, it is found that over 70 per cent of the heat value of solid carbon is recoverable in the gas so produced, a little under 30 per cent being developed on the grate.

If coke is employed as the fuel, and air only as the blast, gas of about the following composition results:—

	Coke, per cent.	Anthracite, per cent.
Carbon monoxide (CO)	29·0	32·3
Carbon dioxide (CO_2)	4·0	1·6
Hydrogen (H)	2·5	4·0
Nitrogen (N)	64·5	61·0
Marsh gas (CH_4)	nil	2·0

Gas of this class is of low quality, and equals only about 72 B.Th.Us. per cu. ft.

Theoretically, pure carbon, if burned in this manner, would give the following composition:—

Carbon monoxide (CO)	33·5 per cent
Hydrogen (H)	1·1 „
Nitrogen (N)	65·4 „

A disadvantage arose in the use of this method of producing gas, in that, with coal containing ash, which all coals do more or less, the ash became fused to clinker on the grate and walls of the generator and upset the uniformity of the gas quality by increasing the CO_2 and lowering the CO value of the product. A great deal of invention has surrounded this problem.

The next step was the application of a steam jet within a pipe so formed as to be an imitation of what is now known as the venturi tube. The steam blowing into this tube induced an air current ample to support combustion, the object of the contrivance being to regulate the volume of air supplied by a suitable adjustment of the steam jet. It was found that much better results were thus obtained, and that steam had a powerful effect in lowering the heat in the vicinity of the grate, i.e., in the combustion zone. But, what is more important, it was found that the thermal value of the gas was improved, as the hydrogen content was thereby substantially increased. The cause is readily recognised, as the steam is merely the gaseous form of water (H_2O), and the action taking place is that the H_2O is decomposed by the incandescent fuel, the hydrogen (H) going free and the oxygen (O) going towards the production of CO.

Here we have an endothermic or heat-absorbing action of steam employed to control the exothermic or heat-giving action of the air, which is adjustable in such a way that these are balanced and manifestly favourable to a more uniform quality of gas. In addition, gas produced by the action of the steam carries less nitrogen. Certain losses are, however, inseparable from these processes, and the whole of the heat value of the fuel is not obtainable from the gas produced.

The pressure system was first applied to the manufacture of gas for heating purposes. It is due to Mr. J. E. Dowson's more perfect apparatus for the production of this mixed air and water gas that the next step was taken in the application of such gas

to power purposes. This was first carried out on the pressure system, a small steam boiler being originally employed to provide the steam jet.

The following is a sample analysis of a gas produced in this manner from anthracite:—

Carbon monoxide (CO)	25·25 per cent
Carbon dioxide (CO_2)	5·25 ,,
Hydrogen (H)	18·50 ,,
Marsh gas (CH_4)	2·00 ,,
Nitrogen (N)	49·00 ,,

It will be noted that a distinct gain in hydrogen is obtained and a fall in nitrogen. The thermal value of the latter gas works out at about 160·00 B.Th.Us. per cu. ft., and of anthracite air gas sample at 132·69 B.Th.Us. per cu. ft.

The thermal efficiency of the air gas system is about 70 per cent, and that of the air and steam system about 80 to 90 per cent as a maximum.

Theoretically, the best possible gas capable of being obtained by the air and steam system is:—

Carbon monoxide (CO)	38·7 per cent
Hydrogen (H)	16·4 ,,
Nitrogen (N)	44·0 ,,

The next important step in the application of gas production is due to Monsieur Bernier, Paris, who in 1891 patented the system of using the suction action of the gas engine piston on its charging stroke to draw in the air supply required for combustion in the generator. No boiler or gas holder is required for this system, which is the one now generally known as the "Suction Gas Producer" system. It has been developed until it is such a simple apparatus, and so efficient and automatic once the engine is started, that it is deemed by most authorities to be the system which will yet become universally applied for the economic generation of gas power. It is this system which, in the author's opinion, will ultimately find widespread application to heavy commercial vehicles as a solution of the motor fuel difficulty with which we in this country and others are threatened.

It was stated that the formation of clinker was one of the first hindrances experienced by the pioneers of the original air gas system. The fusion of the ash has all along been a difficulty even with the air and steam method of producer gas manufacture, but

producer areas and temperatures have become so defined in relation to the power to be generated and the fuel to be used, that the trouble now falls within very manageable limits. The other important hindrance which was first experienced in the pressure type of gas producer was that bituminous fuels could not be used in so simple and direct a manner as anthracites. For metallurgical and other heating purposes this did not matter, as all volatile products carried over were burned. On the other hand, gases generated from bituminous fuel carry so much tarry matter that they are not usable in the gas engine without special cleaning and scrubbing. Many inventors have attempted a solution of this problem.

With anthracite coal of the best Welsh quality little trouble is experienced from this cause, but as the volatile constituents of the fuel increase, so also do the difficulties in their treatment. With an ordinary suction gas producer provided with the usual deep fuel hopper and container using anthracite, the incandescent mass of fuel resting on the grate and held within the fire-brick lining is graduated into three main zones. At the grate is the combustion zone, higher up there is the reduction zone and at the top the distillation zone. It is the latter zone in which the distillates are driven off by the heat from the lower zones. The distillates are of comparatively small volume and are readily dealt with by the gas scrubber. If, however, the hopper and fuel container be charged with bituminous coal to, say, 20 per cent volatile content, the distillates would carry over so much tar that the scrubber would be unable to wash it out, and it is the accumulated effects of the last traces which escape to the engine that upset its working. It will be observed that in the usual gas producer the coal is fed at the top and the air and steam at the bottom, and that the ash is finally produced at or near the fire grate, the gas being drawn off at the top. The oxygen entering at the base is used up, and hence no combustion of the volatile is possible by this system, which is known as the "Updraught." An attempt was made to invert this method of operation by so constructing the generator that the gas is drawn off at the bottom and the air drawn in at the top. The combustion is thus inverted, so that when raw fuel is fed the distillates are subject to combustion as well as the fixed carbon of the coal. Here, however, the difficulty experienced is that the fuel descends ultimately beyond the reach of the air, and, further, the ash descends to the

vicinity of the gas outlet. Another form is that in which air is admitted at both top and bottom, the gas being drawn off about midway. It combines some of the advantages of the pure updraught and downdraught systems, but as the gas exit is midway and usually to one side, unequal action is thereby induced within the fuel bed. Two chambers side by side have been tried, in which one chamber operates on the updraught while the fuel is fed to that chamber and the distilled products are caused to pass through the adjacent downdraught chamber, these being subjected to combustion at the top of the downdraught, the gas exit being at the base of each chamber and provided with reversing valves, the system being a reversible one. The inventor of that system finally sought to avoid the difficulties arising from the reversibility by designing a single structure with a division wall in the centre depending about two-thirds the depth. On one side of this wall is the combustion chamber, and on the other the distillation chamber. Fuel fed to the distillation chamber had the volatiles driven off by the heat from the lower zones, and these passed under the wall and up through the adjacent updraught and exit chamber. The system was more simply reversible, but unfortunately the rapid evolution of the volatile gases is endothermic in action and lowers the temperature to the point of impracticability. A close study of all these and other forms which had for their object the employment of a wider range of fuels, including those classed as bituminous, suggested to the author the idea that if a similar apparatus were employed, having the distillation chamber converted to a downdraught chamber, the action of that chamber would become exothermic instead of endothermic. This system was experimented with for some time with bituminous fuel, and so far as tarry products were concerned it satisfactorily eliminated these by combustion. It was possible to inspect the downdraught side and witness the effect of the engine suction. It was a reversible system, and in that fact lay its chief fault. It showed the way in which a final solution lay, and led to the formulation of another system of two-chamber generator, which avoids the necessity for reversing, which will now be described.

About the time these experiments were under way, it had been shown by an American specialist, who made an elaborate investigation into all the known types, that to gasify the fixed carbon of the coal an updraught generator was best, and that to gasify

the volatile content a downdraught served the purpose best, and that the difficulty was to construct an apparatus which would reconcile these opposing principles. A solution is shown in Fig. 1.

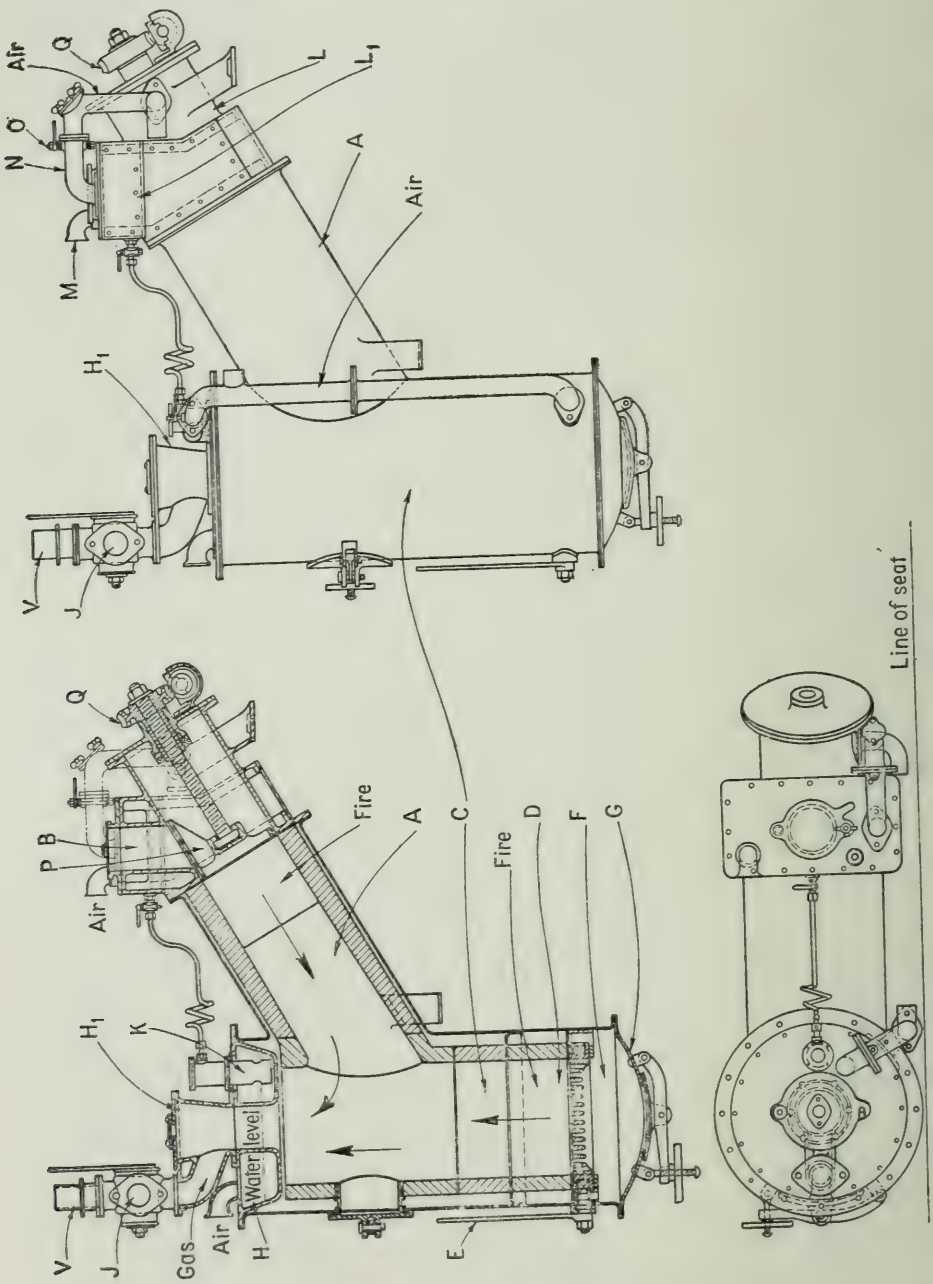


FIG. 1.

The generator in this system has the two functions just referred to carried out in two separate chambers. The angle chamber A is designed to operate continuously on the downdraught principle,

coal being fed in at the rear through the hopper B. Air is supplied also at the rear, together with vapour as required, and combustion takes place. It is to be observed that—assuming for a moment that the apparatus is fully charged and at work—the air, fire and fuel are precisely in the same position as in a simple "downdraught," so that when a fresh charge of raw fuel is fed it falls behind the fire. Hence, as distillation of the volatiles proceeds, it takes place in the presence of a supply of free air or oxygen, consequently the distillates are actually subjected to the process of combustion. This dual action takes place gradually and progressively until the fire creeps backwards entirely through the fresh charge of fuel and the whole mass is incandescent. This process is repeated with every fresh charge of raw material. Further, it should be observed that even should the distillates tend to escape the process of combustion as described, they cannot do so without passing through the high temperature zone immediately in front, whereby their character is changed. Text-books usually state the case thus: that if the volatile products are passed through an incandescent mass of fuel, they are thereby altered in their composition and rendered into fixed gases free from tar. Producer gas practice does not appear to bear this out as a scientific truth, for certainly all the systems referred to proceed on that principle and still fail to attain the desired result. It is here suggested that it is more scientifically true to say that it is essential to have the distillates evolved in the presence of air so that combustion may take place, thus reducing them to the common denominator CO_2, which, in passing through a zone of sufficiently high temperature, reduces to CO.

It will be seen that this chamber meets these conditions, and that its purpose is essentially the proper elimination of tarry matters and that only. This cannot be accomplished, however, without the consumption of a portion of the fixed carbon of the coal.

The function of the main or updraught chamber C is the same as in the ordinary single chamber gas generator. The fuel is, however, fed to it through the angle chamber. By this means it is possible to avoid the addition of raw coal to the updraught chamber during working. The advantage is that this obviates the discharge of the tarry matters into the main gas current, which would otherwise take place if the coal was fed in the usual way. When a charge of raw coal is dumped into a generator,

the heat therefrom drives off the volatiles too rapidly. For this reason containers are provided to permit the more gradual distillation, whereas in the design shown the action is as already described.

Looking next to the appurtenances of the system, there is a grate D swung on trunnions, the grate carrying the fuel and arranged to be operated by means of a vertical lever E on the outside. By this means the ash may be shaken through the grate and collected in the dish-shaped container F which closes the bottom. Subsidiary to this ash container is the small hinged door G for dropping out the ash at the end of a day's run. By operating the grate through a sufficiently large angle to and fro, the fuel may be consolidated at intervals. Also it has been found that the grate can be easily swung through a right angle and the entire fuel mass discharged in a minute. At the top of the updraught chamber is an evaporator H of simple form, the water in which is maintained at a level of $1\frac{1}{2}$ inches. The upper side of this evaporator is covered by a light copper plate, to which is brazed the inlet and outlet air supply which leads to the base of the generator to support combustion under running conditions. Within the air outlet pipe adjacent to the evaporator is fitted a small throttle valve I, which is closed during fanning operations. The gas outlet H is central, formed as a small fuel feed hole for charging up from cold, the gas escaping to the engine or the vent M as the case may be through the three-way cock J. For the regulation of the water supply to the evaporator, there is a small float needle valve, the float being in its own chamber K, which is provided with water outlets to the evaporator. By this arrangement the jolting of the car does not affect the action of the valve, the water level in the chamber corresponding to the water level in the evaporator. A constant volume of water in the evaporator gives a constant vapour saturation of the air. It has been found, however, that the system was capable of utilizing a greater volume of steam than was thus supplied, and provision is now made in the air supply pipe for an additional water drip, which is regulated by a small bib-cock.

The water jacketed casting L is designed to preserve the end of the angle chamber A from fire fracturing, and is extended upwards to form an evaporator and a water pre-heater L, for it is from this vessel that the water supply is fed to the main evaporator H, and to the small water bib-cock feeding the base of the gene-

rator. The angle chamber evaporator is provided with an air inlet M and outlet N and throttle valve O, which operate similar to those already described. It is also fitted with fuel hopper and inlet port B, from which the fuel is fed into the rear of the incandescent fuel bed. Experience has shown that we can dispense with the projecting part of the hopper, which will thus reduce the height of this part in subsequent plants. This chamber is fitted with a sliding grate P, which pushes the fuel forward as required, and by its withdrawal permits the fuel to fall in, there being a projecting wing which laps the $4\frac{1}{2}$ in. porthole in its forward position. The grate is operated by a worm gear and screw secured to the end cover plate Q. Both generators are lined with fire-brick.

It will be observed that this gas producing system belongs to the double fire zone type of generator, and is so arranged that each chamber may be operated as a single-chamber apparatus for a short period.

In all ordinary stationary gas producer plants there are not the restrictive conditions to be met with which pertain to a portable plant where compactness and lightness are essentials. Here, however, we have a means of overcoming one of the chief hindrances experienced with single-chamber apparatus. In the latter the fire-grate area can only be restricted by producing higher temperatures and higher velocities of air and gas currents. By this system it is possible to keep these normal by splitting up the fire-grate area into two sections. In this case the diameter of the larger grate is 12 in. and that of the smaller one 9 in. The depth of fuel usually provided for in stationary plants cannot be considered practicable in the case of a motor vehicle, but because of the method of feeding the fuel to the vertical chamber through the angle chamber, and that only with fuel from which the volatiles have already been discharged, it is thus possible to continue to operate on what would otherwise have been deemed an impracticable proposition. The gas, however, leaves at a higher temperature, and thus the problem of cooling the gas has to be faced under limiting conditions, as, if water cooling is to be applied, the water has to be carried. In this system a water scrubber is used, being placed on the opposite side of the chassis to that of the vertical generator. In the latest development, however, with the object of conserving the water supply, a preliminary tubular air cooler is placed under the driver's seat, and by this means it

has been found that the gas entering this cooler rapidly falls in temperature and becomes quite cool to the touch at the exit end of the cooler. The gas then enters the scrubber on which the minimum of duty is thus imposed, and passes down through the pipe at the side issuing into the scrubber at the base, whence it returns upwards, being douched with a fine water spray on the counter-current principle. It then enters a central downtake pipe and passes out at the lowest point. At the base of the scrubber is provided a water-seal pot forming a water jacket into which the water from the scrubber overflows, and from which it is raised by a small Rotoplunge pump and delivered into the water tank at the rear. The water is drawn from the rear tank by a similar pump and is raised to the water spray level, where it is forced through the jets. It is part of this water circulating system that the two pumps are of equal capacity, so that the pump which draws off the water from the base of the scrubber can deal with all the water which is pumped in by the other, the two pumps being in line and driven by the same chain. The drive is obtained from a special star coupling-piece fitted with small toothed sprocket wheels immediately in front of the gear-box, the drive being by a Coventry silent chain to a small counter-shaft over the chassis cross members, and from there by a roller chain to the pumps, which are nested to the skeleton frame supporting the generators and scrubber. The water pipe connections are led to and from these two pumps and the rear water tank along the inside of the chassis frame. The water tank is fitted with air current tubes, and by breaking up the section of the water facilitates its cooling, the heated water from the scrubber entering at one end and being drawn off cold at the other. The gas having been cooled to a point and washed free from any fine dust carried over, it is then carried forward to the dash-board, where is fitted a tubular cooler and condenser, so that any moisture carried forward with the gas has a final chance of being condensed to water. This drips back into the box at either side, where it falls to the lowest point and drains off through the small water-sealed coils attached to the bottom. The gas is then led to the gas carburettor, Fig. 2, Plate XXVII, which is really a gas and air proportioner for the production of an explosive mixture. It is based on the principle that, for a high-speed quick-revolution engine such as the petrol engine is, time is an important factor in its operation, and that there is every reason to present the mixture as perfectly as possible to the

engine, so that the compression stroke will complete the process. The carburettor is in this case arranged in front of the steering gear column. In the petrol equipment, the carburettor is placed close to the engine so that the heat may assist the vaporisation of the liquid and prevent its recondensation. In the suction gas system we are dealing with a fuel which is already a perfect gas, from which the cold favours the discharge of objectionable moisture. Hence, there is only convenience of arrangement and manipulation to consider. The carburettor has an air shutter at the ingoing end which is set full open for starting purposes. After starting, this is partially closed, and the main air shutter at the centre is opened and set. The gas is regulated by a plunger valve having a conical end, opening or closing the gas inlet. The gas passes forward to the outside of the plunger, which is hollow and perforated with small holes so that the gas may pass through to the interior. Before doing so, however, it comes in contact with the air currents, which are also responding to the suction of the engine, and thus become well mixed on the way. They pass in a straightforward course to the throttle valve, which is close to the cylinder so that immediate control of the engine speed can be obtained. When both air shutters are partially open, it would appear, judging from outside appearance, that these currents would baffle each other, but their direction is unified with the gas direction by means of the two interior cones. Another constructional feature is that the piston guide surrounding the perforated plunger provides that an increased gas opening is accompanied by an increased air opening independent of the main rotating air shutter.

Having described, then, the system which is submitted as a solution of the motor fuel problem so far as commercial cars are concerned, and which has been successfully operating in Glasgow for sufficiently long to give assurance of its practicability and general economic utility, there remains yet the important question as to whether the fuel supply is assured. The coal used in all the experimental work undertaken has been *mainly* Scotch anthracite, and it will be recognised by those with any experience of stationary gas producers working on this fuel that sufficient attention to keep the inlet valve clean is required even when ample scrubbing capacity is provided. These experiments have covered a period of two years, during which only six cwt. of best Welsh anthracite have been used. Needless to say that for

general cleanliness in handling, this coal is to be preferred, and that the general results are better because of the lower ash and volatile content. Still, Scotch anthracite can be gasified in a more satisfactory way than by any other known system because of its ability to properly convert the volatiles into fixed gases. Indeed, it is intended to use bituminous coal in larger sized plants, as experience has shown that this will be practicable in the somewhat larger sizes, and there is no doubt that it can also utilise low temperature carbonisation coke. Having thus submitted a scheme by which a motor vehicle has been operated continuously for ten hours, and that prior to that the fire had been continuously in the vertical generator for the previous eight days, it is justly advocated that suction gas offers the best and most immediate solution of our transport fuel problem. Like the petrol machine, it is dependent on the availability and permanency of the fuel supply.

During the War an Inter-Department Committee on Gas Traction was formed in 1917 under the auspices of the Petroleum Executive.

Their terms of reference were to report upon:—

(1) "The employment of gas in substitution for petrol and petroleum products as a source of power, especially in motor vehicles, and the manner in which such gas may be supplied, stored, carried and used, with due regard to the safety of the public."

(2) "The action, if any, which should be taken by His Majesty's Government to encourage and safeguard the use of gas for this purpose."

In their report they dealt with the subject of town gas containers, compressed gas with steel containers, and suction gas portable plant.

It was found that 250 cu. ft. of free gas was equal to one gallon of petrol, the gas being 490 to 500 B.Th.Us. per cu. ft. The Report states that "an ordinary motor vehicle engine, unaltered structurally as regards the compression space, will on the average give the following results between half and full load:—

When working on petrol	100
When working on town gas of 450 B.Th.Us. gross per cu. ft.	91
When working on suction producer gas of 210 B.Th.Us. gross per cu. ft.	87
When working on suction producer gas of 140 B.Th.Us. gross per cu. ft.	82

That with portable suction gas plant an average performance of 1·8 ton-miles may be obtained per lb. of coke, and 2·2 ton-miles may be obtained per lb. of anthracite.

Average fuel consumption and costs per ton-mile.

With coke at 45*s.* per ton and 0·55 lb. per ton-mile ... = 0·133*d.*
With anthracite at 55*s.* per ton and 0·45 lb. per ton-mile = 0·133*d.*
Taking the average anthracite consumption at 0·5 lb., cost at 50*s.* per ton.
Taking the average petrol consumption at 1 gal. = 40 ton-miles, cost at 2*s.* 6*d.* per gal.
Each penny spent on anthracite yields 7·5 ton-miles.
Each penny spent on petrol yields only 1·33 ton-miles.

Summarising:—On the average 1 cwt. of coke or anthracite used = 5·6 gals of petrol.

The work done by suction gas using anthracite equals that of petrol if its price is 5·4*d.* per gal. At present the price is 3*s.* 8½*d.* to 4*s.* 0½*d.* per gal.

The Report states that each 100 cu. in. explosive mixture when from suction gas of 140 B.Th.Us. per cu. ft. with 1·33 vols. of air to 1 of gas = 3·9 B.Th.Us., against between 4·25 and 4·5 with the average petrol mixtures.

What are the economical advantages presented to the transport industry by the adoption of suction gas vehicles?

This is best determined by a reference to the British Thermal Unit Standard of measurement:—

1 lb. of anthracite produces about 80 cu. ft. of gas.
13 lb. of anthracite, therefore, produces about 1,000 cu. ft. of gas.
1 ton of anthracite costs, say, 55*s.*
1 cwt. of anthracite, therefore, costs 2*s.* 9*d.*
13 lb. of anthracite, therefore, costs 3¾*d.*
1 cu. ft. of producer gas is about 140 B.Th.Us.
1 cu. ft. of town gas is about 550 B.Th.Us.

Hence 1,000 ft. of town gas = 4,000 cu. ft. producer gas.

The Inter-Department report of the Fuel Research Board determined that 250 cu. ft. of town gas = 1 gal. petrol. Therefore, 1,000 cu. ft. of producer gas = 1 gal. petrol.

But 1 gallon of petrol costs, say, 3*s.* 8½*d.* for No. 3 quality, whereas 1,000 cu. ft. of producer gas costs only 3¾*d.*

$$\frac{3s.\ 8\tfrac{1}{2}d.}{3\tfrac{3}{4}d.} = \text{approximately } \frac{1}{12}$$

For practical purposes we may take it that for a 4-ton car the average coal consumption works out at—

28 lb. per hour.

2 cwt. per 8 hours' day.

A day's work on that basis will cost 5*s.* 6*d.* for anthracite; 16 gallons of petrol at 3*s.* 8½*d.* costs £2 19*s.* 4*d.* for petrol,

approximately $\frac{1}{10}$th.

Further, the Fuel Research Board reported that 1,000,000 B.Th.Us. from petrol at 3*s.* costs 21*s.*; therefore, 1,000,000 B.Th.Us. from petrol at 3*s.* 8½*d.* costs 25*s.* 11½*d.*

1,000,000 B.Th.Us. from producer gas costs 3*s.*, using special coke = approximately $\frac{1}{9}$th, estimated on stationary suction gas practice.

Tests have shown as low as 0·85 lb. of anthracite per horse-power per hour for small units.

If we take the usual estimate of 1 lb. of anthracite and an engine developing 40 h.p. at 1,000 revs. per minute and suction gas giving 82 per cent efficiency of petrol, such an engine would give 32·8 h.p. per hour. As a car is not always exerting its full power, it is safe to say that the former estimate quite covers the case at 28 lb. of coal per hour. It should be observed, however, that this fall in power to 82 per cent of petrol power yield is based on unaltered engines. If, however, advantage be taken of the increased compression permissible and this be raised from 80 lb. to 125 lb. per sq. in., then a gain of 12 per cent is obtainable. This would entail, however, the introduction of either electrical or compressed air starting gear.

It will be seen from these figures of cost that however we examine the case there are outstanding economies discoverable which more than justify the application of suction gas as a motor fuel on a very extensive scale, and it seems unlikely that petrol prices will ever reach the point at which suction gas cannot operate at one-sixth of the fuel costs.

Little wonder, then, that Lord Montagu says "that producer gas is the most promising of all recent fuel-using developments."

It will be observed that in the above comparison of heat values of the explosive mixtures these are not by any means proportional to the B.Th.U. value of the fuel used, whether liquid or gaseous.

The lowest value is suction gas, say, at 140 B.Th.Us. per cu. ft.; the next value is town gas, say, at 500 B.Th.Us. per cu. ft.; then petrol, say, at 19,000 B.Th.Us. per lb., which approach

very near each other in final explosive value because of the less volume of air required, thus by the exclusion of the larger percentage of inert nitrogen in the poorer gas raising its useful value.

There is a strong movement towards the elimination of the smoke nuisance arising from the use of the steam driven vehicles, which frequently emit dense trails of smoke at low levels. The suction gas equipment is altogether superior in this respect as no smoke is ejected under running conditions. Further, as compared with the steam vehicle, the plant is very much lighter, and consequently less wear and tear of tyres takes place, and the same applies to roads, the maintenance of which constitutes an important item of expense to public authorities. It occupies much less of the wheel base space than the steam plant, and is more economical in the use of fuel. It is a well recognised fact that suction gas stationary practice is more efficient than steam, and this also applies to the portable gas plant vehicle. For moderate powers, the fuel consumption of stationary plants is approximately double that of the equivalent gas plant. The latter also uses less water as this is not converted into steam as in a boiler, but is conserved for circulation to and from the scrubber. These are all important matters in the problem of motor vehicles and are in favour of the portable gas equipment.

Inasmuch as experience on the road with the car illustrated has satisfied the exacting requirements to be met with in taking its place in the queue of traffic passing along Argyle Street and Jamaica Street, our busiest thoroughfare, and that the fuel consumption has shown that an eight hours' day can be operated on a coal consumption of not more than 2 cwt. of anthracite, it is felt justifiable to predict that suction gas will find widespread application as a motor fuel in the immediate future, and also that petrol can never approach it on the basis of actual fuel costs. Further, that in connection with ship propulsion it offers by far the best means available for meeting foreign competition and is capable of rivalling the economical performance of the Diesel engine for that purpose on an all-round commercial basis reduced to pounds sterling. That, however, is a bigger subject, although not a more difficult proposition.

THE DISCUSSION.

Mr. J. F. Henderson: There are two points I would like brought out. The first is, what approximately is the weight of the whole producer plant which is added to the vehicle, and second, what horse-power per sq. ft. of grate surface does the author get. I understand that Col. Smith got about 70 h.p. per sq. ft.

Mr. J. Richardson: The apparatus described to-night appears to me to be too complicated; the Smith producer which was recently described to us seemed to be only a fraction of the size and weight for performing the same work, and, if that was a satisfactory means of using producer gas for propelling motor vehicles, I fail to see why scrubbers, washers and all these other appurtenances are required. I think the author has been too optimistic when he said that the case for producer gas has been proved, when I gather that he has only burned 6 cwt. of anthracite. In marine work the most economical passenger ships operating to-day are oil fired, in spite of the fact that the price of oil is some three times greater than that of coal, and although these ships only use about 25 per cent less oil than coal per h.p. developed, which shows that the first cost of the fuel is not by any means the only consideration. The "Aquitania," the largest Atlantic liner in operation at the present time, is oil fired, and they are now succeeding in turning round that great ship at Liverpool, putting stores and fuel aboard and doing all the necessary repairs, in seventy-two hours after her arrival from New York. It is savings such as these in the marine world that make all the difference between running ships at a profit or at a loss. I cannot help thinking that the cost of repairs and upkeep of such a large plant as that shown by the author would go a considerable way towards counteracting any saving in actual cost of fuel.

Mr. G. E. McCaw: I would like to ask the author how long it takes from lighting up in the morning until the vehicle is under way, and what is the time required for re-starting after a lengthy stop, say a stoppage to discharge a load of coal? Col. Smith claims that he can get away in twenty minutes' time, and in three to four minutes' time after a lengthy stop. Can the author also give us the approximate cost of the apparatus, because, judging

by the photographs we have been shown, I should say there is a good deal of work entailed in its manufacture, and we have therefore to add the initial cost of the apparatus to that of the vehicle? The test was evidently carried out with a loaded weight of two tons on the vehicle, and I presume it was carried on a 2-tonner with a 25 h.p. engine, because he says it reduced the horse-power down to 20. Will the author give us the actual b.h.p. of the engine and the carrying capacity of the vehicle on which the trials were made?

Mr J. Watt: In the application of suction gas to commercial vehicles, we are as yet only in the early stages of pioneer work. Looking at the plant described, I am struck by its great complication, and I question if a driver of ordinary skill would be capable of keeping it in tune, quite apart from his ability to manipulate properly the numerous air and water cocks, etc. while on the road. The author has not stated whether the fuel is fed automatically, and if it be the case that fuel must be added to the hopper by hand at frequent intervals, it would certainly be necessary to carry two men, a driver and a steerer, so that, from this point of view, suction gas vehicles would appear to be rather in the same class as steam vehicles than in the petrol lorry class.

Would the author say if an experimental vehicle has been run any considerable mileage with a load of two or three tons on board? A plant may appear more satisfactory when running light than would be the case under ordinary service conditions.

Mr. J. D. Parkes: I agree that the consumption of petrol and oil fuel is such as to jeopardise the economical use of internal combustion engines using those fuels. There is undoubtedly a necessity for a substitute, and any research work in this direction is extremely valuable. The whole point at issue is whether suction gas is the most promising substitute or not. The author's plant appears to me to be open to three main objections: it encroaches on the body space, it adds very appreciably to the tare weight of the vehicle, and it is obviously somewhat complicated. He has already admitted that a suction gas plant in any case is an extra as compared with the normal petrol vehicle, and the ideal is therefore to obtain a suction gas plant which will give the minimum amount of complication and additional weight.

I understand that the weight of the author's plant is at least 4 cwt., that is, roughly 5 per cent of the weight of a vehicle weighing four tons. There is in existence a suction plant of

(Mr. J. D. Parkes.)
another type which weighs only 2 cwt. whilst still in its experimental stages, and which gives promise of weighing not more than 200 lb. when perfected. To my mind, this question of weight is extremely important, perhaps even more so than the other point that the Stewart producer encroaches to an appreciable extent on the platform space of the vehicle.

It seems to me that to obtain technical perfection a certain amount of complication is permissible. For example, the Smith producer, which has already been described before the Institution,* is fitted with an automatic fuel feed, governed both by the engine speed and the throttle opening, which entails a few extra mechanical parts which are not present in the Stewart plant. This addition is, I think, justified, because it greatly improves the flexibility of the engine and the overall quality of the gas. It is not difficult to run a producer so as to get good gas at a given load and given speed, but for road work it is absolutely necessary that good gas shall be supplied at almost any engine speed and any throttle opening.

As regards the modifications necessary to the engine and chassis so as to get the best results with producer gas, the first thing is that a bigger engine must be used as a much lower brake mean effective pressure is obtained with producer gas than with petrol fuel, and, secondly, the compression will have to be increased, as the best compression pressure for producer gas is somewhere about 120 lb. per sq. in. gauge, whereas the normal petrol engine will certainly not go above 90 lb. per sq. in. gauge.

It is further essential that the whole of the producer and scrubbing gear shall be mounted so as not to interfere with the existing body space of the vehicle, and so that the whole of the gear is easily accessible for cleaning and general maintenance. These requirements are such that it is obvious that producer gas will not give the best service results until vehicles are specifically designed to run on it as a fuel and not merely as an alternative to petrol.

I would have liked to have a little more information regarding the horse-powers obtainable from various grate areas under working conditions, and as regards the percentage drop in power which is to be anticipated as between petrol and producer gas. I think I am right in stating that a good deal of work has already

* See Proc. I.A.E., Vol. XIV. p. 169.

been done with a Smith producer, and that quite recently something in the order of 88 h.p. has been obtained from a 12 in. grate without any dust or fine particles of fuel being carried over owing to the high gas velocities. The drop in power, assuming that an engine has its compression raised to suit producer gas, would, I think, be something of the order of 20 per cent to 23 per cent, but I would be glad to have the author's confirmation of these figures.

Mr. D. Keachie: I owe an apology to the author for pushing him to deliver his paper at this date, because he wanted to make a number of additional tests, as he felt that tabulated data from both bench and road tests would be more useful to us as well as strengthening the case for suction gas. While the economy of this form of power is an assured fact, it still requires longer road tests to prove its reliability. If twelve or twenty-four vehicles fitted with suction gas producers could be put into the hands of ordinary drivers, the experience gained by these men would go a long way to determine whether suction gas as it now stands is of commercial value. It is only the test of use in the hands of ordinary users and ordinary drivers that is convincing.

Mr. Stewart, in replying on the discussion, said: Regarding the first point raised, namely, the grate area in proportion to the power, I think this question is yet to be solved by pioneers in the matter, for the simple reason that we are up against a new proposition. The ordinary single-cylinder gas engine takes one suction stroke in four, and the grate areas are proportioned to the size of the engine and are worked out at 0·05 sq. ft. per horse-power. Here we have a four-cylinder engine having multiple suctions with high revolutions, and no one has yet determined the actual area required for developing a given power under these conditions, and I do not pretend to define them exactly. We have, however, driven the car with a 12-inch diameter plus 9-inch diameter grate, and run for ten hours, and I conclude that we are on the proper area for the particular type of car. In order to get exact figures it will be necessary to undergo bench tests. Our work has been carried out on the road so as to realise what the actual difficulties were. We are not automobile engineers to begin with, but we did know about suction gas and the difficulties about fuels. Mr. Richardson has referred to the simplicity of the "Smith" plant. I think that the compliment is justifiable, because

(Mr. Stewart.)

Smith's is a very simple plant. It proceeds on the principle that with a bed of fuel of 7 inches it can develop with a 12 inch diameter generator all the power required. Ordinary suction gas practice insists on a suitable depth of fuel for the unification of the gas quality. It has been found from ordinary stationary practice, carried out in technical colleges, that if with raw coal good gas is produced with a 24 inch depth of bed, good gas will continue to be produced until only a 9 inch bed is left, after which the gas ceases to become uniform. Therefore, if in the Smith apparatus they are resorting to a 7 inch bed to begin with, they are essentially departing from ordinary practice. The point of the invention I have described is that we are not using coke or rich anthracite but lower qualities of coal containing a higher percentage of volatile matters. Last week the driver showed me a bucketful of ash produced from the C chamber. If we could run with that quantity of ash produced in a small generator we are not doing badly. If we run with better qualities of coal, we will get better results all round. I appreciate the point about the use of oil for vessels such as the "Aquitania," but if all this oil is diverted into oil-burning ships, then the automobile industry is going to be starved out of existence. Therefore, the proposition of suction gas depends on the root fact that the world is threatened with shortage of petrol, and that is the basis upon which I have rested my case. The total weight of the apparatus is 4 cwt., and there are 3 cwt. of water contained in the rear tank. We can introduce aluminium into the apparatus and reduce the weight very considerably. We are also experimenting with ordinary fire bricks, and we may reduce the weight here too. Regarding the question as to our having used only 6 cwt. of anthracite, what I stated was that during the two years' experiments we had only used 6 cwt. of *Welsh* anthracite; all the remaining anthracite was Scotch of the lower qualities. Our idea was to use inferior qualities of coal, but we have tried, and purpose again trying, the better qualities of Welsh. As a matter of fact, we have used tons of Scotch anthracite.

With regard to the total cost of the plant, it must be recognised that any suction gas proposition is extra over and above that of the petrol equipment. The only items that can be discarded are the petrol carburettor and tank. But as an experimental plant is more costly than one produced commercially, the cost will be a question for manufacturing consideration. I would say that

the fuel saving and the other economies are such that as a commercial proposition it is amply justified. With regard to the question of time in starting, we have lit two generators and started in 30 minutes. We can do it usually in three-quarters of an hour. Gas can be produced in less than 20 minutes, and we are able to start just as well as others are doing, and to operate on a cheaper quality of coal than any other, and that is the thing that counts in reducing the operating costs. The time taken to start after a stoppage is very small, that is to say, a matter of ten minutes, but our ordinary practice is to keep the engine running, because with suction gas the stand-by losses are very small. That is to say, the amount of coal used is in proportion to the load, so that automatically the consumption falls immediately with the revolutions and power absorbed by the engine. The cost of the coal being very small, it is quite a business proposition to continue running the engine for three-quarters of an hour rather than spend ten minutes for starting up.

The engine in question is a 40 h.p. engine in a 4-ton vehicle, but as I said, we have not yet tested its actual capacity. Regarding the other question, I may say we have to coal by hand at present—it is not done automatically. It is proposed to do as Smith did, that is to say, erect a small hopper over the gas plant so that the coal can gravitate into the hopper, being regulated as required, and then pushed forward as described.

With regard to the regular feeding of the Smith plant, it strikes me that it is necessary to use the very best quality of Welsh anthracite, but that even then the system does not get rid of the volatiles, but only distributes them over a longer period by giving off less in a given time. Because the depth of fuel in the plant referred to is less than standard practice, the temperature of the outgoing gases is greater. With these hotter gases additional cooling medium is required, i.e., either larger scrubbers or an air cooling system. If the gases are being obtained from a 7-inch depth of fuel bed instead of a 21-inch depth or over, naturally the temperature is greater, as a shallow depth does not permit of the usual re-actions which take place with ordinary producers in the decomposition and distillation zones, in fact, the whole of the 7-inch depth is virtually a combustion zone. I think there is a greater loss of efficiency due to that very fact. So far as speeds are concerned, I have sometimes wondered whether suction gas is not equally efficient compared to petrol, because we have taken

(Mr. Stewart.)
a 2-ton load uphill at ten miles an hour and without changing gear, and when on the level have travelled as fast as the law will allow. Regarding the question of attending to the firing, the same practice as in the case of the steam lorry will have to be followed—one man to drive and one man to attend to the producer.

Answering the question by Mr. Crowther as to whether hills such as that in Gardner Street, Partick, had been attempted, I may say that we have not faced hills of that quality. We have gone through Rutherglen, and we have gone to Uddingston from Parkhead Cross with a $2\frac{1}{2}$-ton load in 25 minutes, up hill and down dale. There is a fair incline from Parkhead Cross to Tollcross. As to the fear that going up a steep hill might affect the water level and scrubbing arrangements, a little valve protects the water in the evaporator from the motion of the car. The tilting of the car might disturb it, but so long as the means of admitting the water to the evaporator operates it would not matter. So far as the scrubber is concerned, this is provided for by the special type of seal at the base of the scrubber.

With regard to the general principle of the gas producer, the double-chamber is chosen primarily for meeting the objections which all ordinary persons have to the gas producer, namely, the presence of the distillates. These cannot be removed in a single-chamber apparatus for the reasons already submitted, namely, the absence of means for their combustion. The reason for the introduction of a scrubber is that the gases are of high temperature when leaving the generator and have to be cooled. Nothing will cool gases so quickly as water, and therefore a scrubber is used. In all gas producer practice, particularly where agitation takes place, as in a moving vehicle, a certain amount of dust is produced which has to be washed out or cleared. In our case, we wash it out. Regarding the question of weight carried, at one time we thought we would not be able to carry big loads, but we found we did better with the load than without it, because of the area of the producer which provided fuel surface to meet the increased power required by the increased load. The area of the producer is determined by the amount of work to be done by the producer. We have found with the areas for which this plant was made that we could carry a 2-ton load because there is an increased temperature due to the load, and that means an increased quality of gas so long as there is the proper quantity of fuel. The main

point is the ability of the producer to get rid of the volatiles by purely chemical reactions and without the introduction of intricate mechanical appliances.

I may be permitted to add as an appendix to the discussion a statement of the fact that I am now in a position to meet the main characteristic objections raised by my critics in regard to weight and cost. In my latest design, which is based on the practical tests, I have incorporated all the ideas which experience on the road has suggested. This result was foreshadowed in my reply dealing with the method of feeding, where I suggested simplification as the ideal to be aimed at. The latest design accomplished this by cutting out the most complicated and heaviest casting and substituting an aluminium casting of simple form which permits easy access for charging and poking down the whole interior of the angle chamber, the fuel descending by gravity instead of by means of the grate and its gear. It is noteworthy that no other system permits this opening out, poking down, and inspection of fire during working conditions, and which at the same time permits of the fuel being fed at intervals as required. Further, the three-way cock on the earlier design is now abandoned, and one casting made to serve a dual purpose. It is now possible to raise the scrubber and dispense with its earlier binding straps as illustrated. Other details are receiving attention in the same direction, i.e., towards simplification without departure from the main principle which has already been demonstrated to be practical and efficient. Further, by placing the generator behind the driver, the engine is as accessible as ever, and the space occupied by the generator may be somewhat compensated for by drawing the loading platform backwards to overhang the chassis frame on the rear while avoiding any structural alterations and obstruction forward of the driver and at the same time placing the load more central over the back-axle. These latest modifications, it will be recognised, tend towards reduced costs as well as simplicity and accessibility to all vital parts.

THE
INSTITUTION OF AUTOMOBILE ENGINEERS.

SCOTTISH CENTRE.

The Sixth Ordinary General Meeting of the Session was held at the Royal Technical College, Glasgow, on Monday, May 2nd, 1921, at 7.30 p.m., Mr. D. Keachie being in the Chair.

The Minutes of the previous Meeting were read, confirmed and signed.

The following paper was then read and discussed:—

"Comparative Motor Cycle Performances," by D. S. Heather.

There were present 39 Members and visitors.

COMPARATIVE MOTOR CYCLE PERFORMANCES.

By D. S. HEATHER, B.Sc.
(Associate Member).

In a previous paper* the author dealt in some detail with the mechanical design of the motor cycle. In this paper he has confined his attention entirely to the performance of modern motor cycles and motor cycle engines, since, while there is now a considerable amount of published matter relating to mechanical questions, there is very little indeed relating to the performance of motor cycles.

He therefore trusts that this paper will not only prove of interest to those whose primary interests lie with the design and manufacture of four-wheeled vehicles, by reason of the opportunity which it gives for comparing the motor cycle with the car or the lorry, but that it will also be of use to those actually connected with the motor cycle industry, because it contains information which has not previously been collected into handy form.

The subject necessarily divides itself into three main sections:—Tractive Resistance, Engine Performance and Road Performance. These three sections are dealt with in the paper in the order named.

Tractive Resistance.—It has been shown by Wimperis and others that the resistance to motion of a vehicle at varying speeds may be expressed with quite reasonable accuracy by the formula:—

$$\mathrm{Tr} = \mathrm{A} + \mathrm{B}v^2$$

where Tr = resistance to motion (tractive resistance)

v = speed of vehicle.

A and B are constants depending on the particular vehicle and road.

This formula is not strictly true, since it is likely that there are certain resistances which vary directly as the speed of the vehicle, so that $\mathrm{Tr} = \mathrm{A} + \mathrm{C}v + \mathrm{B}v^2$. Experimental data, however, show that the Cv term has so small a constant as to be negligible. Unfortunately it is not possible to determine the constants analytically, by reason of their complex character. For instance, the

* See Proc. I. A. E., Vol. XIII., p. 247.

constant A, which gives the term which is unaffected by the speed of the vehicle, is obviously related to the friction in the wheel bearings, and also to the rolling resistance of the tyres on the ground, which has been shown by Ewing and others to be substantially independent of speed in the case of pneumatic tyres. The bearing friction cannot be determined analytically with any accuracy, neither can the rolling resistance, particularly when it is remembered that it depends on the degree of inflation of the tyres, and also on the state of the road surface. The constant B is similarly complex, since although it is undoubtedly partly a question of wind resistance, it may also contain some allowance for such losses as oil churning in the mechanism, the suction effect of tyres on a wet road, and losses due to the impact of the tyre on the road. An analytical determination being, therefore, impossible, the only possible method is to proceed experimentally.

In a paper read before the Institution in 1914,* Mr. Wimperis gave particulars of the method which he adopted. This consisted essentially in allowing the vehicle to decelerate with the clutch disengaged, measurements of the deceleration being made at intervals, by means of an accelerometer, as the car slowed down. These figures are then plotted on a velocity base, the resultant curve being, of course, a deceleration-velocity curve. The weight of the vehicle being known, and a suitable allowance being made for the inertia of the rotating parts, such as the road wheels, it is only necessary to alter the scale of ordinates to convert this deceleration-velocity curve into a resistance-velocity curve.

For example, suppose that the car to be tested weighed 1900 lb. complete, and that the equivalent weight of the rotating parts was 100 lb., so that the total weight for deceleration purposes could be taken as 2000 lb., and that testing in the manner described above gave the following decelerations at the noted speeds, as plotted in Fig. 1:—

M.P.H.	Deceleration. ft. per sec. per sec.
10	0·82
15	0·94
20	1·11
25	1·33
30	1·60
35	1·91
40	2·27

* See Proc. I. A. E., Vol. VIII., p. 281.

Now it is evident that the deceleration at any particular speed bears the same ratio to the acceleration due to gravity that the decelerating force bears to the total weight of the car, so that, at 20 miles per hour, for instance, the tractive resistance

$$\mathrm{Tr} = \frac{\text{deceleration}}{g} \times 2000 = \frac{1{\cdot}11 \times 2000}{32{\cdot}2} = 69\,\text{lb.}$$

In other words, it is only necessary to multiply the deceleration at any particular speed by $2000/32{\cdot}2 = 64{\cdot}1$ to obtain the tractive resistance at this speed, and to this the scale on the right of Fig. 1 corresponds. Examination of the curve shows it to follow the law $\mathrm{Tr} = 45 + 0{\cdot}06\,v^2$, and it is, therefore, legitimate to say that the resistance to motion of the car is given by that equation. This, of course, applies only to the particular car under test, or to other cars of identical design, but it is evident that if tests are

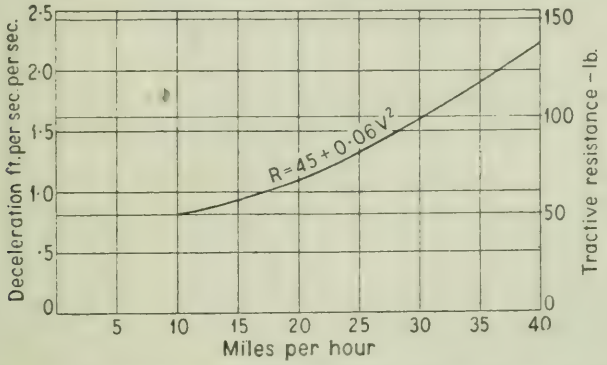


FIG. 1.—Deceleration Curve for Hypothetical Car.
Weight of car, 1900 lb.
Equivalent mass of rotating parts, 100 lb.

made on a sufficient number of dissimilar cars it will be possible to predict with reasonable accuracy the tractive resistance equation for almost any car.

The large number of tests which have been made on cars and lorries with the Wimperis and Lanchester accelerometers therefore cover the ground fairly well so far as four-wheeled vehicles are concerned, but unfortunately the author has failed to find any published results of tests on motor cycles or sidecar combinations, and he has, therefore, been constrained to obtain the necessary experimental data himself. An accelerometer is of necessity a rather delicate instrument, and tests on two sidecar combinations with a Lanchester accelerometer of the autographic type showed that the vibration present on a motor cycle seriously interfered with the correct functioning of the instrument.

The following alternative method of obtaining the desired information was therefore adopted. This method is simplicity itself, and requires no special instruments, since the whole of the figures required are obtained from an ordinary Bonniksen Isochronous Speedometer, manufactured by Messrs. Rotherham & Sons, Ltd., of Coventry. This instrument works on the following principle:—A gear wheel having fifty teeth is mounted on a shaft in such a manner that it can rotate with the shaft by friction through steel disks pressed up by a spiral spring, or it can be held in any definite position within the magnitude of one tooth, the shaft still revolving. The wheel is driven, through gearing, by the road wheel, and carries a small peg which comes against a stop when the pointer reaches zero. By this wheel, and by a duplicate of it, the recording hands are driven. The two hands record the speed alternately, the idle hand occupying the zero position. The pointer rests at zero until carried forward in the manner described below.

The speed record depends on two factors: (a) the rate of rotation of the fifty tooth gear, and (b) the length of time the gear wheel is allowed to rotate. The first factor depends entirely on the speed of the road wheel, the second on the operation of a watch escapement, itself actuated by the friction disks referred to above, and so arranged as to allow the pointer to rotate for exactly half a second, starting always from zero, and then to remain at rest for five seconds before returning to zero. Thus, if the road wheel is running at 50 miles per hour, the pointer goes once round the dial in half a second, but if the speed is only half this, the pointer only goes half round the dial in the half second available, and, therefore, stops at a reading of 25 miles per hour. When the hand has remained in position for five seconds the other hand comes up to position. Thus the instrument gives a speed reading every five seconds, this reading being the average speed of the vehicle over one half of a second, to the nearest mile per hour. For practical purposes it is sufficient to say that the instrument gives the speed of the vehicle every five seconds. The indicating hand being definitely locked in position for five seconds, it is perfectly easy to read, there being no possibility of error through vibration or swinging of the indicating hand. When carrying out the tests, the machine is taken to a perfectly level stretch of road on a substantially windless day. It is then accelerated up to its maximum speed, and the gear lever slipped into neutral, the successive readings of the speedometer as the machine comes to a standstill

being noted. To avoid the possibility of error, at least two runs in each direction are made, and averages taken for the final result.

The following figures were obtained in this manner on a 6/7 h.p. B.S.A. sidecar combination, fitted with a windscreen of the Easting type which was thrown right up into the storm protection

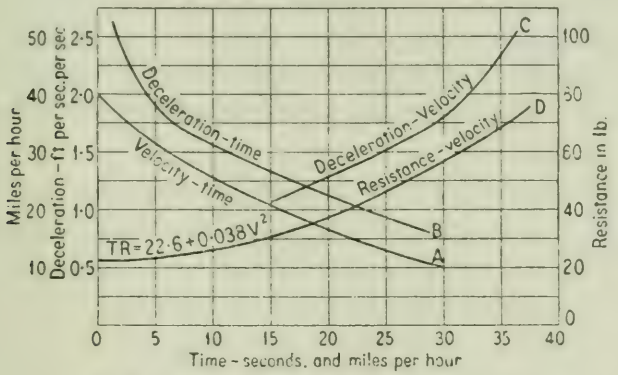


FIG. 2.—6 h.p. B.S.A. Combination.
Easting type screen—right up.
Total weight, 950 lb.
Road—perfect tarmac—dry.

position. The total weight of the outfit loaded, including an allowance for the inertia of the wheels, was 950 lb., and the tests were made on a stretch of perfect tar macadam. Successive readings of the speedometer as the machine slowed down were as follows:—40, 31, 25, 21, 17, 13 and 10 miles per hour. These are plotted

TABLE I.

Speed. Miles per hour.	Resistance in lb. from Curve.	Resistance in lb. $R = 22{\cdot}6 + 0{\cdot}038v^2$.	Error per cent.
15	31·0	31·0	0
20	37·6	37·6	0
25	45	46·1	2·4
30	53·1	56·4	6·2
35	68·6	68·7	0·15

in the lower left-hand curve A in Fig. 2 on a time base. Now it is evident that the slope of this velocity-time curve will give the deceleration-time curve, which is the upper left-hand line (B) in Fig. 2, and from the two curves it is a simple matter to draw the deceleration curve on a velocity base (upper right-hand line

(C) in Fig. 2). This is the exact equivalent of the accelerometer curve given in Fig. 1. In the particular case under consideration, this curve, when measured off by the correct scale, gives the values for tractive resistance stated in the second column of Table I. Examination of these figures shows that the curve is substantially that represented by $Tr = 22{\cdot}6 + 0{\cdot}038\,v^2$, the figures given by this equation being given in the third column, while the fourth column gives the percentage error. These errors are of such a small order that it is evidently justifiable to say that the tractive resistance of this machine is adequately represented by the equation given above.

The author suggests that this method of obtaining the tractive resistance curve is probably quite as accurate as the accelerometer method, since it involves the reading of one instrument only, and that instrument gives absolutely dead beat readings. It is true that a little graphical work is required, but this is of a very simple nature, and the disadvantage is quite outweighed by the absence of the necessity of purchasing a special instrument for the work.

Since any determination of tractive resistance must of necessity involve a knowledge of the weight of the vehicle concerned, it will, perhaps, be as well to give a few particulars of the weights of modern motor cycles before proceeding further. For this purpose the author has analysed the figures given in the reports of the 1919 and 1920 A.C.U. Six Days' Trials, which figures include oil, petrol, and all kit. Little difference is to be observed between the figures for the two years, so that the weights actually given below are those for the 1920 Trial only. Arbitrary divisions have been made for the purpose of obtaining averages, these divisions corresponding roughly to distinct alterations in weight.

Type of Machine.	No. of Machines averaged.	Average Weight in lb.
Solo up to 275 c.c.	14	198
,, 275 to 350 c.c.	14	229
,, 350 to 600 c.c.	49	297
,, 600 to 1000 c.c.	13	334
Sidecar up to 650 c.c.	7	460
,, 650 to 1000 c.c.	26	578

It may be remarked that in the 350 cc. class the Blackburne engined machines were heavy, at about 252 lb. against an average

of 229 lb., while the 3½ h.p. Precision machines in the same class weighed 287 lb., and have, therefore, been omitted from the average. The heaviest solo machine was the solitary 7-9 Indian, which weighed no less than 420 lb., or exactly the same as the A.B.C. sidecar outfits. The 4 h.p. single-cylinder solo machines were very consistent, there being practically no variation between half a dozen makes. For instance, a 4¼ h.p. B.S.A. machine weighed in at 280 lb., exactly the same figure as several 4 h.p. Triumphs, which is surprising in view of statements sometimes made that B.S.A. machines are heavy. Phelan & Moore machines varied from 280 to 308 lb., while the 3½ h.p. Sunbeam machines were all round about 310 lb. Scott machines weighed 280 lb., like Triumphs, while the solo A.B.C. machines averaged 290 lb. Among the big sidecar machines Enfields were noticeable for their light weight, 532 lb., while the Matchless were heavy at an average of 651 lb. The effect of the spring frame is noticeable, for the 8 h.p. Clyno was similarly heavy at 644 lb., while the A.J.S. outfits averaged about 580 lb.

Since certain machines were entered both solo and with a sidecar, it is possible to estimate the sidecar weights, which come out as follows:—

A.B.C. sidecar	130 lb.
Dunelt ,, ...	161 lb.
Humber ,, ...	156 lb.
Norton ,, ...	152 lb.
Rover ,, ...	156 lb.

These figures may be compared with the following figures for standard 1921 B.S.A. machines, without accessories, fuel or oil.

4¼ h.p. B.S.A. Chain-Drive solo.....	262 lb.
4¼ h.p. ,, Belt-Drive solo	255 lb.
4¼ h.p. ,, Sidecar body and mudguard	101 lb.
4¼ h.p. ,, ,, chassis, springs, wheel and tyre..	55 lb.
4¼ h.p. ,, ,, complete.....	156 lb.
4¼ h.p. ,, Chain-Drive combination complete	418 lb.
4¼ h.p. ,, Belt-Drive ,, ,, ...	411 lb.
6 h.p. ,, Solo	350 lb.
6 h.p. ,, Sidecar body	97 lb.
6 h.p. ,, ,, chassis, wheel, tyre and springs..	113 lb.
6 h.p. ,, ,, complete.....	210 lb.
6 h.p. ,, Combination complete	560 lb.

No comment on these figures appears to be called for, but it is, perhaps, desirable to give some idea of the weight distribution on a typical sidecar combination, since the weight available on the driving wheel is often the limiting factor in climbing freak hills. The following figures are for a $4\frac{1}{4}$ B.S.A. combination with an 11 stone driver and a 13 stone passenger:—

	Total.	Back Wheel. Weight.	per cent.	Front Wheel. Weight.	per cent.	Sidecar Wheel. Weight.	per cent.
Empty	504 lb.	203 lb.	40	182 lb.	36	119 lb.	24
Driver only.	658 ,,	318 ,,	48	227 ,,	34	113 ,,	18
Full Load..	840 ,,	350 ,,	42	276 ,,	33	214 ,,	25

It will be observed that with the outfit fully loaded, 42 per cent of the total weight is on the rear wheel. Therefore, if the coefficient of friction between the tyre and the road is 0·5, the tractive effort available is 21 per cent, or, in other words, the maximum grade which

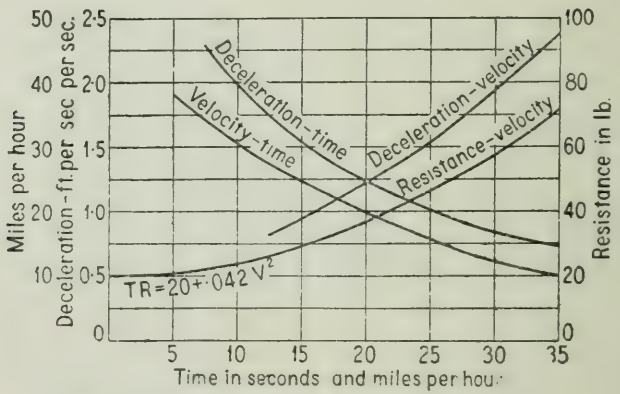


FIG. 3.—6 h.p. B.S.A. Combination.

No windscreen.
Gross weight, 950 lb.
Road surface—perfect.
$Tr = 20 + 0{\cdot}042\,v^2$.

can be climbed is about 1 in 5. The situation, however, is not so bad as this, because the effect of the gradient is to increase the rear wheel loading, and this tendency is still further helped by the driving torque, which is, of course, tending to lift the front wheel,

and consequently to increase the load on the rear wheel. When it is remembered, however, that a really greasy surface probably gives a coefficient as low as 0·1 between the tyre and the road, it is easily seen that the proportion of total weight on the rear wheel is a most important matter. For ordinary use, nevertheless, a load distribution such as that given above is quite satisfactory

To return to the question of tractive resistance, Figs. 3, 4 and 5 represent the results obtained from a series of tests on a 6 h.p.. B.S.A. combination, which, with driver and passenger, and an allowance for the inertia of the rotating parts, had a gross weight of 950 lb. The tests were made on a perfect Tarmac surface under ideal conditions. The sidecar was fitted with a windscreen of the

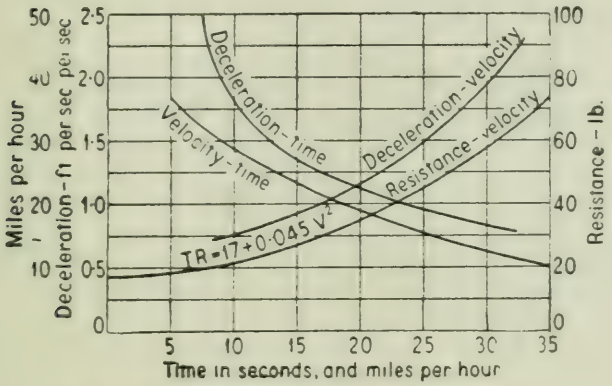

Miles per hour
Deceleration - ft per sec per sec
Resistance - lb.
Deceleration - time
Velocity - time
Deceleration - velocity
Resistance - velocity
TR = 17 + 0·045 V²
Time in seconds, and miles per hour

FIG. 4.—6 h.p. B.S.A. Combination.
Easting type screen in normal position.
Gross weight, 950 lb.
Road surface—perfect.
Tr = 17 + 0·045 v^2.

Easting type, and tests were made with the screen removed, with the screen in the normal position, and with the screen thrown right up, in order to determine what effect, if any, the screen had on the wind resistance. Figs. 3, 4 and 5 represent the conditions in the order stated, the resistance curves being—

$$\text{Tr} = 20 - 0{\cdot}042\ v^2$$
$$\text{Tr} = 17 - 0{\cdot}045\ v^2$$
$$\text{Tr} = 22{\cdot}6 + 0{\cdot}038\ v^2.$$

These three resistance curves are plotted together in Fig. 6 for purposes of comparison. It is notable in the first place, that there is a considerable variation in the constant term. This is difficult to explain, but it has been noticeable in all the tests which have

been made, and the author is inclined to think that it is due to the fact that the Bonniksen instrument reads only to the nearest mile per hour. The possible error thus introduced is unimportant at relatively high speeds, but may be of sufficient importance at low speeds to affect the constant factor. An independent check is easily made by finding the friction angle of the outfit, that is, the slope on which it will just keep rolling at a steady speed without either accelerating or decelerating. In this case it was found that 20 lb. was the correct figure. The equations may therefore be re-stated as—

No screen ..... $Tr = 20 + 0{\cdot}042\,v^2$
Screen normal ..... $Tr = 20 + 0{\cdot}045\,v^2$
Screen up ..... $Tr = 20 + 0{\cdot}038\,v^2$

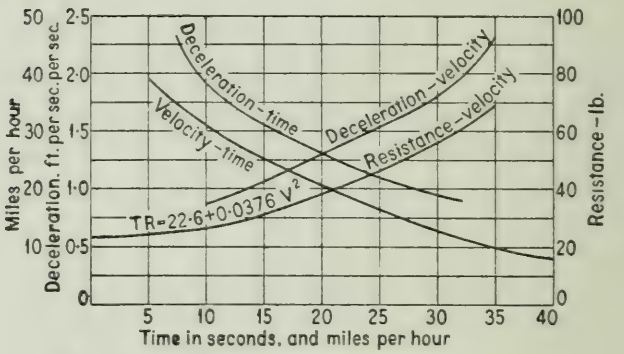


Fig. 5.—6 b.p. B.S.A. Combination.

Easting type screen right up.
Gross weight, 950 lb.
Road surface—perfect.
Tr. = 22·6 + 0·0376 v^2.

The second point worthy of note is the very small effect of the screen on the v^2 term, which shows that a curved screen of the Easting type has very little effect at all on the wind resistance. Actually, the resistance is slightly less when the screen is thrown right up than when no screen is fitted, which is to be expected in view of its streamlining effect. With the screen in the ordinary position the resistance is slightly increased, but only to a negligible extent. These statements, of course, apply only when there is a passenger in the sidecar. When no passenger is carried the screen will slightly increase the resistance in all positions. It may further be noted that the constant term of 20 lb. is rather high. This is due partly to the fact that the machine was fitted with 28 in. × 3 in.

tyres run rather soft for the sake of comfort, and partly, perhaps, to the unusually large diameter of the bearings in the interchangeable wheels.

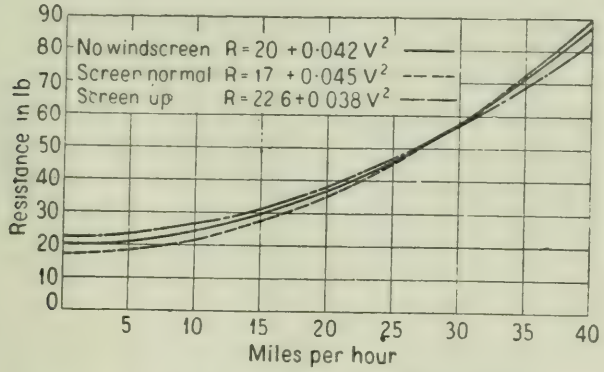


FIG. 6.—6 h.p. B.S.A. Combination.

Resistance velocity curves.

A similar series of tests was made on a 10 h.p. four-cylinder Henderson combination, fitted with a coach-built sidecar, the gross weight being 850 lb. In addition to the

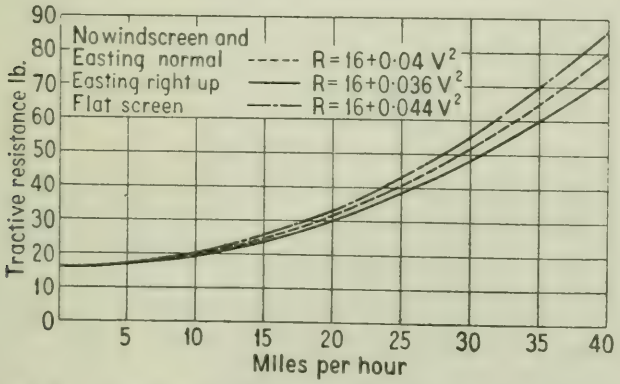


FIG. 7.—10 h.p. Henderson Combination (coach-built sidecar).

Tractive resistance curves.

Gross weight, 850 lb.

Road surface—perfect.

tests without a screen and with an Easting screen in the two positions, a test was made with a flat screen 18 in. high × 24 in. wide, set at about 70 degrees to the horizontal. The resulting

resistance curves are plotted in Fig. 7, and give the following results:—

No screen ..... $R = 16 + 0{\cdot}04\,v^2$
Easting screen, normal position ..... $R = 16 + 0{\cdot}04\,v^2$
„ „ , right up ..... $R = 16 + 0{\cdot}036\,v^2$
Flat screen ..... $R = 16 + 0{\cdot}044\,v^2$

It will be noted that in this case the addition of the Easting screen made no difference whatever to the resistance when set in the normal position, but that when right up it decreased the resistance as in the previous case. The use of a flat windscreen, however, increased the v^2 term by exactly 10 per cent, as would be expected. At 40 miles per hour the tractive resistance with

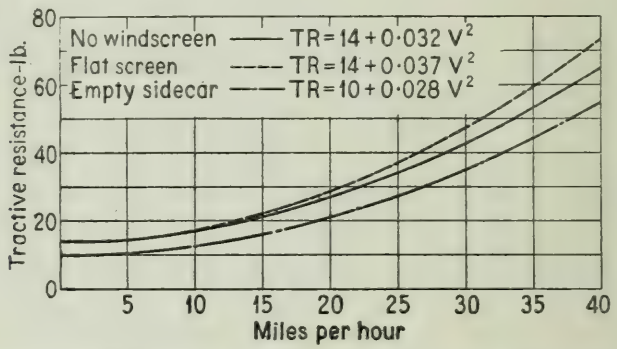


FIG. 8.—10 h.p. Henderson Combination (cane sidecar).

Tractive resistance curves.
Gross weight, 780 lb.
Road surface—perfect.

the flat screen is 12·7 lb. more than with the Easting screen in the best position, which represents a difference of no less than 1·35 h.p. in the power required to propel the machine.

The sidecar was then removed and replaced by a smaller cane sidecar, the gross weight of the outfit then being 780 lb. Tests were made with no screen, with a flat screen as before described, and also with the sidecar empty and covered by a tightly stretched apron so as to streamline it as far as possible. The resulting curves are shown in Fig. 8, and give results as follows:—

No screen ..... $Tr = 14 + 0{\cdot}032\,v^2$
Flat screen ..... $Tr = 14 + 0{\cdot}037\,v^2$
Sidecar empty ..... $Tr = 10 + 0{\cdot}028\,v^2$

It may be noted here that the reduction in weight from 850 lb.

to 780 lb. gives a reduction of 2 lb. in the constant term, while the further reduction of 140 lb. in weight due to the absence of the passenger reduces the constant by a further 4 lb. This would appear to indicate that the constant might be taken at 2 lb. for every 70 lb. of weight. If this were the case, however, the constant term for the outfit at 850 lb. would be 24·3 lb. instead of the actual term of 16. Before any definite decision as to the relation of this constant term to the total weight can be come to, therefore, it is evident that further experiments must be made with varying loads on the same machine. Unfortunately, the author has not, as yet, been able to find the necessary time to carry out these experiments.

As would be expected, there is a considerable reduction in the

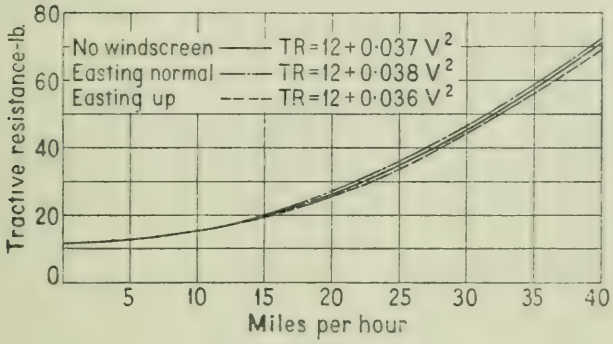


FIG. 9.—$4\frac{1}{4}$ h.p. B.S.A. Combination.
Tractive resistance curves.
Gross weight, 780 lb.
Road surface—perfect.

v^2 term when no passenger is carried, though the decrease is not so great as the increase caused by the use of a flat screen. Between the extreme conditions there is a difference in tractive resistance at 40 miles per hour of 18·3 lb., or no less than 1·96 h.p.

A $4\frac{1}{4}$ h.p. B.S.A. combination gives very similar figures, the resistances under various conditions being as follows:—

No screen ..... $Tr = 12 + 0{\cdot}037\,v^2$
Easting, normal ..... $Tr = 12 + 0{\cdot}038\,v^2$
Easting, up ..... $Tr = 12 + 0{\cdot}036\,v^2$

The weight of this outfit was 780 lb., and the wind resistance perhaps rather less than usual for that type of combination since the sidecar body was set very close in to the machine. The same machine was tested solo, with the rider sitting upright and then

with head down in as near a racing posture as possible in view of the fact that the speedometer had to be kept in view. Resistances were as follows:—

Sitting up ..... $Tr = 8 + 0{\cdot}02\,v^2$
Semi T.T. ..... $Tr = 8 + 0{\cdot}015\,v^2$

These figures are plotted in Figs. 9 and 10 respectively.

In Table II. the whole of the resistance figures which the author has obtained so far are tabulated, those marked with an asterisk having been deduced in each case from the performance of a machine of which the engine torque curve is known, while the remainder have been obtained direct by the deceleration tests as described above.

Table III. gives a summary of the results in general terms, and covers all types and sizes of machines. In the case of sidecar

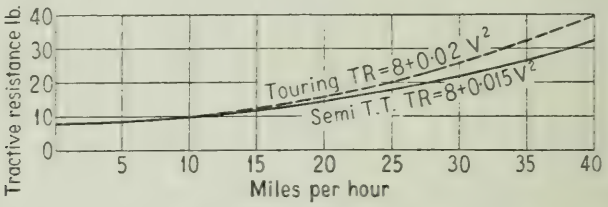


FIG. 10.—4¼ B.S.A. Motor cycle (Solo).
Tractive resistance curves.
Gross weight, 440 lb.
Road surface—perfect.

combinations, the projected area of the outfit, with driver and passenger seated, is given in order that the comparative sizes of the three classes for which figures are given may be realised. While, of course, these figures are not likely to represent very accurately the tractive resistance of any particular machine, yet the author feels sure that they are good average figures which may fairly be used for any analysis of the performance of a machine.

ENGINE PERFORMANCE.

It is not an easy matter to obtain a representative set of power curves which will cover all sizes of engines. Some of the curves considered in the paper have been obtained by the author himself on a Froude Dynamometer, but for others he is indebted to various sources, including the technical press, and he is, therefore, unable to vouch for the accuracy of all the figures, though he has no reason

TABLE II.

Machine.	Condition.	Tractive Resistance (Tr.).
6 h.p. B.S.A. Sidecar	No Screen	20 + 0·042 v^2
,, ...	Screen Normal	17 + 0·045 v^2
,, ...	,, up	22·6 + 0·038 v^2
4¼ ,, ...	No Screen	12 + 0·037 v^2
,, ...	Screen Normal	12 + 0·038 v^2
,, ...	,, up	12 + 0·036 v^2
Henderson & Coach Sidecar	No Screen	16 + 0·04 v^2
,,	Screen Normal	16 + 0·04 v^2
,,	,, up	16 + 0·036 v^2
,,	Flat Screen	16 + 0·044 v^2
Henderson & Cane Sidecar.	No Screen	14 + 0·032 v^2
,,	Flat ,, ...	14 + 0·037 v^2
,,	Empty Sidecar	10 + 0·028 v^2
4¼ h.p. B.S.A. Solo	Sit up.....	8 + 0·02 v^2
,, ...	Semi-T.T.	8 + 0·015 v^2
*Lightweight Solo	Sit up.....	6 + 0·015 v^2
* ,, ...	Semi-T.T.	6 + 0·012 v^2
*3½ Norton Solo	Brooklands condition.	6 + 0·01 v^2
*3½ Sunbeam Solo.....	,, ,,	6 + 0·01 v^2
*2¾ A.J.S. Solo	,, ,,	6 + 0·009 v^2
*2¾ ,, Sidecar	,, ,,	8 + 0·018 v^2
*7 h.p. Indian Sidecar	,, ,,	10 + 0·02 v^2

TABLE III.

Machine.	Windage.	Tr.
Large Sidecar Outfit.....	20 sq. ft.	20 + 0·042 v^2
Medium ,, ,, ...	18 ,,	16 + 0·04 v^2
Small ,, ,, ...	16 ,,	12 + 0·037 v^2
Large Solo Touring	....	8 + 0·02 v^2
,, ,, Semi-T.T.	....	8 + 0·015 v^2
,, ,, Racing	....	6 + 0·01 v^2
Small ,, Touring	....	6 + 0·015 v^2
,, ,, Semi-T.T.	....	6 + 0·012 v^2
,, ,, Racing	....	6 + 0·009 v^2
Large Sidecar Racing	12 sq. ft.	10 + 0·02 v^2
Small ,, ,, ...	10 ,,	8 + 0·018 v^2

to think that any are far from the truth. In connection with this however, it must be remembered that air cooled engines are difficult to test on a dynamometer, because the power output varies somewhat with the cylinder temperature. A side-valve engine of normal design will often show a difference of 10 per cent in brake mean effective pressure between dead cold and steady hot running. By taking snap readings while the engine is cold, it is obviously possible to show a power curve which is considerably above what would be obtained were the readings taken with a hot engine. The author's method is to vary the velocity of the cooling air with the engine speed, and to wait until the engine has reached a steady temperature before taking any readings. Figures obtained in this

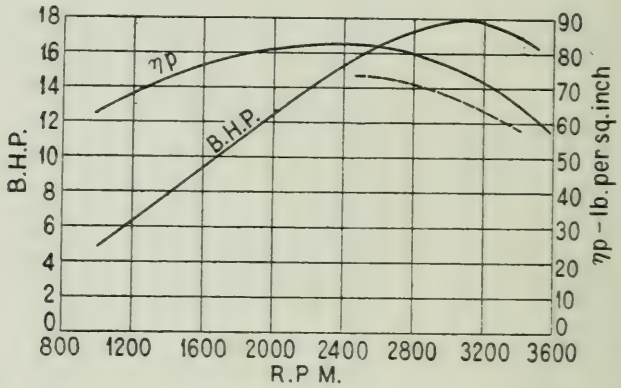


FIG. 11.—1918 Harley-Davidson 7-9 h.p. Engine.
Power curves.
Capacity, 989 c.c.
Compression ratio, 3·7 to 1.

way are evidently the only ones which are of any real value, and the point should always be borne in mind when power curves are analysed.

In Fig. 11 is given a power curve for a 1918 Harley-Davidson 7/9 h.p. twin-cylinder engine. It will be noted that the peak of the horse-power curve gives 18 h.p. at 3100 revs. per minute, while the maximum ηp is 83 lb. per sq. in. at 2400 revs. per minute. The dotted line is the ηp curve for a 1916 Henderson. This curve has been deduced from the known tractive resistance and the known maximum speed of the machine on various gear ratios. The method is so obvious that it requires no explanation. It must be mentioned, however, that the Henderson being a hub-geared machine, the tractive resistance figures refer to the back wheel,

since the gear lever was slipped into neutral for the deceleration tests. The Harley-Davidson curve, however, is taken at the crankshaft. If an allowance of 90 per cent is made for the efficiency of the Henderson transmission, which includes bevel gear, cross-shaft and chain, the Henderson curve approximates to that for the Harley-Davidson machine. Since the engines are of the same capacity, there is evidently nothing to choose between them.

Fig. 12 shows the results obtained from a standard 6 h.p. B.S.A. engine, 76 mm. bore × 85 mm. stroke, giving a capacity of 770 c.c. The maximum horse-power produced was 13·5 at 3200 revs. per minute, while the peak of the ηp curve is 82 lb. per sq. in. at 2000 revs. per minute. Thus the maximum results

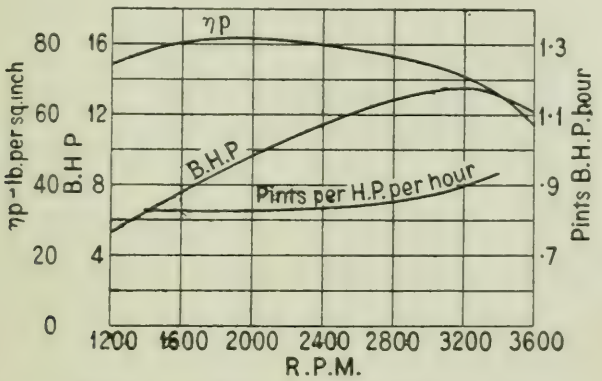


Fig. 12.—6 h.p. B.S.A. Engine.
Power curves.
Capacity, 770 c.c.
Compression ratio, 3·6.

obtained are substantially the same as in the case of the Harley-Davidson, but the B.S.A. engine is very much better than the other at lower speeds, and is, therefore, a better engine for touring purposes. The lowest curve is for petrol consumption with full throttle, in pints per brake horse-power per hour of No. 1 petrol using a B.S.A. carburettor.

The brake mean effective pressure curve shown in Fig. 13 is of an unusual shape and cannot be considered satisfactory. Since, however, it is drawn from the brake horse-power curve which was supplied by the makers of the well known 4 h.p. flat twin engine concerned, it must be assumed to be correct. This engine, in any case, cannot be accused of suffering from what is generally considered to be the failing of many flat twins, namely, inability to

pull at slow speeds. With the ηp curve still rising sharply at 1200 revs. per minute, it is evident that such an engine will be a slogger, but the speed droop will make it very bad for maximum power, which as a matter of fact only reaches 7·2 b.h.p. at 2600 revs. per minute.

The curves in Fig. 14 are for a 1920 type $4\frac{1}{4}$ h.p. B.S.A. single-cylinder engine. Curve A, shown as a full line, is for a standard engine with the usual compression ratio of 3·52 to 1. The test was made after the engine had been carefully tuned up to give the best results, the only alteration from standard being that a few holes had been drilled in the piston skirt. It will be noticed that the ηp is substantially constant at 83 lb. per sq. in.

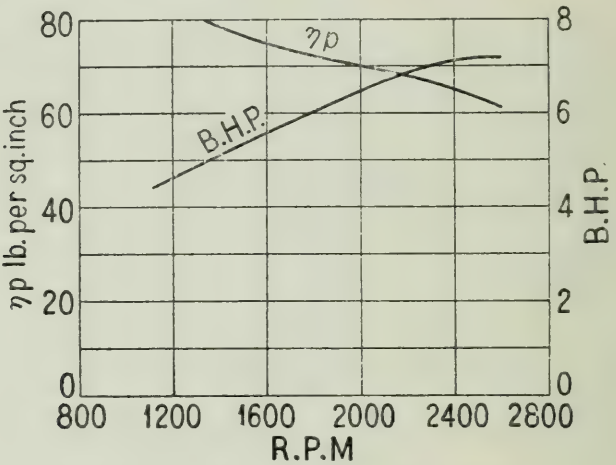


Fig. 13.—4 h.p. Flat Twin Engine.
Power Curves.
Capacity, 595 c.c.

from 1900 to 2500 revs. per minute. The maximum horse-power was 9·53 at 2900 revs. per minute. This engine was excellent on the road, the sidecar machine in which it was fitted being considerably faster than most outfits of similar size. After several thousand miles running, the engine was again tested, without being tuned up, and curves B were obtained, giving a maximum ηp of 76 lb. per sq. in. and a maximum brake horse-power of 7·6 at 2700 revs. per minute. Only a few hours' skilled attention was necessary to bring the engine back to its original condition. Curves C are for a similar engine with a dome-topped piston, giving a compression ratio of 4 to 1, and light reciprocating parts. Tuned up for sprint work, the engine gave an ηp of 93 lb. per

sq. in. and 12·25 brake horse-power at 3300 revs. per minute. It would not, however, stand prolonged runs at full throttle and low revolutions, and for that reason was not so suitable for sidecar work as the engine with 3·5 to 1 ratio. It should be remembered that both these engines are of the standard side-valve type, with cast-iron pistons and ordinary cams. For instance, the valve timing of the engine to which curves A and B apply was as follows:—

Inlet opens 8 degrees late, closes 55 degrees late.

Exhaust opens 57 degrees early, closes 14 degrees late.

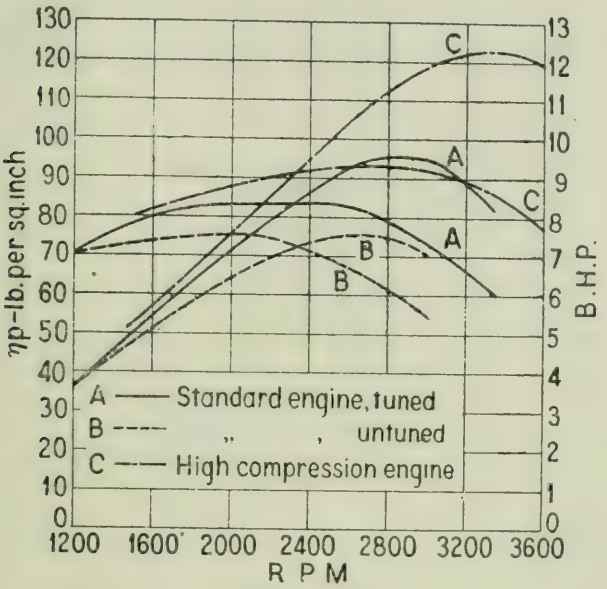


FIG. 14.—$4\frac{1}{4}$ h.p. B.S.A. Engine.

Power curves.

Capacity, 557 c.c.

Compression ratio, 3·6 to 1.

It may be mentioned that a bench test of another make of 4 h.p. single-cylinder engine 85 mm. bore × 97 mm. stroke equal to 550 c.c. capacity, gave a maximum ηp of 76 lb. per sq. in. and a maximum brake horse-power of 9·2 at 2900 revs. per minute, which is not quite so good as curve A in Fig. 14. With an open carburettor, the petrol consumption of the $4\frac{1}{4}$ B.S.A. engine with the standard compression ratio is about 0·9 pints per brake horse-power per hour. With the standard air gauze fitted it falls to 0·8 pints, and if a special hot air pipe be fitted it may even be

as low as 0·7 pints, all these figures, of course, being at full load with the carburettor jet wide open.

In Fig. 15 are curves for a 350 c.c. two-cycle engine of popular make. The horse-power curve A was issued by the manufacturers in their advertising matter as an indication of the results obtained. It will be noticed that the maximum brake horse-power obtained is 4·1 at 2400 revs. per minute, while the corresponding ηp curve, also marked A, rises to a maximum of just over 34 lb. per sq. in. at 2100 revs. per minute. This, of course, corresponds to about 68 lb. per sq. in. in a four-cycle engine of the same nominal capacity, which is probably as high as would be obtained from the average touring engine of that size. An unsatisfactory feature, however, is the extraordinarily rapid fall of the ηp curve

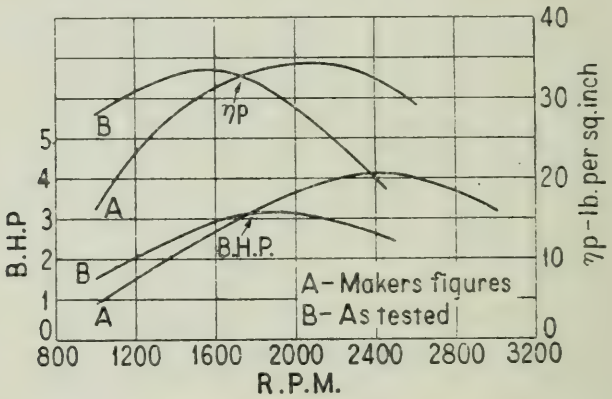


Fig. 15.—350 c.c. Two-cycle Engine.
Power curves.

on both sides of the maximum. The drop at higher revolutions merely means lack of maximum power, or inability to run fast, which is not a very serious fault for touring purposes. The rapid droop as the speed falls, however, is a most serious fault. The author was not surprised to find, therefore, that the engine was not really as bad as it was painted by its makers, for test of one which had done a thousand or so miles gave the results plotted in curves B in Fig. 15. It will be noted that these curves show a very much higher ηp at low speed; for instance at 1000 revs. per minute the figures are 16 lb. per sq. in. and 28 lb. per sq. in. On the other hand, the ηp falls off alarmingly at speeds over 1600 revs. per minute, with the consequence that the maximum brake horse-power reached was only 3·6 at 1900 revs. per minute.

Nevertheless, there is no doubt that the B curves represent a much better touring engine than the A curves issued by the manufacturers. As a matter of fact, the early speed droop on the B curves is probably due in part to the fact that the engine was tested in the condition in which it finished its road work, and the author has little doubt that reasonable attention would have improved the speed performances, though this might, perhaps, have been done at the expense of the lower end of the revolution scale. Unfortunately, no time was available for tuning up the engine to see what it would do at its best, so the curves in Fig. 15 must be left as they stand.

The author regrets that he has no power curve for a four-cycle engine of similar capacity, or for any two-cycle engines of the smaller class, such as the 211 c.c. There is no reason, however, to suspect that they differ in their specific performance from the larger engines which have been considered, except that it is possible that a trifling loss in ηp may be obtained by reason of the greater loss of heat to the walls in the smaller cylinders.

A few interesting figures are obtainable as a result of analysis of various speed performances, an assumed value being taken for the tractive resistance, on the basis of the figures given in Table III. For example, the record-breaking 7-9 h.p. Indian which, ridden by Le Vack, recently did 95 miles per hour on a declared gear of 3.1/16 to 1, must have been producing an ηp of 91 lb. per sq. in. at 3500 revs. per minute, an excellent figure for a side-valve engine, but not at all abnormal. The $3\frac{1}{2}$ h.p. Norton, with O'Donovan up, did 83 miles per hour on a gear of 3.11/16 to 1, which means an ηp of 99 lb. per sq. in. at 4000 revs. per minute. A $2\frac{3}{4}$ h.p. Douglas, with overhead valves and steel cylinders (pre-war type), was stated to give a maximum of 13 h.p. at 5000 revs. per minute, which means 96·5 lb. per sq. in. ηp at that speed. A $3\frac{1}{2}$ h.p. Sunbeam giving $14\frac{1}{2}$ b.h.p. maximum at 4250 revs. per minute shows 89 lb. per sq. in. These figures, of course, are not the maximums for ηp, since the maximum horse-power is not reached until the mean effective pressure has fallen off considerably.

To take a different type of engine, a $3\frac{3}{4}$ h.p. Scott was recently clocked at 69 miles per hour. Assuming the revolutions to be 2500 per minute, which is a reasonable figure for a two-cycle engine, the brake mean effective pressure would be 48 lb. per sq. in. on the gross volume, or 60 lb. per sq. in. on the nett volume,

assuming the latter to be 80 per cent of the gross. On the other hand, the pre-war Singer machine, which for a considerable time held the hour record in the 350 c.c. class at 62·68 miles per hour, was geared 4.7/8 to 1, and was, therefore, doing substantially 4,000 revs. per minute for the whole hour. This gives an ηp of only 71 lb. per sq. in. Although this seems low after the figures given above, it must be remembered that the higher figures are definitely for sprint engines, which probably could not maintain their power for an hour at 4000 revs. per minute. In this connection it may be noted that at the time of writing this record is

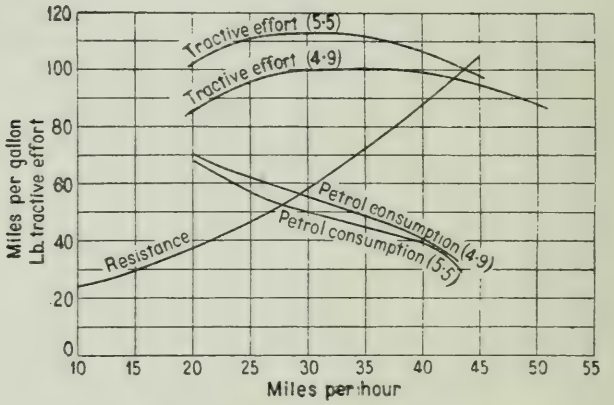


FIG. 16.—6 h.p. B.S.A. Combination.

Tractive effort and resistance curves.

No windscreen fitted.
Gross weight, 950 lb.
$R = 20 + 0{\cdot}042\,v^2$.
Gear ratio, 4·9 to 1 and 5·5 to 1.
Maximum speed by curves = $42\frac{3}{4}$ m.p.h. (4·9).
" " observed = 43 "
T.E. curve obtained from engine test 4-5-20, B.S.A. carburettor.

held by an overhead valve $2\frac{3}{4}$ Douglas, at 63·69 miles per hour, only one mile per hour faster than the little single-cylinder Singer with its side-valve cylinder.

Lastly, the little $2\frac{3}{4}$ h.p. A.J.S., which has made such sensational sprint performances, is said to give $13\frac{1}{2}$ h.p. on the brake. Unfortunately, the author has no idea of the speed at which this is developed, so it can only be said that if the speed is 5000 revs. per minute, the ηp is 100 lb. per sq. in., if 4500 revs. per minute, 112 lb. per sq. in., or if 4000 revs. per minute, it is 125 lb. per sq. in. In any case the figures are remarkable enough. As to whether they can be maintained for any but short periods the

author can only guess, but it is significant that up to the present only short distance records have been captured.

Road Performance.

In Fig. 16 the power curve given in Fig. 12 for a 6 h.p. B.S.A. engine has been converted into tractive effort curves, assuming the top gear ratios of 4·9 and 5·5 to 1, which are the extremes of the range fitted by the B.S.A. Co. as standard. The tractive resistance curve is also shown, and it will be observed that this cuts the tractive effort curve for the 4·9 gear at $42\frac{3}{4}$ miles per hour, and for the 5·5 gear at $43\frac{3}{4}$ miles per hour, these speeds being, of course, the maximum possible on a good level road with no wind. These speeds, as a matter of fact, agree almost exactly with the author's observations on the road. Lest it be thought that this particular combination is unusually slow, it may be as well to point out that it can only attain a maximum possible speed of 44 miles per hour, for, at this speed, the resistance horse-power is exactly equal to the maximum horse-power produced by the engine. To give this, a gear of 6 to 1 is required if no wheel slip is allowed for, so that in all probability 5·5 to 1 is about the fastest possible gear on the road.

The Harley-Davidson engine, the power curve for which is given in Fig. 11, with its maximum of substantially 18 h.p. at 3100 revs. per minute, would do just 51 miles per hour against the same resistance, and would need to be geared 5 to 1. Since it is improbable that any ordinary touring engine produces more power than this example of the Harley-Davidson, it may fairly be said that 50 miles per hour is about the upper limit of speed for a fully equipped touring combination with large sidecar and screen. Reduction of the v^2 term in the resistance formula will, of course, increase the maximum possible speed very considerably, so that a racing outfit with this Harley-Davidson engine would probably do about 67 miles per hour. The 50 miles record is at present held by a Harley-Davidson sidecar with a speed of 68·97 miles per hour.

To return to Fig. 16, the lower curves represent the probable petrol consumption of the B.S.A. 6 h.p. combination at various road speeds, as calculated from the bench tests and the tractive resistance curves. It will be noticed that the higher gear gives an advantage of about five miles per gallon over most of the range, although at the upper and lower limits the difference is not

so great. These consumption curves, which agree very well with road results, show the immense importance of keeping the speed low if petrol economy is an object, since while the consumption may be 68 miles per gallon at a steady 20 miles per hour, it increases to 50 miles per gallon with an increase of speed to 30 miles per hour, and to 39 miles per gallon if the speed be increased to 40 miles per hour.

The liveliness and climbing power of the machine is obviously represented by the space between the tractive effort and tractive resistance curves, and Fig. 17 shows this on a horizontal base. The ordinates to the curve in Fig. 17 represent the excess tractive effort available at the speeds concerned, and, therefore, represent

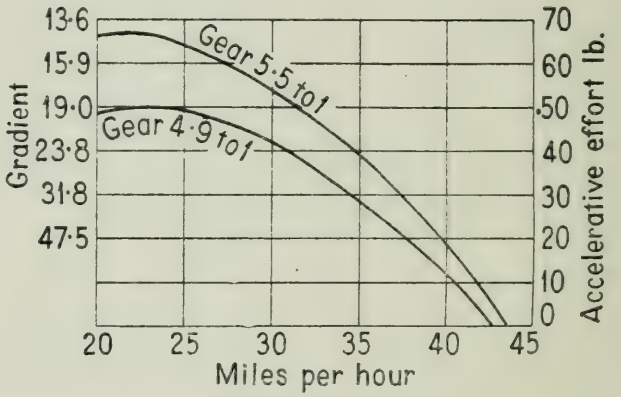


FIG. 17.—6 h.p. B.S.A. Combination.
Acceleration and climbing capacity.
Gross weight, 950 lb.
Curves obtained from T.E. curves dated 27-6-20.

the accelerative effort or climbing capacity of the vehicle at each speed. The considerable advantage gained by the use of the lower of the two gear-ratios is at once evident, since over the whole range there is very much better accelerative power available, together with an actual increase in the maximum speed. On the 4·9 gear, a steady gradient of 1 in 19 can just be climbed, while on the 5·5 gear the maximum is 1 in 14·2—a very great improvement, which more than outweighs the slight increase in petrol consumption.

The tractive effort curves on all three gears of the same combination are given in Fig. 18, together with the resistance curves on various gradients. The maximum gradient climbable on top gear is, of course, 1 in 14·2 as shown in Fig. 17, while on the second gear

1 in 8 is just possible, and on the first gear something steeper than 1 in 4 is covered. These figures refer to steady gradients which persist for a sufficient distance to destroy any momentum the machine may possess when it strikes the gradient. Very much steeper gradients may be climbed if the length of the steep pitch is short. It will be noticed that if maximum climbing speed is desired, it pays to change down from the top to the second gear at 30 miles per hour, and from the second to the first gear at about 20 miles per hour. In actual fact the machine will easily do 22 miles per hour on the first gear and 33 miles per hour on the second.

The performance of the $4\frac{1}{4}$ h.p. B.S.A. combination on all gears

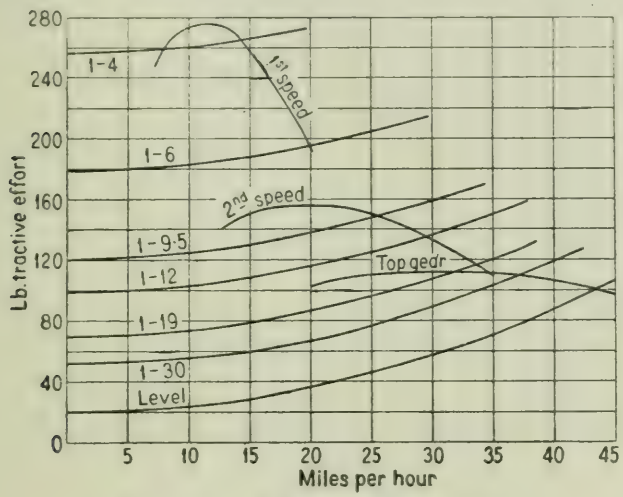


FIG. 18.—6 h.p. Combination.

Tractive effort and resistance curves.
Gear ratios, 5·5, 8·5, and 15 to 1.
Gross weight, 950 lb.
$R = 20 + 0{\cdot}042\,v^2$.

is described graphically in Fig. 19. The curves are plotted to show what the author calls the accelerative ratio on the three gears of 5·5, 8·8 and 14 to 1 respectively at all road speeds. The accelerative ratio is merely the accelerative force of Fig. 17 divided by the gross weight of the machine. For example, in the case of the $4\frac{1}{4}$ h.p. combination, reference to Fig. 9 will show that at 30 miles per hour the tractive resistance is 45·3 lb. The engine speed corresponding is 2150 revs. per minute, at which speed the engine is developing 83 lb. per sq. in. ηp, which means a tractive effort of 95 lb. at the rear wheel. Obviously, therefore, there is a surplus tractive effort of 49·7 lb. available for acceleration

or climbing, or for overcoming unfavourable road conditions. The ratio of this surplus tractive effort or accelerative force to the gross weight of the vehicle is 0·064, and it is this which the author calls the accelerative ratio. Evidently an accelerative ratio of 0·1 is equivalent to the ability to climb a gradient of 1 in 10, or to accelerate on the level at 3·22 ft. per sec. per sec.

Since the maximum accelerative ratio on top gear in Fig. 19 is approximately 0·08, the maximum gradient climbable is 1 in $12\frac{1}{2}$, while as the curve cuts the zero line at 43 miles per hour this is obviously the maximum speed. The second gear capacity is just short of 1 in 6, while on the first gear 1 in 3·2 is possible.

It will be seen, therefore, that on paper the performance of this

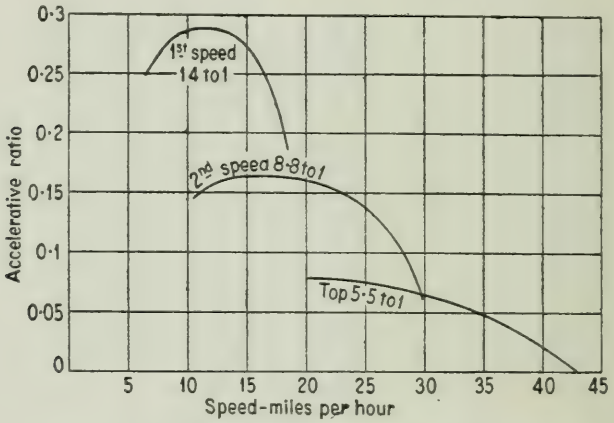


FIG. 19.—$4\frac{1}{4}$ h.p. B.S.A. Combination.
Acceleration curves.
Gross weight, 780 lb.

outfit is substantially the same as that of the 6 h.p. combination. This is due largely to the fact that the 6 h.p. outfit taken is unnecessarily heavily loaded, since its gross weight is 170 lb. greater than that of the $4\frac{1}{4}$ h.p., as against an actual difference in the unladen weights of about 100 lb. The larger machine, moreover, is capable of considerably more rapid acceleration on the road than the single-cylinder engine in spite of equality on paper, due largely to the more even turning moment of the two-cylinder engine.

The same machine solo is described by Fig. 20, in which the full line curves represent the performance of the machine on gear-ratios of 4·75, 7·6 and 12·2 to 1 respectively, the gross weight being taken to be 440 lb., and the rider presumed to be sitting

bolt upright in the conventional but decidedly uncomfortable touring position. The power curve used is the full line in Fig. 14, which represents the results obtained from the standard engine. The maximum speed under these conditions is 53 miles per hour, with a top gear capacity of 1 in 7·5 at just under 30 miles per hour. Second and first speeds will suffice for gradients of 1 in 3·8 and 1 in 2·2 respectively, so that it is evident that such a machine can never be stopped by gradient alone on any road in these islands provided a speed of about 10 miles per hour can be maintained. If the surface is so bad that 10 miles per hour is almost impossible, then the gear must be lowered, not to give a greater tractive effort but to make it effective at a lower road speed.

Returning to the top gear curves, the dotted curve represents

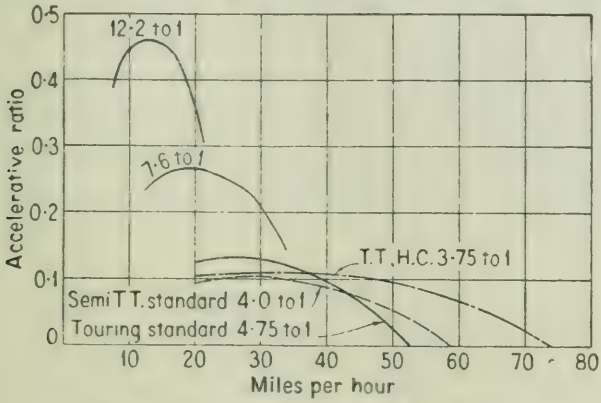


FIG. 20.—4¼ h.p. B.S.A. Solo.
Acceleration curves.
Gross weight, 440 lb.

exactly the same machine with the gear raised to 4 to 1, and the rider adopting a semi-T.T. position. This has the effect of pushing the maximum speed up to 59 miles per hour, and at the same time reducing the climbing capacity to 1 in 10 on the top gear. The chain curve is for the top gear performance of the high-compression engine of which the power curve is given in Fig. 14, with the rider in a full racing position and a gear ratio of $3\frac{3}{4}$ to 1. Note that the climbing and accelerative power remains excellent at all ordinary speeds, while the maximum speed possible has gone up to 74 miles per hour, assuming track resistances.

Similar curves may obviously be drawn for any other machines, provided that a power curve of the engine is available, but the author does not propose to pursue the subject farther, but to make a few remarks on the relative performances of cars and cycles.

Car *v.* Motor Cycle.

Taking first the engine, the motor cycle engine is, as a rule, of a somewhat lower compression ratio than the average car engine, and its ideal ηp is, therefore, correspondingly lower, while its ideal consumption is a trifle higher. In making comparisons, therefore, it is necessary to allow for this difference in compression ratio, and for this purpose the author has prepared the curves in Fig. 21, which show the variation of ideal ηp and consumption with varying ratios of compression. These curves are based on Ricardo's proposed standard of Attainable Ideal Efficiencies,* and while their exact accuracy may be of a doubtful order, their shape is certainly sufficiently nearly correct for purposes of comparison. From page 121 in the same volume the author has extracted particulars of a number of car engines, as given in Table IV., which shows the ratio of compression, the actual ηp developed, the ideal ηp from Fig. 21, and the percentage figure which is given by the ratio of actual to ideal. At the bottom are given figures for certain of the motor cycle engines already mentioned:—

Table IV.

Engine.	R.	Actual ηp. lb. per sq. in.	Ideal ηp. lb. per sq. in.	Percentage.
20 Daimler	4·6	88·7	118	75
6/30 ,, ...	4·6	90·7	118	77
4/30 ,, ...	4·5	87·8	117	75
6/45 ,, ...	4·5	87·5	117	74·8
16/20 Napier	4·5	83·0	117	71
A Wolseley	4·1	77·0	112	68·8
C ,, ...	4·1	81·0	112	72·3
M ,, ...	4·1	79·0	112	70·5
20 Vauxhall	4·5	85·0	117	72·6
20 ,, P.H.....	5·0	104·0	121	86
25 ,, ,, ...	4·5	90·0	117	77
$4\frac{1}{4}$ B.S.A. Standard	3·52	83·0	103	80·5
$4\frac{1}{4}$,, H.C.	4·0	93·0	110	84·5
6 h.p. B.S.A. Standard..	3·6	82·0	105	78·0
8 h.p. Harley-Davidson..	3·7	83·0	106	78·0

* See Proc. I.A.E., Vol. XIII., p. 120.

It will be seen from these figures that motor cyclists have no need to be ashamed of the performance of their engines. The motor cycle engines quoted are by no means of special design; on the contrary, three of the four are ordinary standard engines as sold to the public for touring purposes, yet their performance is better than that of any car engine in the list except the 20 h.p. Prince Henry Vauxhall, which was a special high-efficiency engine designed solely for use in a sporting car.

A comparison of petrol consumptions is more difficult, since the author has not the data respecting the performance of car engines to enable a real comparison to be made. One example may, however, be taken. In Mr. Pomeroy's last paper,* a large number of

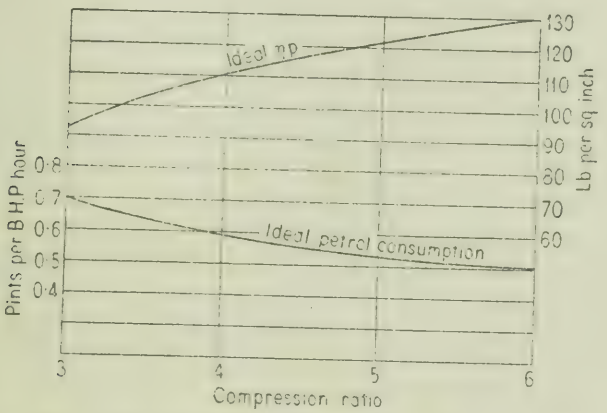


FIG. 21.—Ricardo's Ideal Engine Performance.
See Proc. I. A. E., Vol. XIII., p. 120.

petrol consumption figures were given, the best result obtained on the standard side-valve engine being 0·71 pints per brake horse-power per hour. This engine has a compression ratio of 4·8 to 1, its ideal consumption from Fig. 21 being 0·54 pints. The actual is, therefore, 1·32 times the ideal. The 6 h.p. B.S.A., on the other hand, has a compression ratio of 3·6 to 1, and its ideal consumption is, therefore, 0·63 pints against an actual consumption of 0·78 pints per brake horse-power per hour at the most economical point. The actual in this case is, therefore, 1·24 times the ideal. The $4\frac{1}{4}$ h.p. B.S.A. gives 0·8 pints against an ideal of 0·635, or a ratio of 1·26, while with a hot-air pipe fitted the consumption

* See Proc. I.A.E., Vol. XIII., p. 163.

actually falls to 0·7 pints per brake horse-power per hour, which is only 1·1 times the ideal.

From these few figures it does not appear that the petrol consumption of the motor cycle engine is other than good. It is certainly better than that of the car engine referred to above, when the variations in compression ratio are taken into account. Yet statements are frequently made to the contrary, and complaints have often been voiced in the technical press. Almost all of this dissatisfaction has been caused by the pernicious habit into which many people have fallen of talking of ton-miles per gallon. To use ton-miles per gallon as a standard of reference is always bad, and in the case of comparisons between such dissimilar vehicles as a motor cycle and a large car it is ridiculous.

Take for an example, a comparison between a 6 h.p. B.S.A. combination and a 20 h.p. Vauxhall car fitted with the engine referred to above. Mr. Pomeroy gives the weight of this car as 32 cwt., or 1·6 tons, and states that its petrol consumption is 24·7 miles per gallon at 20 miles per hour, and 23 miles per gallon at 30 miles per hour, so that this car gives 39·5 and 36·8 ton-miles per gallon at 20 and 30 miles per hour respectively. Reference to Fig. 16 will show that the B.S.A. outfit, weighing 950 lb. gross, gives 68 and 50 miles per gallon at 20 and 30 miles per hour, or 28·9 and 21·25 ton-miles per gallon respectively. These are definitely worse figures than those given by the Vauxhall car so far as the ton-miles per gallon basis is concerned. If the figures are converted into pints per brake horse-power per hour, the results are very different. Assume that the Vauxhall resistance is represented by $40 + 0{\cdot}07\,v^2$, which is reasonable, especially as it gives a maximum speed agreeing with Mr. Pomeroy's statements. Then the tractive resistances at the two speeds are 68 lb. and 103 lb., and the tractive horse-power is 3·64 at 20 miles per hour and 8·23 at 30 miles per hour. The consumption is, therefore, 1·78 pints per brake horse-power per hour at the lower speed, and 1·27 pints per brake horse-power per hour at the higher speed.

The sidecar outfit requires 36·8 lb. tractive effort to drive it at 20 miles per hour and 57·8 lb. at 30 miles per hour, the respective horse-powers being 1·96 and 4·62. The consumptions are, therefore, 1·2 and 1·03 pints per brake horse-power per hour, at the chosen speeds, which compare very favourably indeed with the 1·78 and 1·27 pints obtained on the car. While, therefore, a comparison on the basis of ton-miles per gallon shows that at 20 miles per

hour the car is apparently getting 1·37 times as much work out of a gallon of petrol as the sidecar, it is actually getting less than 70 per cent of the work when a correct analysis is made.

The reason for the failure of the ton-miles per gallon system as a basis of comparison is surely obvious. It is quite useless to compare the performance of different types of vehicles on a standard which acknowledges weight only, when the deciding factor in the resistance to motion is not the weight at all but the wind resistance. Until, therefore, a formula is devised which will incorporate the v^2 resistance term as a basis of comparison, the only reasonable method of making a comparison between dissimilar vehicles is that which the author has used above.

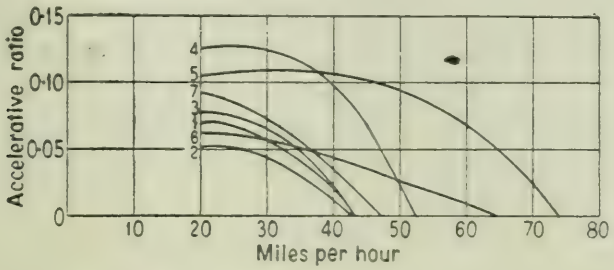


FIG. 22.—Accelerative ratio curves.

1. 6 h.p. B.S.A. Sidecar, $5\frac{1}{2}$ to 1 top gear.
2. 6 ,, ,, ,, 4·9 to 1 ,,
3. $4\frac{1}{4}$,, ,, ,, $5\frac{1}{2}$ to 1 ,,
4. $4\frac{1}{4}$,, ,, Solo, $4\frac{3}{4}$ to 1 ,,
5. $4\frac{1}{4}$,, ,, ,, racing.
6. Vauxhall car.
7. $3\frac{3}{4}$ h.p. Two-cycle solo, $3\frac{3}{4}$ to 1.

NOTE.—3 also substantially represents 6 h.p. B.S.A. with normal weight of 860 lb.

Similarly, it is useless to attempt to compare the "liveliness" of dissimilar vehicles by a comparison of the respective ratios of brake horse-power to weight, or by using such figures as the maximum tractive effort in lb. per ton. It is still more useless to make comparisons on the basis of thousands of litres per ton-mile, or similar figures, since such comparisons do not even take into account variations in ηp in different engines, in addition to the fact that they do not include any allowance for the v^2 term in the tractive resistance formula. The only reasonable means of comparing the performance of different vehicles is to draw the accelerative ratio curve for each vehicle, which, of course, neces-

sitates a knowledge of the engine performance as well as the tractive resistance curve in each case.

The curves in Fig. 22 show the "accelerative ratio" curves for a representative set of motor cycles, and also for the Vauxhall car which has been taken for comparison. It will be observed that five of the curves are of substantially the same form, but the curve No. 6 for the car, and No. 5 for the racing motor cycle, are entirely different in character. This, of course, is due to the fact previously mentioned, namely, that the weight of the car affects its figures at the lower speed, whereas its wind resistance is small comparatively, and, therefore, the tractive effort required at high speed is less in comparison than it is in the case of a motor cycle. The curve for the racing motor cycle is similar to that of the car because the wind resistance of the machine has been reduced to the lowest possible limits, and its behaviour is, therefore, more like that of a car than of an ordinary touring motor cycle.

The figures in Table V. represent the "thousands of litres per ton-mile" for each of the vehicles included in Fig. 22:—

TABLE V.

1 — 6 h.p. Sidecar combination	geared	$5\frac{1}{2}$	to 1 —	7·75
2 — ,, ,, ,,	,,	4·9	to 1 —	6·9
3 — $4\frac{1}{4}$,, ,, ,,	,,	5·5	to 1 —	6·83
4 — $4\frac{1}{4}$ Solo touring motor cycle	,,	4·75	to 1 —	10·5
5 — $4\frac{1}{4}$ Solo racing ,,	,,	3·75	to 1 —	8·36
6 — Vauxhall car	,,	$4\frac{1}{2}$	to 1 —	5·13
7 — $3\frac{3}{4}$ Twin two-cycle (semi-T.T.)	,,	$3\frac{3}{4}$	to 1 —	8·14

Comparison of these figures with the curves will show that the author's contention that any such basis of comparison is useless is fully justified. It will be seen that the car has the lowest figure of all, and yet is the second fastest vehicle, while the other figures bear very little relation to the real capabilities of the various machines on the road.

In conclusion, the author would like to point out that all the figures given for tractive resistance refer to good roads only, and that for bad road surfaces the constant factor must be largely increased. The author had originally hoped to give exact figures for the effect of road surface irregularities and for mud, and also to split up the v^2 term into its components of wind resistance and internal resistance. Unfortunately, however, the experiments

are not in a sufficiently advanced stage to be worth recording, and it is only possible to say that it appears that the wind resistance accounts for about 70 per cent of the v^2 term.

The author feels, moreover, that it is necessary to apologise for the rather scrappy nature of the paper, but he trusts that the fact that it contains, to the best of his knowledge, a good deal of information as to the performance of the modern motor cycle which has not previously been published, will atone for the many omissions.

THE DISCUSSION.

Mr. J. F. Henderson, in opening the discussion, said: It is certainly very satisfactory to see from Table IV. what high efficiencies have been got out of bicycle motors in comparison to the Air Standard Efficiency. Mr. Ricardo produced these curves to show how the efficiency of an engine could be improved by increasing the compression, and I think the author is making rather a virtue of a necessity in comparing the efficiencies on this basis, as of course the allowance for the comparatively low compression of the motor cycle engine makes it appear better than it would otherwise be. I agree with the author in his disapproval of a ton-mile basis as a standard for comparing the performance of vehicles unless they are fairly similar in type. After all, it is the distance which a gallon of petrol will carry a passenger which is the criterion of any passenger vehicle. I have, therefore, compared the various vehicles on this basis, with the following result:—

	Miles per gallon.	Passenger Miles per gallon (including driver).	Passenger Miles per gallon (excluding driver).
6 H.P. B.S.A. Combination at 30 M.P.H.	50	100	50
15·9 H.P. Hispano-Suiza at 23-24 M.P.H.	30·5	120	90
Ford	30	120	90
6 H.P. B.S.A. Combination at 15 M.P.H.	80	160	80
30 Seat Charabanc	8·8	265	255
6 H.P. B.S.A. Combination at 18 M.P.H.	75	150	75
15 Seat Charabanc	13½	200	190

On page 846 the author says: "The driving torque which is, of course, tending to lift the front wheels and consequently to increase the load on the rear wheels"; with this I entirely disagree. I find that quite a number of engineers seem to think that this is the case, but it is an absolute fallacy, as I think I can show. A body will remain at rest or at uniform velocity in a straight line unless acted upon by some external force; conversely, if a body moves with uniform velocity in a straight line, the sum of all the external forces acting on it must be zero. If a vehicle, there-

fore, is at rest on a perfectly smooth plane, either level or inclined, the only forces acting are the weight of the vehicle downwards through its centre of gravity, and the pressure of the road upwards at the points where the wheels touch the plane; the algebraic sum of these is zero. If we now consider the same vehicle travelling along the plane with a uniform velocity, the only additional forces excluding wind resistance, are a driving force acting on the rear wheel in the plane of the road and a resisting force at each wheel also in the plane of the road, and as these forces are in the same line and their sum is zero, since the velocity is constant, there is no further couple acting on the vehicle, and therefore the loading on the front and the rear wheel will remain the same as it was when at rest. Looking at it in another way, if the vehicle is accelerated, it is clear that there will be a further couple tending to lift the front of the vehicle, and when it is decelerated the rear wheel of the vehicle will tend to rise. This is because the centre of the inertia of the whole mass is above the surface of the road. It is clear, therefore, that if we reduce both the acceleration and deceleration to zero, that is when the velocity is uniform, the couple tending to rotate the vehicle will vanish.

If we consider a vehicle on rails, say with the centre of gravity in the plane of the rails, there will be no couple produced, either when it is accelerated or retarded, as all the forces, including those due to inertia, will be in the plane of the rails. Then, if we consider a vehicle with the centre of gravity below the rails, the back wheel will tend to lift when it is accelerated, and the front wheel will tend to lift when it is retarded. If the road is not a plain surface and the gradient suddenly changes, the plane of the road which touches the front wheel will not be in line with the plane which touches the rear wheel, and there will then be a free couple tending to lift either the back wheel or the front wheel off the road, depending on whether the road gets steeper or less steep.

In an ordinary vehicle, the centre of wind pressure is at some height above the road, and therefore produces a couple, the effect of which is to reduce the load on the front wheel and to increase the load on the back wheel, increasing as the speed increases.

It may be of interest to add that a vehicle running with uniform velocity along a road which is somewhat soft will usually be acted on by a couple slightly tending to throw more load on to the front wheel, which is exactly the opposite of what is usually

(Mr. J. F. Henderson.)

supposed. As shown in Fig. 23, a small wave will form on the road surface in front of each wheel, so that each wheel will be continuously running slightly up-hill. The propelling force will, therefore, be in some such direction as A C coincident with the surface of the road at the point A. Part of this A B is balanced by the retarding force on the rear wheel A R1. The remainder of it, B C, is balanced by the retarding force F R2 on the front wheel.

These are, however, not now in line, and form a couple having an arm R2 C. This couple tends to rotate the vehicle in an anti-clockwise direction, so throwing more load on to the front wheel.

Mr. J. Richardson: Anyone who has observed motor cycles travelling along our great highways must have come to the conclusion that their performances are really remarkable, and I

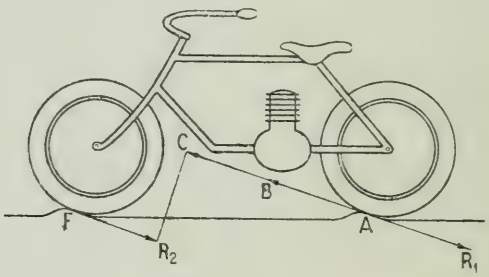


Fig. 23.

think that on the basis of the number of people carried, the motor cycle would compare very favourably with the charabanc, if it would not even surpass it. I am, therefore, not at all surprised on analysing the curves given in the paper (which are the first I have ever seen in connection with motor cycle and engine performance) to find that the efficiencies are of a remarkably high order.

I would have liked to have seen included some figures regarding piston speed, because surely the maximum revolutions possible with these engines is to some extent a function of the piston speed. I note the poor performance credited to the two-cycle engine. The brake mean effective pressure does not appear to rise in any case to above half of that achieved with the four-cycle engine. I would be glad if the author would enlarge upon this aspect of the subject and explain why two-cycle engines are fitted to motor

cycles if their performances are so poor as is indicated in the curves given. Not only do they not seem to have the same capacity for developing high speeds nor the same power at high speed, but lubricating oil and petrol consumptions are much higher than in the four-cycle engines. In spite of the instances of fine performances of the motor cycle, the author gave us clearly to understand that there is still open a large field for development. I do not understand why engines which give such high figures of output in sprint races cannot maintain these outputs, at all events approximately, over a much longer period than that of a sprint race. In other classes of engineering it is true that for purposes of trial an extra degree of efficiency over and above the normal can be attained by the prime mover, but the difference between short trials and normal running is not usually nearly so marked as the author gives us to understand in regard to motor cycles. I would confidently expect that if and when the results of the intensive research and experimental work which was conducted during the war on air cooled aero engines were assimilated and applied to motor cycle engines, the large gap indicated could largely, if not entirely, be bridged.

Mr. J. A. McGillivray: Nearly all the records have been made with long stroke engines, which appear to be directly against the case for low piston speed, although it is natural to assume that the limit of speed is reached at a certain piston speed. Regarding the charabanc and motor cycle comparison, I saw an instance the other day of a sidecar machine with five up. If we take the mileage per gallon at 75, we arrive at the figure of 59, which is as good as the charabanc. The author quoted a 4 h.p. machine represented to weigh about 200 lb. I saw one weighed recently minus lamp, horn and other unnecessary fittings and the weight came out at 197½ lb.

Mr. J. A. Martin: I quite agree with the author's conclusions with regard to the two-cycle engine. After running a light-weight two-cycle motor cycle for several years, I must say I have never found it altogether satisfactory. It pulled well at low speeds and climbed hills very easily, giving an even torque up to about 18 miles per hour, but above 25 the vibration became excessive and the power fell away considerably. This, I think, agrees with the torque and b.h.p. curves given by the author. Then the petrol consumption was poor compared with that of a four-cycle engine of equal rated horse-power: 97 miles per

(Mr. J. A. Martin.)
gallon was the best consumption I ever obtained. This was taken over a period of three weeks, during which the weather conditions were ideal, a definite known mileage being covered each day; under average running conditions, however, the consumption was between 60 and 70 miles per gallon. The engine was lubricated by the petroil mixture method, and though this is undoubtedly effective, the oil consumption was rather high for such a small engine. From the oily condition of the silencer and exhaust pipe it was evident that waste was caused by a portion of the oil going right through the engine.

Mr. J. Watt: The author has given us much useful information regarding acceleration and tractive effort, but he makes no mention of deceleration. I have yet to drive a sidecar combination incorporating a really efficient braking system. Apart altogether from the fact that the brakes themselves are very often not as powerful and reliable as they might be, only the rear wheel of the cycle is braked (the usual front brake may be ignored), and it would appear impossible to obtain sufficient adhesion from this one wheel. If the brake is really powerful, the rear wheel simply locks and skids. As an engineer, when driving a heavy sidecar combination, I would therefore feel much more confident if in addition to that on the rear wheel, a well-designed brake were applied to the sidecar wheel, although it must be admitted that this would be a very tricky brake to put into the hands of a novice. To put a direct question, could the author give any information regarding the maximum braking effort available with a 6/7 h.p. B.S.A. combination?

Like Mr. Richardson, I have never had any great admiration for the two-cycle engine, particularly as applied to motor cycles. The four-cycle engine has reached such a high degree of mechanical perfection that engine trouble is to all intents and purposes unknown, and the slight mechanical complication is well worth the extra production cost chiefly because the petrol and oil consumption is so much better. An ordinary four-cycle sidecar outfit properly lubricated gives from 1,500 to 2,000 miles per gallon of engine lubricating oil, and my own 6/7 h.p. B.S.A. sidecar machine shows a petrol consumption of 60·3 miles per gallon over 2,895 miles during a period of eight months, including losses due to short measure, evaporation and washing of parts. Comparing these figures with those quoted by Mr. Martin for a light two-cycle solo machine, it is evident that a big four-cycle engine pulling

up to eight cwt. has given on the road practically the same petrol consumption as a two-cycle engined machine having a gross weight of 300 or 400 lb. In connection with petrol consumption in general, to obtain reliable figures the measurement must be based on a reasonably large mileage, say 3,000 or 4,000 miles, and a point that must be borne in mind is that the total volume of fuel used over even this distance is, after all, comparatively small, so that any leakage in the petrol system has a proportionately big effect on the number of miles per gallon. I really believe many motor cycle engines showing an apparently heavy consumption are being debited with petrol which never reaches the carburettor jet, simply because 10 per cent of the fuel or even more is being lost at leaky cocks and unions. In comparing a motor cycle with a car, other factors besides petrol consumption must be considered. First cost, tyre wear, garage expenses, taxation and so on must be taken into account. It is very interesting for us as technical men to discuss miles per gallon and passenger-miles per gallon, but this does not go to the root of the matter, which from the user's point of view is, of course, running cost. No one can deny that a motor cycle and sidecar is the least expensive form of motoring: really it comes out cheaper than third class railway tickets.

I also would like to refer to the point brought up by Mr. Henderson. I must confess that until now my idea was that in ascending a hill the front wheel of a car or motor cycle tended to rise, due to torque reaction. If wind resistance is neglected, this would appear not to be the case so long as there is no change in velocity, and for those not mathematically-minded the matter can, perhaps, be explained in this way. Imagine two rails and on them a bogie with the top of the platform exactly on the plane of the rails: if a man were to lie on the platform and pull the bogie along by a rope, keeping the rope also on the plane of the rails, it is impossible to imagine any tendency for the front wheels to rise and still the bogie is being propelled. The next step is that, instead of hauling the rope by hand, the man utilises a windlass, the centre line of the rope being still in the plane of the rails. When he turns the handle of the windlass the bogie is again pulled along, and again it is not possible to imagine any tendency for the wheel to rise. The third step is to dispense with the rope. Imagine the windlass drum to be keyed to the axle of the wheels. The vehicle is then propelled by the adhesion of these wheels on the rails, and still there is no tendency to rise. The final step is that instead of

(Mr. J. Watt.)
having a hand-operated windlass fixed to the wheels, this is connected up to an engine and the power derived from that. The position of the centre of gravity of the complete bogie, engine and windlass does not enter into the question so long as there is no change in velocity and, as we are neglecting wind resistance, neither are the projected area nor the centre of pressure factors to be taken into consideration.

Mr. J. D. Parkes: I have been struck by the extraordinarily good case made out by the author for the motor cycle as a vehicle and for its power unit as an internal combustion engine. Compared with the results obtained from bigger engines, some of the figures he gives are astonishingly good, though I feel I must disagree with his method of comparing the performance of motor cycle engines with that of car engines. It is all very well to assert that the motor cycle necessarily runs at a lower compression ratio, and then to make an allowance in respect of it. If it is necessary to run at such a low compression the fact is a disadvantage, and the motor cycle engine should be debited with it, instead of which the author credits it with an allowance which I think rather questionable.

As regards the question of the lifting of the front wheel of a motor bicycle when moving at constant speed, I must admit that I held the popular opinion that the front wheel would lift, but after going into the matter fairly carefully I feel that this is incorrect, and, neglecting wind resistance, etc., the distribution of weight as between the two wheels remains constant, unless the machine is accelerating or decelerating.

As regards the point raised by several members in relation to the braking effort possible, it might be of interest to note that on a fully laden commercial vehicle of about 4 tons gross weight, on ideal road surfaces for adhesion, it is possible to obtain a decelerating force of up to 750 lb. per ton. This, however, is a maximum, because any further brake effort skids the wheel. Of course, on a motor cycle it might be impossible to reach such a figure as this because of loading difficulties.

Mr. Heather, in replying on the discussion, said: Mr. Henderson makes some interesting comparisons between motor cycles and other vehicles on the basis of passenger-miles per gallon. His figures, however, only prove my contention that any such basis of comparison is unfair when the comparisons are between

vehicles of different types. He compares motor cycles with charabancs. I might as well compare charabancs with railway trains, when, on the passenger-mile basis, the charabanc would certainly be inferior. The only fair basis of comparison is that of direct analysis, which I have adopted in my paper.

Mr. Henderson disagrees with my statement that the front wheel tends to lift when hill-climbing. He has, I think, convinced my intelligence, but he has not convinced my imagination, for I have on many occasions seen the front wheel of a sidecar combination actually lift off the ground on a freak hill. What caused that? There must be some force acting which Mr. Henderson and Mr. Watt have failed to take into account, and there is evidently room for further analysis of the question.

So far as we have gone at present, the piston speed does not seem to be a limiting factor in engine speed at all as stated by Mr. Richardson. The limiting factor is probably the pumping efficiency due to the fact that for dynamic reasons there is a limit to the port openings which can be obtained with poppet valves.

The two-cycle engine is used on motor cycles for two reasons. It is cheap to manufacture, and it provides a more even torque than a four-cycle engine with the same number of cylinders. The standard of performance on two-cycle engines as used for motor cycles is not high, and therefore any small firm can produce a two-cycle engine which will give average results. I believe, however, that there is a considerable field for the two-cycle engine if it is carefully studied, and that its petrol consumption can be made to compare favourably with that of the average four-cycle engine. A considerable amount of research is, however, required before this state of affairs can be reached.

The difference between the horse-power obtained for short sprints and that which can be maintained for long periods is largely due to heating of the new charge causing a reduction in the volumetric efficiency as the engine warms up. Since an air cooled engine reaches much higher temperatures than a water cooled engine, the effect is obviously more marked. Also pinking troubles have to be contended with, again due to temperature. I believe that during the next twelve months considerable advances may be expected in this direction, but it is hardly reasonable to expect that results as good as those quoted by Dr. Gibson in his paper will be obtained, since these refer to slow running

(Mr. Heather.)
aero engines cooled by a steady breeze. Our engines run three times as fast, and under much worse cooling conditions.

Several speakers asked for figures of braking effort. Under good conditions the rear brake on a sidecar combination should give 500 lb. per ton, which is quite as good as the average car. The figure given by Mr. Parkes is for a commercial vehicle with a large portion of its weight on the rear wheels. Our sidecar taxis are fitted with brakes on the sidecar wheel, but I should not care to fit such a brake as standard, as it is a very tricky thing to use. There is no reason, however, why a good front brake should not be fitted, which, in conjunction with the rear brake, would give about 800 lb. per ton, which is ample.

Mr. Parkes criticised the fact that in comparing car and cycle engines I allowed for the lower compression ratio used in the latter. I cannot see any alternative. I was dealing with engines as they exist to-day, and was therefore bound to take into account such a fundamental factor as the ratio of compression. Most of my figures are for ordinary touring engines as sold to the public. Had I dealt in detail with racing engines, using compression ratios even higher than those used by cars, the figures would probably have been rather astonishing, for the results obtained are certainly good.

WOLVERHAMPTON CENTRE.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

WOLVERHAMPTON CENTRE.

OFFICERS FOR SESSION 1920—1921.

Committee.

F. A. S. Acres. H. Stevens.
C. M. Van Eugen. O. W. J. Watson.

Hon. Secretary.

K. B. Miller.

The First Meeting of the Session was held at the Talbot Hotel, Wolverhampton, on Wednesday, October 6th, 1920, at 7.30 p.m., Mr. D. S. Heather being in the Chair. Mr. J. E. Greenwood read a paper on "The Racing Motor Cycle." There were present thirty-six Members and visitors.

The Second Meeting of the Session was held on Tuesday, November 2nd, 1920, at 7.30 p.m., Mr. H. Stevens being in the Chair. A paper was read by Mr. H. B. Benny on "The Influence of Detail Design on Production." There were present twenty Members and visitors.

The Third Meeting of the Session was held on Wednesday, December 8th, 1920, at 7.30 p.m., Mr. O. W. J. Watson being in the Chair. A paper was read by Mr. M. Tombs on "The Trend of Design as indicated by the Olympia Show." There were present twenty-one Members and visitors.

The Fourth Meeting of the Session was held at the Technical School on Tuesday, January 4th, 1921, at 7.30 p.m., Sir Henry

Fowler being in the Chair. A paper was read by Mr. H. Stevens on "The Problems of a Designer and how they affect Production." There were present one hundred Members and visitors.

The Fifth Meeting of the Session was held at the Talbot Hotel on Wednesday, February 2nd, 1921, at 7.30 p.m., Mr. A. E. Palmer being in the Chair. A paper was read by Mr. F. A. S. Acres on "Accessibility." There were present nineteen Members and visitors.

The Sixth Meeting of the Session was held on Wednesday, March 2nd, 1921, at 7.30 p.m., Mr. O. W. J. Watson being in the Chair. A paper was read by Mr. D. S. Heather on "Change Speed Mechanism." There were present eighteen Members and visitors.

The Seventh Meeting of the Session was held at the Technical School, Wolverhampton, on Wednesday, April 6th, 1921, at 7.30 p.m., Dr. Leslie Aitchison being in the Chair. A paper was read by Mr. David Flather on "Case-hardening." There were present seventy-five Members and visitors.

The Eighth Meeting of the Session was held at the Talbot Hotel on Tuesday, May 3rd, 1921, at 7.30 p.m., Mr. F. A. S. Acres being in the Chair. A paper was read by Mr. H. D. Teage on "Air Cooling as applied to Motor Cycles." There were present sixteen Members and visitors.

EPICYCLIC GEARING IN AUTOMOBILE PRACTICE.

Daimler Premium Paper—Session 1919–20.

Read by
D. L. PRIOR (Graduate),
Before the Coventry Graduates' Centre.

EPICYCLIC GEARING IN AUTOMOBILE PRACTICE.

By D. L. PRIOR (Graduate).

Part I.

Notes on Design.

The automobile engineer occasionally desires to avail himself of the advantages offered by epicyclic gearing under certain circumstances. When such occasion arises, the designer usually has in mind a certain ratio which he desires to adopt, and he is faced with the task of arranging his gears, and deciding upon his driving and driven members to give the required result. This in itself is not a simple problem, and is further complicated when two ratios are required with one set of gears. The many excellent methods given in text-books and the like for analysing existing gears are not as a rule convenient for use in a reverse direction, i.e., to obtain the proportions of the gears to suit a given ratio, and the author will attempt now to present the problem from this view-point.

There are, of course, a large number of possible combinations of epicyclic trains, but the author will restrict himself to the three most commonly used in motor car practice, namely, the simple epicyclic, the compound epicyclic, and the reverted train in which the internally toothed ring is replaced by a gear wheel in alinement with the first or sun wheel.

Simple Epicyclic Train.

A diagram of this gear is shown in Fig. 1, in which C represents the sun pinion, D the planet pinions, A the cage or member carrying the planet pinions D, and B the internally toothed ring. The gears are indicated by their pitch diameters, and it is this diameter which is referred to in all cases when a gear is mentioned by letter.

There are two methods of analysing an epicyclic gear train to which the author would draw attention. The first is given in "The Theory of Machines," by R. F. McKay, and the second

can be found in "Machinery's Handbook" and other works of reference. Both methods have similar characteristics, and can be used for any type of epicyclic gearing, no matter how complex. They also possess the advantage that they can be easily memorised. The six possible combinations of stationary, driving, and driven members are given in Table I., and are numbered 1 to 6. It will be obvious that case 1 is the inverse of case 2 since the stationary member is the same, and this is also true of cases 3 and 4, and also 5 and 6. The velocity-ratio of the first case will now be investigated by both methods.

Analysis of Simple Epicyclic Gearing.

1*st Method. Case* 1. Stationary member. Ring B.
Driving member. Sun pinion C.
Driven member. Cage A.

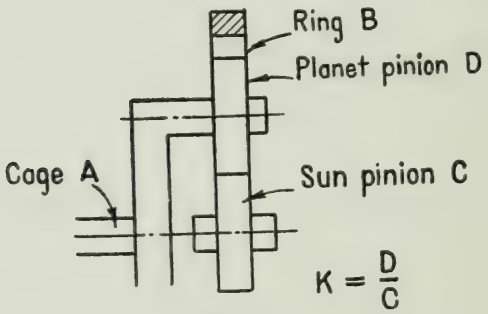


FIG. 1.

Since the relative motion between the component parts is the same, no matter which element is assumed fixed, it can be written down at sight by choosing judiciously the fixed element. By holding the cage A the gear resolves itself at once into a simple train, and by revolving any one of the other members the behaviour of the remaining elements can at once be noted. This gives us row (a), Table II.

These values in row (a) can have a constant added to each of them, or subtracted from them, or they can each be multiplied by a constant without altering their relative values, and they can therefore be manipulated until they show the conditions of the problem, namely, B stationary, C driving, A driven.

By adding C B to row (a), row (b) is obtained, and the relation between A and C can at once be seen, and also the fact that they

TABLE I.

Case.	(a) Stationary Member.	(b) Driving Member.	(c) Driven Member.	(d) Ratio.	(e) Substituting K	(f) Value of K
(1)	Ring B	Sun Pinion C	Cage A	$A_r = \frac{C}{B+C}$	$A_r = \frac{1}{2(K+1)}$	$K = \frac{1}{2A_r} - 1$
(2)	Ring B	Cage A	Sun Pinion C	$C_r = 1 + \frac{B}{C}$	$C_r = 2(K+1)$	$K = \frac{C_r}{2} - 1$
(3)	Cage A	Sun Pinion C	Ring B	$B_r = -\frac{C}{B}$	$B_r = -\left(\frac{1}{2K+1}\right)$	$K = -\frac{1}{2}\left(\frac{1}{B_r}+1\right)$
(4)	Cage A	Ring B	Sun Pinion C	$C_r = -\frac{B}{C}$	$C_r = -(2K+1)$	$K = -\frac{1}{2}\left(C_r+1\right)$
(5)	Sun Pinion C	Cage A	Ring B	$B_r = 1 + \frac{C}{B}$	$B_r = 1 + \frac{1}{(2K+1)}$	$K = \frac{1}{2}\left\{\frac{1}{(B_r-1)} - 1\right\}$
(6)	Sun Pinion C	Ring B	Cage A	$A_r = \frac{B}{B+C}$	$A_r = 1 - \frac{1}{2(K+1)}$	$K = \frac{1}{2}\left\{\frac{1}{(1-A_r)} - 2\right\}$

In using column (f) give A_r, B_r, C_r the correct sign as indicated in column (d).

both revolve in the same direction. If it is required to know the revolutions of A to one of C it is known that:—

When C revolves $1 + \frac{C}{B}$, A revolves $\frac{C}{B}$

$\therefore$,, C ,, 1 ,, ,, $\frac{C}{B} \times \frac{B}{B + C}$

$= \frac{C}{B + C}$

The rules governing this method are therefore quite simple.

1st. Assume the cage fixed, rotate one of the other members, and write down the resulting velocity-ratios.

TABLE II.

	A	B	C	D
(a) Cage A Fixed ..... C given + 1 turn	0	$-\frac{C}{D} \cdot \frac{D}{B} = -\frac{C}{B}$	+ 1	$-\frac{C}{D}$
(b) Add $+\frac{C}{B}$ to (a).....	$+\frac{C}{B}$	0	$1 + \frac{C}{B}$	$\frac{C}{B} - \frac{C}{D}$
(c) Reducing C to 1	$\frac{C}{B + C}$	0	1	$\frac{C(D - B)}{D(B + C)}$

2nd. Manipulate the results obtained so that the required member is fixed.

2nd method. Case 1, as before.

In this method each element is given a turn in the same direction, usually positive. The cage is then assumed to be held, and the element which is fixed is given a negative turn, the results of this negative turn on the other elements being noted under the appropriate headings. These two results are then added algebraically, the answer giving the required velocity-ratio. Taking case 1 again as an example, each element is given a positive turn, resulting in row (a), Table III.

The cage A is then assumed to be held, and B the stationary member rotated −1, thus giving row (b), Table III. Adding the

two gives row (c), from which it is seen that if C rotates $1-(B \div C)$ times, then A rotates once in the same direction.

If C rotates once, then A revolves the inverse of C, this being $C \div (B - C)$ as obtained by the first method.

It is sometimes stated that in using this method it is the driving member which is to be assumed stationary when obtaining row (b). It is important, however, to notice that in all cases the cage A must be taken as held, otherwise the whole simplicity of the Table is lost. In those cases where A is the driving member, the required ratio is given directly to one revolution of the cage, and when A is driven, as in the example above, it is quite a simple matter to invert if necessary. It will be seen, therefore, that in those cases where A is the stationary member, this method cannot be used, but it is really not necessary, since the required

TABLE III.

	A	B	C	D
(a) Each element + 1....	+ 1	+ 1	+ 1	+ 1
(b) A held B − 1.....	0	− 1	$+\frac{B}{D}\cdot\frac{D}{C}$	$-\frac{B}{D}$
(c) (a) + (b).....	+ 1	0	$1+\frac{B}{C}$	$1-\frac{B}{D}$

relative velocities can be written down at sight from the diagram. It is of the greatest importance, in both methods, to affix the correct sign of rotation to the results obtained.

As stated before, both methods are very similar, and perhaps the only advantage possessed by the first method is that all six cases can be written down in one Table, while by the second method it is better to have a separate Table for each case.

The other cases are dealt with in a similar manner, and will result by either method in column (d), Table I., where A_r, B_r, C_r are the revolutions of A, B and C to one revolution of the driving member, a minus sign indicating that the driven member revolves in the opposite direction to the driving member. For purposes of reference the full workings of each case are given in Appendix A.

Turning now to Fig. 1, it will be seen that if the proportions of two of the elements are decided upon, then the size of the third is necessarily fixed. Thus $C + 2D = B$.

$$\text{Let } \frac{D}{C} = K$$

$$\text{Then } 2K + 1 = \frac{B}{C}$$

$$\text{And } B = C(2K + 1)$$

Substituting in the first case, Table I., column (d)

$$A_r = \frac{C}{C(2K + 1) + C}$$

$$\therefore \quad A_r = \frac{1}{2(K + 1)}$$

$$\text{And } K = \frac{1}{2A_r} - 1$$

The other cases are dealt with in a like manner by substituting the above value of B in each of the remaining equations, resulting in columns (e) and (f), Table I.

In these two columns equations are now given from which can be found the velocity-ratio corresponding to a given proportion between the sun pinion and the planet pinion, or alternatively the diameters of these pinions necessary to give a certain velocity-ratio. Having found the relative proportions of the sun and planet pinions, it is an easy matter to find the diameter of the ring B from the preceding formula, $B = C + 2D$.

In Fig. 2 are plotted the equations given in row (f), Table I. From this graph can be read directly the gear-ratio resulting from any particular arrangement of the epicyclic gear for a given size of planet pinion, and it also shows clearly the limiting ratio for any particular combination. For example, it can be seen at once that there is only one arrangement of the elements which will give a ratio of exactly 2 to 1; this is case 4, in which the cage is stationary, a negative drive resulting in which the sun pinion (driven) revolves in the opposite direction to the ring (driving). A further use for the chart is in those cases where it is desired that one member shall always be driving, the other members being alternately held and driven, thus obtaining two velocity-ratios. A line drawn parallel to the base line through the selected value of K will cut the "ratio lines" at certain points, and lines dropped vertically from these points on to the base line will give the corresponding ratios. The chart can, of course,

also be used in the reverse direction; working from one ratio a

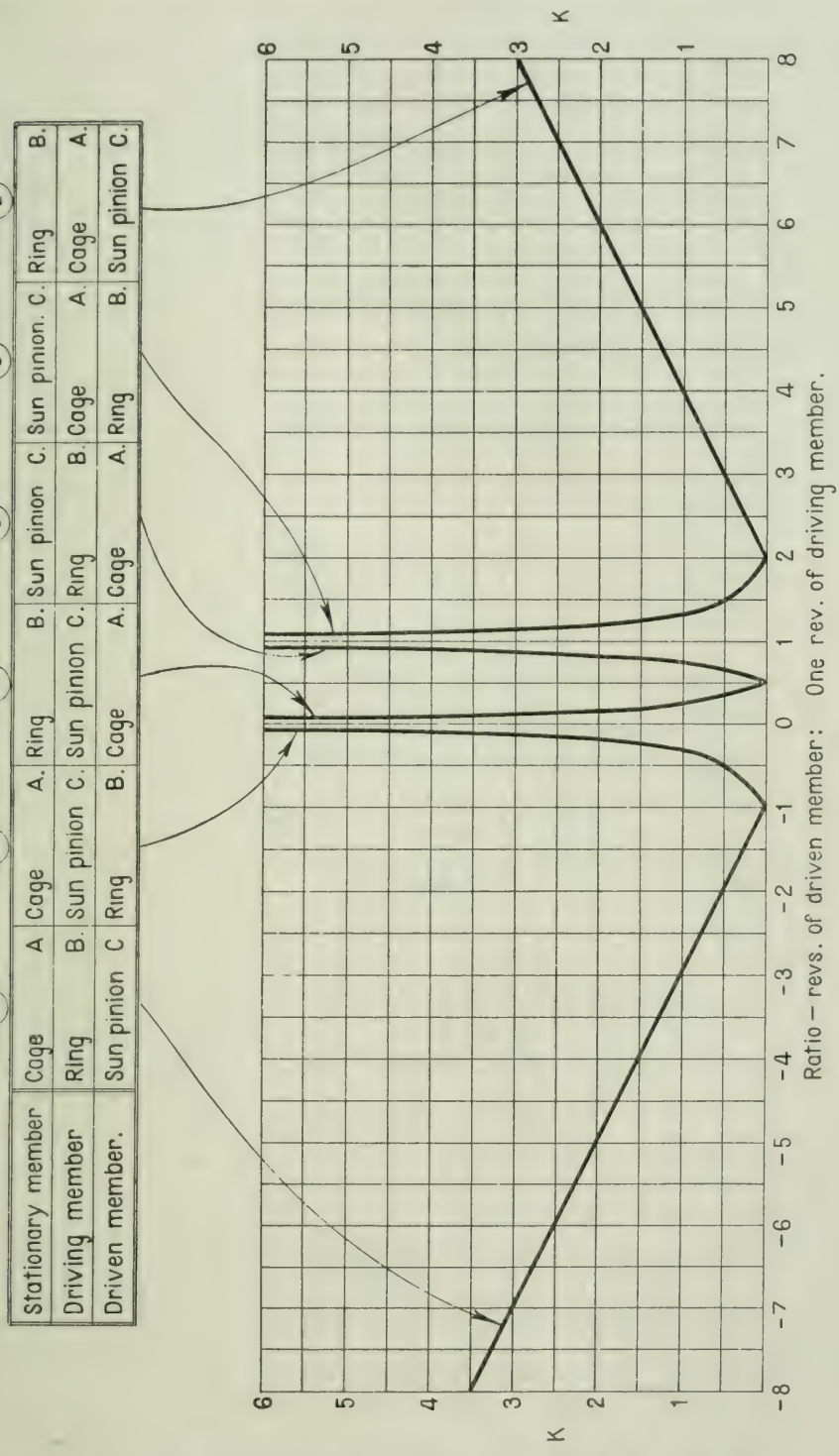

Stationary member | Cage | A | Cage | A. | Ring | B. | Sun pinion | C. | Sun pinion. C. | Ring | B.
Driving member | Ring | B. | Sun pinion | C. | Sun pinion | C. | Ring | B. | Cage | A. | Cage | A.
Driven member. | Sun pinion | C | Ring | B. | Cage | A. | Cage | A. | Ring | B. | Sun pinion | C.
4
3
1
6
5
2
K
Ratio – revs. of driven member: One rev. of driving member.
K

FIG. 2.

line is projected upwards from the base until it cuts the correct

"ratio line." and another drawn through this point parallel to the base will give the other gear-ratio and the necessary value of K to obtain these results. As an example, let it be supposed that the cage is to be the driving member in both cases, and that K is to be 0·75. A line drawn through $K = \frac{3}{4}$ cuts "ratio lines" for cases 2 and 5 at $R = 1{\cdot}4$ and $R = 3{\cdot}5$, and these are the two ratios possible under the conditions given when the various elements have the proportions:—$C = 1$, $D = \frac{3}{4}$, and $B = C + 2D = 2\frac{1}{2}$. Since the pitch is constant throughout, these figures are also proportional to the number of teeth in each gear, the actual pitch and number of teeth being selected to suit.

The range of Fig. 2 greatly exceeds proportions which can be used in practice; actually, the planet pinion will never be

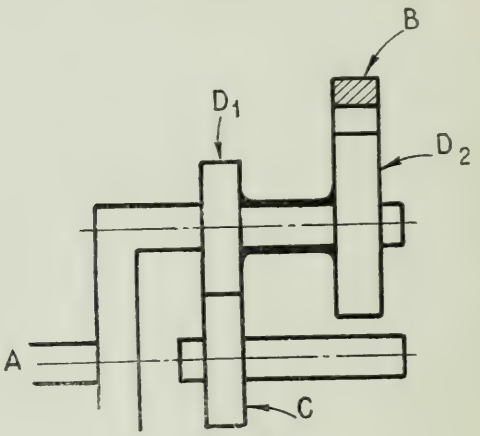


Fig. 3.

more than twice the diameter of the sun pinion except in special cases, and therefore all the ratios possible in practice are represented on the graph between lines drawn through $K = 2$ and $K = 0$.

Compound Epicyclic Train. (a) *With Internally Toothed Ring.*

The analysis of compound epicyclic gearing is carried out in exactly the same way as already described for the simple epicyclic gear. An actual example will perhaps make this perfectly clear, a reference to Fig. 3 indicating the notation employed. In this diagram A is the cage carrying the planet pinions D_1 and D_2, which are locked together so that they revolve as one but are free to rotate upon a spindle incorporated in A, the pinion D_1 engages with the sun pinion C, and D_2 with the ring B.

TABLE IV.

Case.	(a) Stationary Member.	(b) Driving Member.	(c) Driven Member.	(d) Ratio.	(e) Conversion Formula.	(f) Substituting G and K in (d).	(g) Value of K.
(1)	Ring B	Sun Pinion C	Cage A	$A_r = \frac{CD_2}{CD_2 + BD_1}$	$(1-(6))\left(\frac{1}{1-(4)}\right)$	$A_r = \frac{G}{(G+1)(K+1)}$	$K = \frac{G}{A_r\,(G+1)} - 1$
(2)	Ring B	Cage A	Sun Pinion C	$C_r = 1 + \frac{B}{C}\cdot\frac{D_1}{D_2}$	$\left(\frac{1}{1-(6)}\right)(1-(4))$	$C_r = \frac{K(G+1)+(G+1)}{G}$	$K = \frac{G(C_r - 1) - 1}{(G+1)}$
(3)	Cage A	Sun Pinion C	Ring B	$B_r = -\frac{C}{B}\cdot\frac{D_2}{D_1}$	$(1-(5))\left(\frac{1}{1-(2)}\right)$	$B_r = -\left(\frac{G}{1+K(G+1)}\right)$	$K = -\left(\frac{G}{B_r\,(G+1)} + \frac{1}{(G+1)}\right)$
(4)	Cage A	Ring B	Sun Pinion C	$C_r = -\frac{B}{C}\cdot\frac{D_1}{D_2}$	$(1-(2))\left(\frac{1}{1-(5)}\right)$	$C_r = -\left(\frac{1+K(G+1)}{G}\right)$	$K = -\left(\frac{GC_r + 1}{G+1}\right)$
(5)	Sun Pinion C	Cage A	Ring B	$B_r = 1 + \frac{C}{B}\cdot\frac{D_2}{D_1}$	$\left(\frac{1}{1-(1)}\right)(1-(3))$	$B_r = 1 + \frac{G}{1+K(G+1)}$	$K = \frac{G}{(B_r - 1)(G+1)} - \frac{1}{(G+1)}$
(6)	Sun Pinion C	Ring B	Cage A	$A_r = \frac{BD_1}{BD_1 + CD_2}$	$(1-(1))\left(\frac{1}{1-(3)}\right)$	$A_r = \frac{1+K(G+1)}{(G+1)(K+1)}$	$K = \frac{A_r\,(G+1) - 1}{(G+1)(1 - A_r)}$

In using Col. (g) give A_r, B_r, C_r, the correct sign as indicated in Col. (d).

As before, six different combinations of the train are possible, counting inversions, and these six cases are set out in columns (a), (b) and (c), Table IV.

Analysis of Compound Epicyclic Gearing, with Internally Toothed Ring.

1*st method*. *Case* 1. Stationary member. Ring B.
Driving member. Sun pinion C.
Driven member. Cage A.

It will be seen, therefore, that when C revolves once A rotates $\frac{D_2C}{D_2C + D_1B}$ in the same direction.

2*nd method*. Case 1, as before.

This method gives, as it should do, exactly the same result.

The other cases are dealt with in a similar manner, the results being given in column (d), Table IV., and for purposes of reference the full workings are shown in Appendix B.

In the case of a compound epicyclic gear train of the type under consideration, there are two proportions governing the size of gear, that of D_1 to C (Fig. 3) and D_2 to B, or alternatively, D_2 to D_1. The first, that of D_1 to C, has been called K, so that $D_1/C = K$ as before. For the second proportion, the author has selected D_2 to D_1, and has introduced a factor $G = D_2/D_1$. With the aid of these two factors the third member can be fixed, since:—

$$D_1 = CK$$
$$D_2 = D_1G = CKG$$
$$B = (C + D_1 + D_2) = (C + CK + CKG) = C(1 + K + KG)$$

Turning now to Case 1, column (d) (Table IV.), it is seen that

$$A_r = \frac{CD_2}{CD_2 + BD_1}$$

and substituting the foregoing values of D_1, D_2 and B this becomes

$$A_r = \frac{C^2KG}{C^2KG + C^2K(1 + K + KG)} = \frac{G}{G + 1 + K + KG}$$

$$\text{And } A_r = \frac{G}{(K + 1)(G + 1)}$$

From which

$$K = \frac{G}{A_r(G + 1)} - 1$$

The other cases are dealt with in the same way, the results being given in columns (f) and (g), Table IV.

An examination of column (d), Table IV., reveals the interesting

fact that there is a simple relation existing between the various cases. The most obvious, perhaps, is that between Cases 2 and 4,

TABLE V.

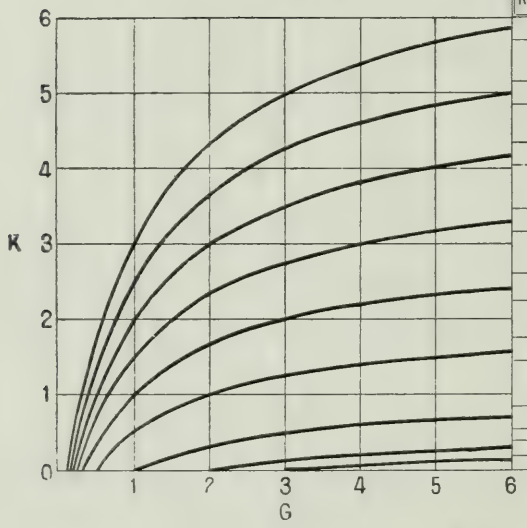
	A	B	C	D_1 & D_2
(a) Fixing A revolve B once.	0	+ 1	$-\frac{B}{D_2}\cdot\frac{D_1}{C}$	$+\frac{B}{D_2}$
(b) Add − 1 to row (a)	− 1	0	$-1-\frac{D_1}{D_2}\cdot\frac{B}{C}$	$-1+\frac{B}{D_2}$
(c) Reducing C to 1 ..	$\frac{D_2C}{D_2C+D_1B}$	0	1	

TABLE VI.

	A	B	C	D_1 & D_2
(a) Each element positive, turn.	+ 1	+ 1	+ 1	+ 1
(b) A held, B negative, turn.	0	− 1	$+\frac{B}{D_2}\cdot\frac{D_1}{C}$	$-\frac{B}{D_2}$
(c) Adding rows (a) and (b).	+ 1	0	$1+\frac{D_1}{D_2}\cdot\frac{B}{C}$	$1-\frac{B}{D_2}$
(d) Reducing C to 1 ..	$\frac{D_2C}{D_2C+D_1B}$	0	1	

where it will be seen that Case 2 = (1 − Case 4) and conversely Case 4 = (1 − Case 2). The conversion formulæ for the remaining

cases are given in column (e), Table IV., where the encircled figures refer to cases. It is clear, then, that having worked out the velocity-ratio for a particular train, it is a simple matter to convert this result to the corresponding ratio for any other combination, and further, that if K and G are plotted graphically with their corresponding ratios, one chart will accommodate all the different combinations. Such a chart is shown in Fig. 4, and gives the various relations between K and G necessary for a given ratio. A minus sign before a velocity-ratio indicates that



	①	②	③	④	⑤	⑥
Stationary member	Ring B	Ring B	Cage A	Cage A	Sun pinion C	Sun pinion C
Driving member.	Sun pinion C	Cage A	Sun pinion C	Ring B	Cage A	Ring B
Driven member.	Cage A	Sun pinion C	Ring B	Sun pinion C	Ring B	Cage A
	Ratio revs. of driven member One rev of driving member					
	1/8	8	−1/7	−7	1 1/7	7/8
	1/7	7	−1/6	−6	1 1/6	6/7
	1/6	6	−1/5	−5	1 1/5	5/6
	1/5	5	−1/4	−4	1 1/4	4/5
	1/4	4	−1/3	−3	1 1/3	3/4
	1/3	3	−1/2	−2	1 1/2	2/3
	1/2	2	−1	−1	2	1/2
	2/3	1 1/2	−2	−1/2	3	1/3
	3/4	1 1/3	−3	−1/3	4	1/4

FIG. 4.

the driven member revolves in the opposite direction to the driver. The chart is self-explanatory, but an example will be taken in order to make its use perfectly clear. Taking $K = 1$ and $G = \frac{1}{2}$ it is found that the curve upon which these values intersect, if followed round until it reaches the Tables, shows that the ratios under the conditions named at the head are 1/6, 6, −1/5, −5, $1\frac{1}{5}$ or 5/6, and the proportions of the gear to obtain these results are $C = 1$, $D_1 = CK = 1$, $D_2 = D_1G = \frac{1}{2}$, and $B = (C + D_1 + D_2) = 2\frac{1}{2}$. The chart can, of course, be used to solve existing gear trains, but in this connection it should be noted that K and G must be

calculated on pitch diameters and not on numbers of teeth, unless the pitch is constant throughout.

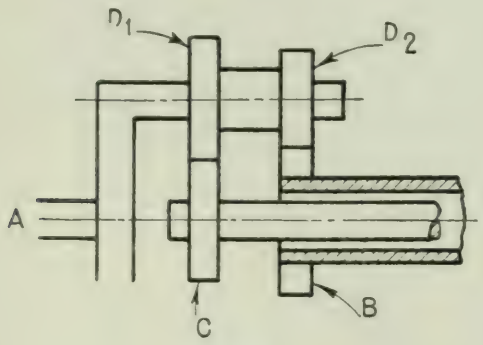
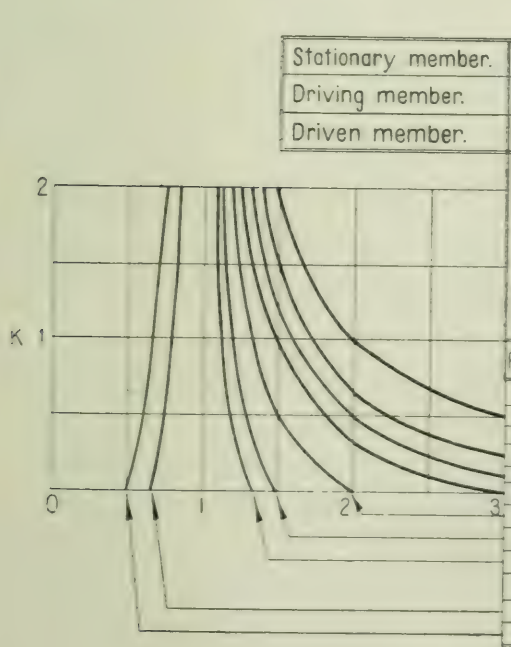


FIG. 5.

Compound Epicyclic Train.—Reverted Train.

This arrangement is shown diagrammatically in Fig. 5, from which it will be seen that the ring has been replaced by a pinion



	①	②	③	④	⑤	⑥
Stationary member.	Wheel B	Wheel B	Cage A	Cage A	Sun pinion C	Sun pinion C
Driving member.	Sun pinion C	Cage A	Sun pinion C	Wheel B	Cage A	Wheel B
Driven member.	Cage A	Sun pinion C	Wheel B	Sun pinion C	Wheel B	Cage A

Ratio revs of driven member : One rev. of driving member.

Limiting ratio					
1 ⅓	⅚	6	⅙	− 5	− ⅕
1 ⅓	¾	4	¼	− 3	− ⅓
1 ½	⅔	3	⅓	− 2	− ½
2	½	2	½	− 1	− 1
3	⅓	1½	⅔	− ½	− 2
4	¼	1⅓	¾	− ⅓	− 3
− 2	− ½	⅔	1½	⅓	3
− 1	− 1	½	2	½	2

FIG. 6.

on the same centre as the first. The velocity-ratios can be found by the methods already described, and are given for each case in column (d), Table VII., while Appendix C. shows in detail

TABLE VII.

Case.	(a) Stationary Member.	(b) Driving Member.	(c) Driven Member.	(d) Ratio.	(e) Conversion Formula.	(f) Substituting G and K in (d).	(g) Value of K.
(1)	Wheel B	Sun Pinion C	Cage A	$A_r = \frac{CD_2}{CD_2 - BD_1}$	$(1-(6))\left(\frac{1}{1-(4)}\right)$	$A_r = \frac{G}{(G-1)(K+1)}$	$K = \frac{1}{A_r\left(1-\frac{1}{G}\right)} - 1$
(2)	Wheel B	Cage A	Sun Pinion C	$C_r = 1 - \frac{B}{C}\cdot\frac{D_1}{D_2}$	$(1-(4))\left(\frac{1}{1-(6)}\right)$	$C_r = \frac{(G-1)(K+1)}{G}$	$K = \frac{C_r}{\left(1-\frac{1}{G}\right)} - 1$
(3)	Cage A	Sun Pinion C	Wheel B	$B_r = \frac{C}{B}\cdot\frac{D_2}{D_1}$	$(1-(5))\left(\frac{1}{1-(2)}\right)$	$B_r = \frac{G}{1+K-KG}$	$K = \frac{G}{B_r(1-G)} - \frac{1}{(1-G)}$
(4)	Cage A	Wheel B	Sun Pinion C	$C_r = \frac{B}{C}\cdot\frac{D_1}{D_2}$	$(1-(2))\left(\frac{1}{1-(5)}\right)$	$C_r = \frac{1+K-KG}{G}$	$K = \frac{GC_r - 1}{(1-G)}$
(5)	Sun Pinion C	Cage A	Wheel B	$B_r = 1 - \frac{CD_2}{BD_1}$	$(1-(3))\left(\frac{1}{1-(1)}\right)$	$B_r = \frac{K(1-G)-(G-1)}{K(1-G)+1}$	$K = \frac{G}{(1-B_r)(1-G)} - \frac{1}{(1-G)}$
(6)	Sun Pinion C	Wheel B	Cage A	$A_r = \frac{BD_1}{BD_1 - CD_2}$	$(1-(1))\left(\frac{1}{1-(3)}\right)$	$A_r = \frac{K(1-G)+1}{K(1-G)-(G-1)}$	$K = \frac{A_rG}{(A_r-1)(1-G)} - \frac{1}{(1-G)}$

In using Column (g) give A_r, B_r, C_r, the correct sign. Cases (1), (2), (5), (6) can have both positive and negative drives for the same ... of K, by varying G. Cases (3) and (4) are always positive.

how these results were obtained. There is, again, a simple relationship (shown in column (e), Table VII.) between each case, so that a graph showing the variations of G and K for a given ratio will be common to all the various combinations, and this chart is shown in Fig. 6. As before, $K = D_1/C$ and $G = D_2/D_1$, and it will be obvious from Fig. 5 that $B = (C + D_1 - D_2)$.

The proportions of the gear train can be written in terms of G and K, thus:—

$$D_1 = CK$$
$$D_2 = D_1G = CKG$$
$$B = (C + D_1 - D_2) = C(1 + K - KG).$$

These values can now be substituted in column (d), Table VII.,

TABLE VIII.

K	Limiting value of G.
$\frac{1}{4}$	5
$\frac{1}{2}$	3
1	2
2	$1\frac{1}{2}$
3	$1\frac{1}{3}$
4	$1\frac{1}{4}$
5	$1\frac{1}{5}$
6	$1\frac{1}{6}$

and will result in columns (f) and (g), from which the graph, Fig. 6, has been plotted.

There is, of course, already a limit to the proportions of the gear train when $K = 0$, but it should be noted also that in this case G reaches its limit when B becomes 0. The limiting value of G in respect to K can therefore be found as follows: $B + D_2 = C + D_1$.

Limit occurs when $B = 0$

$$\text{and } D_2 = C + D_1$$
$$= CKG = C + CK$$
$$= KG = 1 + K$$
$$\text{and } G = \frac{1 + K}{K} = 1 + \frac{1}{K}$$

Therefore G cannot exceed $1+(1/K)$, and these values of G from $K=\frac{1}{4}$ to $K=6$ are given in Table VIII. and are shown graphically in Fig. 6 by the curve referred to as "Limiting Ratio."

The chart, Fig. 6, can be used for selecting or determining velocity-ratios in the way already described for Fig. 4, and calls for no further comment beyond pointing out an interesting feature of cases 1, 2, 5 and 6. It will be noticed that after $G=1$ is passed the direction of rotation of the driven member is reversed. In cases 1 and 2 for values above $G=1$ the driven member rotates

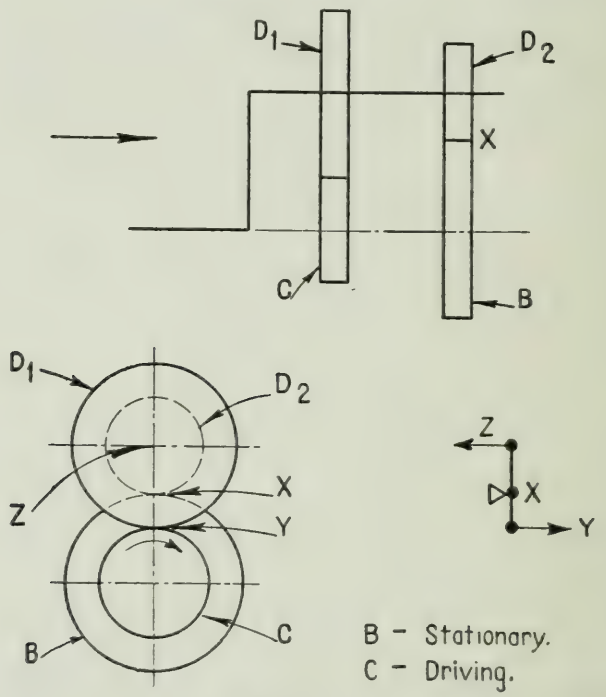


Fig. 7.

in the same direction as the driving member, below $G=1$ its direction is opposite to that of the driving member; in cases 5 and 6 these conditions are reversed, i.e., above $G=1$ the rotation of the driven element is negative, below $G=1$ it is positive. Remembering that $G=D_2/D_1$ this action is best described by reference to Figs. 7 and 8, the conditions being as in case 1. Fig. 7 represents an arrangement in which G is greater than 1, and upon examining the end view it will be seen that if C is revolved in the direction of the arrow then the resultant pressure on the radius of D_1 is in the direction of the arrow Y. Since the point X can be regarded

as fixed, then the resultant motion of the axis of D_1 and D_2 must be in the direction of the arrow Z, which is the same as the rotation of C.

In Fig. 8 is shown the position when G is less than 1. Referring to the end view, it will be seen that if C is rotated in the direction of the arrow, then the resultant motion on the radius of D_1 is in the direction of the arrow Y as before, but in this case the fixed point X is between Y and the axis of D_1, and D_2, which therefore revolves in a direction opposite to C as shown by the arrow Z. Arising out of this change of direction, it will be noted that when

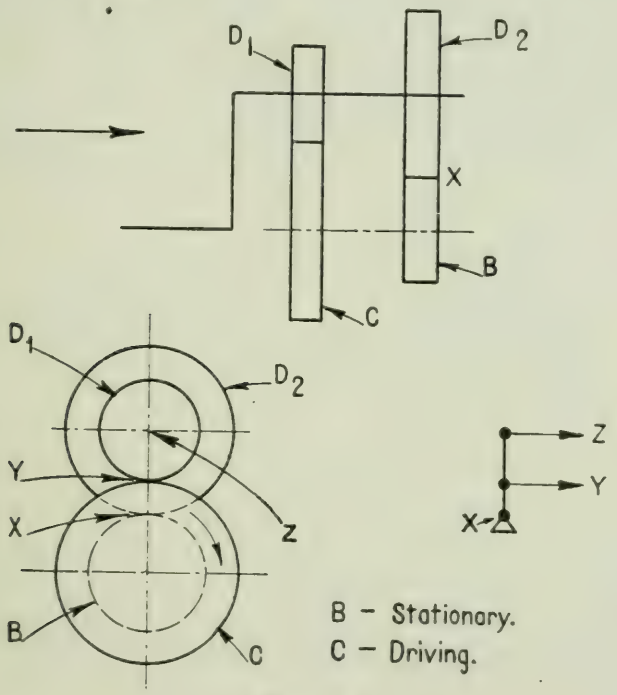


FIG. 8.

G approximates closely to 1 very high velocity-ratios can be obtained. As an example, let $G = 1{\cdot}01$ and $K = 1$.

Taking case 1, i.e., B stationary, C driving, from column (f), Table VII., it is seen that

$$A_r = \frac{G}{(G-1)(K+1)}$$

$$= \frac{\frac{101}{100}}{\frac{1}{100} \times 2} = \frac{101}{100} \times 50 = 50{\cdot}5 : 1$$

As a further example, let $K = \frac{1}{2}$ and $G = 1{\cdot}001$
then for case 1,

$$A_r = \frac{G}{(G-1)(K+1)}$$

$$= \frac{\frac{1001}{1000}}{\frac{1}{1000} \times \frac{3}{2}} = \frac{1001}{1000} \times \frac{2000}{3} = \underline{\underline{667\frac{1}{3} : 1}}$$

As before, G is the ratio between the pitch diameters of D_1 and D_2, Fig. 5.

These examples are, of course, extreme cases, but they are given in order to show the theoretical possibilities of this type of epicyclic gearing.

In concluding this portion of the paper, it may be as well to point out that Figs. 4 and 6 cannot claim to show a complete range of possible ratios. They are intended to act merely as a guide,

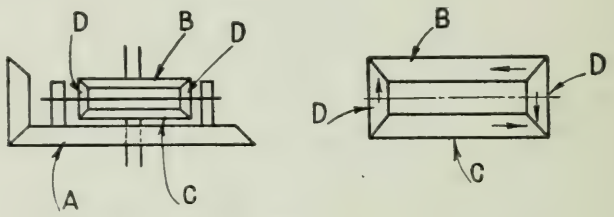


FIG. 9.

and if the desired ratio does not happen to be one of those given in the graphs, a close approximation to G and K can be made by interpolation, when a few minutes spent in plotting the portion of the curve required will enable suitable values of G and K to be selected.

PART II.

Application to Motor Car Design.

The only use of epicyclic gearing which is at all general in automobile work is its application to the back-axle differential. This is usually a bevel gear train of a type not mentioned in the previous notes, since its use is restricted to this one instance. The general arrangement of a differential gear is too well known to need a detailed description, and its design presents no difficulties beyond that of deciding upon the pitch and number of teeth it is desired to employ. The action of the gear can be analysed by the methods already described.

Fig. 9 shows a differential gear in diagrammatic form where A comprises the cage carrying the planet pinions D which gear with the equal wheels B and C.

The procedure described under Method 1 has to be varied somewhat in this instance, but the alteration is a matter of detail only, the underlying principle being the same. It is clear, from an inspection of Fig. 9, that if A is held, one turn of the wheel C will cause the wheel B to turn once in the opposite direction, and row (a), Table IX., can therefore be written down, giving C "X" turns in a positive direction.

The whole mechanism is then given Y revolutions (row (b)), and the addition of rows (a) and (b) will result in row (c). From an inspection of this row it will be seen that A is the mean of

TABLE IX.

	A	B	C
(a) Hold A, C + X ,.....	0	− X	+ X
(b) All elements + Y.....	Y	+ Y	+ Y
(c) Add (a) and (b).....	Y	Y − X	Y + X

the number of revolutions of B and C. Thus, if X is zero, then B and C revolve at the same speed and in the same direction as A, while if one wheel runs X revolutions per minute faster, the mechanism permits the other to run X revolutions per minute slower, the speed of A remaining constant. The spur gear type can be analysed in the same way.

It is in the differential gear that the epicyclic train has so far held its own, but there are signs that in this case it may have to give way to some other form of gearing, or that at any rate its range of action may be limited. It possesses advantages in that it is compact, simple, and cheap to manufacture, but if it is to work properly, the resistances offered to the wheels C and B should be the same. It frequently happens, however, that, due to a bump, one road wheel leaves the ground, in which case, assuming

the wheel remains suspended. "X" (Table IX.) increases rapidly, the speed of the other road wheel falling in proportion until in the limit when X = Y, the speed of the wheel in the air becomes twice that of the cage, the other wheel being stationary. When the wheel returns to earth the conditions are reversed, "X" changing sign and falling to zero, until the drive becomes normal again.

Actually, of course, the wheel would not remain in the air long enough for this to happen. A part of the power released when one wheel leaves the ground will cause additional equal pressures on the teeth of the differential wheels B and C, and therefore will develop additional equal torques in the respective axle driving-shafts. The torque in the axle shaft connected to the wheel in the air is absorbed in accelerating its rotational speed (Y + X), whereas the torque in the other axle shaft will be absorbed in overcoming the resistance to rotation of the wheel which is still in contact with the ground. If this resistance is less than the available torque the car will slightly gain speed, and if it is greater the car will lose speed, but in general the speed change will be negligible and (Y – X) may be regarded as constant. When the wheel again comes to the ground Y returns to approximately its original value, and X decreases to zero.

In the case of a propeller shaft brake, if the adhesion of each wheel is the same, and their radii are equal, the retardation on each wheel will be the same, and there will be no tendency for reverse rotation when the brake is applied, but if the wheel diameters differ, or if one wheel has a greater coefficient of adhesion than the other, then if the cage A is locked, the small wheel, or the wheel with the smaller coefficient of friction will be accelerated backwards.

The attention of designers has been for some time directed towards the elimination of these defects, but up to the present the bevel or spur gear epicyclic train is still conventional practice.

Epicyclic Trains for Gear-Boxes.

At first sight epicyclic gearing seems to be ideal for the change-speed mechanism of a motor car. The advantages are obvious; gears are always in engagement, and the different velocity-ratios are obtained by means of brake-bands or clutches operating on the members which have to remain stationary. The engagement of a gear, therefore, becomes a simple matter, demanding no special experience or particular "knack" to ensure a quiet change such

as is required by the usual "unmechanical" sliding-gear type of box. These advantages, however, are offset by the inherent complexity of the gear when more than two velocity-ratios are required, as an examination of the problem will show that considerable ingenuity is necessary to keep the number of parts within reasonable limits. Upon consideration it will be seen that although different members can conveniently be held stationary when required by means of suitably arranged brake-drums, it will greatly simplify the design if either the driving or the driven member is the same throughout. This condition implies that another train of gears will have to be introduced, as a glance at graphs Figs. 2, 4 and 6 shows that only two different ratios can be obtained when using the same driving or driven member. The same graphs also show

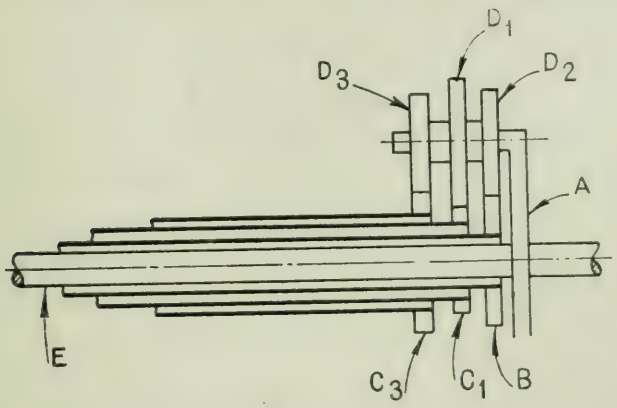


FIG. 10.

that it is advisable to have a separate reverse train, since although it is possible to obtain two drives having opposite directions of rotation and using the same driving member, the velocity-ratios involved are not convenient for use in practice.

From this it is apparent, then, that if the gear-box is to be reasonably simple, a reverse and three forward speeds only are available. For these reasons, perhaps, the epicyclic gear-box has not found favour amongst designers in this country, there being, as far as the author is aware, only one British car employing this type of change-speed gearing.

The Ford Gear-Box.

It may be of interest to examine two epicyclic gear-boxes which have given satisfactory service over long periods.

The Ford gear is shown in diagrammatic form in Fig. 10. Two

forward speeds and a reverse are provided, and since the second speed is a direct drive the reverse and one forward speed only are obtained by epicyclic gearing. Referring to Fig. 10, A is the cage connected to the crankshaft and carrying the three planet pinions D_1, D_2 and D_3, these engaging with the gears B, C_1, C_3; B is connected to the propeller shaft, and the second speed is obtained directly by clutching B to E, an extension of the clutch shaft. For the first speed, gears B, D_2, D_1 and C_1 are employed, A is, of course, the driving member, C_1 is held stationary by means of a brake-band acting on a drum, and B is the driven member. This corresponds to case 5, Fig. 6. The proportions of the gears have been scaled from a small size illustration, and from this it appears that $D_1 C_1 = K$ is approximately $-1\frac{1}{2}$, and $D_2/D_1 = G$ is 5/6. Reference to Fig. 6, case 5, will show that this ratio is in the neighbourhood of 1/3 to 1, thus the engine makes three revolutions to one of the propeller shaft. The reverse train consists of gears C_3, D_3, D_2 and B. The driving member is again A, the stationary element being C_3. In this case K is approximately $\frac{3}{4}$ and G is 7/6, and from Fig. 6, case 5, it is seen that this gives a ratio of -1 3, the reverse, therefore, being approximately equal to the first speed. An estimate of the number of parts shows that there are at least nine gear wheels, two brake-drums, and three shafts necessary to obtain the two speeds and reverse. A third brake-drum is fitted, but this is not part of the gear-box as such, being solely used for reducing the car speed.

The Lanchester Gear-Box.

The foregoing example was selected as it illustrates one of the simplest forms of epicyclic gear-box, and an examination of the Lanchester gear may be of interest as showing its possibilities when applied to high-class automobile practice. The author has been unable to find a later description of this gear-box than that given in Vol. I. of the "Automobile Engineer," and from the illustration given on page 85 the diagram Fig. 11 has been prepared. It will be seen that the gear consists of three simple epicyclic trains, two of them, however, being inter-connected. These two trains provide the first and second speeds, the third speed is direct drive, and the reverse is obtained by means of the left-hand train, which consists of gears C_1, D_1, the internally toothed ring B_1 and the cage A_1. This last is capable of being held stationary when the reverse is required, and C_1, rotating with

the crankshaft, becomes the driving member, turning B_1 in the reverse direction, as can be seen by inspecting graph Fig. 2 for the conditions specified. The proportions of the gears have been roughly scaled from the illustration in the "Automobile Engineer" and a value of K = 4 deduced, and reference to Fig. 2, case 3, shows the ratio to be approximately −0·11; actually for this value of K it is −1 9. The first-speed train consists of the gears C_2, D_2, the internally toothed ring B_2, and the cage A_2, which is the driven member. To obtain the first speed, B_2 is held stationary, and C_2, revolving with the crankshaft, becomes the driving member. These conditions correspond to case 1, Fig. 2, and estimating, as before, the value of K to be $\frac{3}{4}$, the ratio is seen to be about 0·3; actually for this value of K it is 2 7, as can be proved by an inspection of the graph for case 2, which is the inverse of case 1.

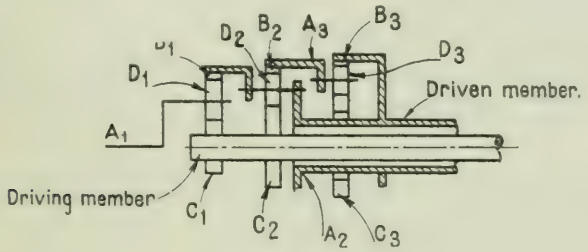


FIG. 11.

For the next speed, the gears C_3, D_3, the ring B_3, and the cage A_3 are used in addition to the first speed train. For the second speed, C_2 is again the driving member, the stationary member being the sun pinion C_3 of the second train, and the driven member B_3. It will be noticed that B_3 is connected to, and revolves with, A_2, and that A_3 is really part of B_2.

As the analysis of these trains is rather interesting, it is set out in full beneath Fig. 11, and the rules given under the "1st Method" described previously have been followed. Obviously, the first thing to do is to discover the relative motion of the elements. This is best accomplished by considering the trains separately, holding the cages A_2, A_3 and turning one of the other elements in each train. This results in row (a), Fig. 11. Now, considering the first train for the present, imagine the ring B_2 to be stationary, and by adding C_2 B_2 to row (a) for this train the corresponding motions of C_2 and A_2 are obtained as in row (b). But B_2 is not stationary, it has rotational motion imparted to it

by the second train, this rotation being the same as that given to A_3. By adding 1 to row (a) for the second train, the conditions are obtained in row (b) where the second sun pinion C_3 is stationary, and it is seen that for a certain rotation of B_3, A_3 revolves once in the same direction. By substituting this value of A_3 for B_2 the two trains can be compared, since they satisfy the conditions previously set forth. To bring B_2 to this value, it is necessary to add 1 to row (b) of the first train, and the result is seen in row (c). It is now clear that since C_2 is revolving, C_3 is stationary and B_2 and A_3 are rotating at equal speeds, A_2 is of necessity equal to B_3. It therefore follows that this design of epicyclic gearing can only work when C_2/B_2 equals C_3/B_3. These results can be brought to a more convenient form by reducing the rotation of C_2 to 1, and this can be done by multiplying row (c) throughout by $B_2/(2\ B_2 + C_2)$, thus obtaining row (d). The conditions necessary for the second speed have now been complied with; C_2 has rotated once, C_3 is stationary, B_2 is being driven by the second train, and the corresponding rotation of A_2 can now be obtained.

From the illustration previously mentioned, the proportions of the gears appear to be approximately as follows:—

$C_2 = 4$	$C_3 = 3\frac{1}{5}$
$D_2 = 3$	$D_3 = 2\frac{2}{5}$
$B_2 = 10$	$B_3 = 8$

On substituting these values in row (d) it is seen that the second speed ratio is roughly 7/12.

An estimate of the number of pieces in the gear shows that there are at least nine gear wheels, three internally toothed rings, and three brake-drums. This, no doubt, explains the charge of complexity usually levelled against epicyclic gear boxes, but an examination will show that in the main the parts are comparatively simple, and the great advantage of obtaining a noiseless and mechanically correct gear must considerably offset the disadvantage of using a few extra parts.

Engine Starters, &c.

Another direction in which epicyclic gearing offers possibilities is in connection with the gear reduction for the engine starter. A good example of this is the Scott Dynamotor, illustrated and described in "The Autocar" for November 1st, 1913. From the

diagram of this gear given in Fig. 12, it will be seen that it is of the reverted type. When used for starting the engine, the sun pinion C (Fig. 12) becomes the driving member of the train and B the driven member. The speed of A increases as the engine picks up the drive, until it reaches the same speed as the shaft of C, to which it then becomes clutched by means of a free-wheel device, and the epicyclic gearing goes out of action. Thus, a low gear is provided for engine starting, and a higher driven gear for the rotation of the electric motor as a dynamo when the engine has taken up the work. An estimate of the proportions of the gears indicates values of $K = 2$ and $G = \frac{1}{2}$. From the equation given in row (f), case 3, Table VII., the velocity-ratio is seen to be about $\frac{1}{4}$ to 1, or, putting it the other way round, the armature shaft revolves at four times the speed of the driven wheel. It will be understood, of course, that this is only the epicyclic gearing

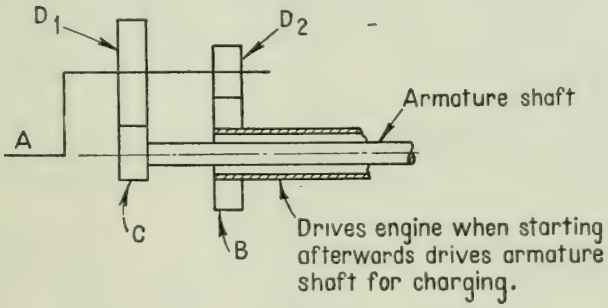


FIG. 12.

reduction; between this and the engine is a further reduction of approximately $3\frac{1}{2}$ to 1.

The principal uses of epicyclic gearing in automobile practice have now been briefly touched upon. There remains, however, one further example of the use of this gear in motor car design, and that is in steering-box reduction. So far, this has been confined to comparatively light cars with short wheel bases, and one of the reasons for this is apparent upon inspection of Fig. 2. Bearing in mind that the usual steering box ratio is in the neighbourhood of 8 to 1, it will be seen that to obtain this with simple epicyclic gearing, which is the type usually employed for this purpose, a planet pinion three times the diameter of the sun pinion will be required. A gear of this size would, of course, have a clumsy appearance, and would seem to be out of all proportion to the work required of it. Other factors enter into the restriction

of the use of epicyclic gearing in this direction, one being that to take full advantage of it the steering-box requires to be close up to the front axle in order that a connection can be taken direct on to the steering cross rod.

The study of epicyclic gearing is fascinating because of the element of uncertainty about it. Its movements cannot very well be pictured graphically, and it is only by the aid of pencil and paper that its action can be analysed.

The epicyclic gear-box is acclaimed by many as being the cure for all change-speed troubles, and this is undoubtedly true, but an inspection of the Lanchester gear-box will show that a tremendous amount of thought had to be expended before such a compact arrangement could be arrived at for providing three epicyclic gear ratios. This type of gear also calls for careful and unusual machining operations, since it will be obvious that a cage which has to carry three pinions engaging accurately with a common sun pinion and internally toothed ring needs to be very accurately produced. These difficulties, however, are not insurmountable with modern machine-shop methods, and in the author's opinion the greatest difficulty that the epicyclic gear-box has to face is the question of weight. If it can be designed together with its change-speed mechanism to closely approximate to the weight of the conventional type of box and change-speed gear providing the same number of speeds, then the epicyclic gear-box will have gone a long way towards establishing its supremacy.

APPENDIX "A."—See Table I.

Case.	1st Method.	A	B	C	D	2nd Method.	A	B	C	D
(1)	(a) Hold A, C, + 1	0	$-\frac{C}{D}\cdot\frac{D}{B}$	+ 1	$-\frac{C}{D}$	(a) Each element + 1	+ 1	+ 1	+ 1	+ 1
	(b) Add $\frac{C}{B}$ to (a)	$\frac{C}{B}$	0	$1+\frac{C}{B}$		(b) Hold A, B, − 1	0	− 1	$+\frac{B}{D}\cdot\frac{D}{C}$	$-\frac{B}{D}$
	(c) Reducing C to 1	$\frac{C}{B+C}$	0	1		(c) (a) + (b)	+ 1	0	$1+\frac{B}{C}$	$1-\frac{B}{D}$
						(d) Reducing C to 1	$\frac{C}{B+C}$	0	1	
(2)	(d) Reducing A, row (b) to 1	1	0	$1+\frac{B}{C}$		(e) Given in row (c)	1	0	$1+\frac{B}{C}$	
(3)	(e) Given in row (a)	0	$-\frac{C}{B}$	+ 1	$-\frac{C}{D}$	(f) By inspection of diagram.	0	$-\frac{C}{B}$	+ 1	$-\frac{C}{D}$
(4)	(f) Reducing B, row (a) to 1	0	+ 1	$-\frac{B}{C}$		(g) By inspection of diagram.	0	+ 1	$-\frac{B}{C}$	$+\frac{B}{D}$
(5)	(g) Row (a) − 1	− 1	$-1-\frac{C}{B}$	0	$-1-\frac{C}{D}$	(h) Each element + 1	+ 1	+ 1	+ 1	+ 1
	(h) Changing signs	+ 1	$1+\frac{C}{B}$	0	$1+\frac{C}{D}$	(j) Hold A, C, − 1	0	$+\frac{C}{B}$	− 1	$+\frac{C}{D}$
						(k) (h) + (j)	+ 1	$1+\frac{C}{B}$	0	$1+\frac{C}{D}$
(6)	(j) Reducing B, row (h) to 1	$\frac{B}{B+C}$	+ 1	0		(l) Reducing B, row (k) to 1	$\frac{B}{B+C}$	+ 1	0	

APPENDIX "B."—See Table IV.

Case.	1st Method.					2nd Method.				
		A	B	C	D_1 & D_2		A	B	C	D_1 & D_2
(1)	(a) Hold A, B, + 1	0	+ 1	$-\frac{B}{D_2}\cdot\frac{D_1}{C}$	$+\frac{B}{D_2}$	(a) Each element + 1....	+ 1	+ 1	+ 1	+ 1
	(b) Add — 1 to row (a) ..	— 1	0	$-1-\frac{D_1}{D_2}\cdot\frac{B}{C}$	$\frac{B}{D_2}-1$	(b) Hold A, B, — 1	0	— 1	$+\frac{B}{D_2}\cdot\frac{D_1}{C}$	$-\frac{B}{D_2}$
	(c) Reduce C to 1	$\frac{D_2C}{D_2C+D_1B}$	0	1		(c) (a) + (b)	+ 1	0	$1+\frac{D_1}{D_2}\cdot\frac{B}{C}$	$1-\frac{B}{D_2}$
						(d) Reduce C to 1	$\frac{D_2C}{D_2C+D_1B}$	0	1	
(2)	(d) Given in row (b)	+ 1	0	$1+\frac{D_1}{D_2}\cdot\frac{B}{C}$		(e) Given in row (c)	+ 1	0	$1+\frac{D_1}{D_2}\cdot\frac{B}{C}$	
(3)	(e) Reduce C, row (a) to 1	0	$-\frac{D_2}{D_1}\cdot\frac{C}{B}$	1		(f) From inspection	0	$-\frac{C}{D_1}\cdot\frac{D_2}{B}$	+ 1	$-\frac{C}{D_1}$
(4)	(f) Given in row (a)	0	+ 1	$-\frac{D_1}{D_2}\cdot\frac{B}{C}$		(g) From inspection ...	0	+ 1	$-\frac{B}{D_2}\cdot\frac{D_1}{C}$	$+\frac{B}{D_2}$
(5)	(g) Add — 1 to row (c) ..	$-\frac{D_1B}{D_2C+D_1B}$	— 1	0		(h) Each element + 1 ..	+ 1	+ 1	+ 1	+ 1
	(h) Reduce A, row (g) to 1	1	$1+\frac{D_2C}{D_1B}$	0		(k) Hold A, C, — 1	0	$+\frac{C}{D_1}\cdot\frac{D_2}{B}$	— 1	$+\frac{C}{D_1}$
						(l) (h) + (k)	+ 1	$1+\frac{D_2C}{D_1B}$	0	$1+\frac{C}{D_1}$
(6)	(k) Given in row (g)	$\frac{D_1B}{D_1B+D_2C}$	1	0		(m) Reduce B, row (l) to 1	$\frac{D_1B}{D_1B+D_2C}$	1	0	

Case.	1st Method.					2nd Method.				
		A	B	C	D_1 & D_2		A	B	C	D_1 & D_2
(1)	(a) Hold A, C, + 1	0	$+\frac{C}{D_1} \times \frac{D_2}{B}$	+ 1	$-\frac{C}{D_1}$	(a) Each element + 1 ..	+ 1	+ 1	+ 1	+ 1
	(b) Add $-\frac{C}{D_1} \times \frac{D_2}{B}$ to (a)	$-\frac{C}{D_1} \times \frac{D_2}{B}$	0	$1-\frac{C}{D_1} \cdot \frac{D_2}{B}$		(b) Hold A, B, — 1	0	— 1	$-\frac{B}{D_2} \cdot \frac{D_1}{C}$	$+\frac{B}{D_2}$
	(c) Reduce C to 1	$\frac{CD_2}{CD_2 - BD_1}$	0	1		(c) (a) + (b)	+ 1	0	$1-\frac{B}{D_2} \cdot \frac{D_1}{C}$	
						(d) Reduce C to 1	$\frac{CD_2}{CD_2 - BD_1}$	0	1	
(2)	(d) Reduce A, row (b) to 1	+ 1	0	$1-\frac{BD_1}{CD_2}$		(e) Given in row (c)	+ 1	0	$1-\frac{BD_1}{CD_2}$	
(3)	(e) Given in row (a)	0	$+\frac{CD_2}{BD_1}$	+ 1		(f) From inspection.....	0	$+\frac{C}{D_1} \cdot \frac{D_2}{B}$	+ 1	$-\frac{C}{D_1}$
(4)	(f) Reduce B, row (e) to 1.	0	1	$\frac{BD_1}{CD_2}$		(g) From inspection	0	+ 1	$+\frac{B}{D_2} \cdot \frac{D_1}{C}$	$-\frac{B}{D_2}$
(5)	(g) Add — 1 to row (a) ..	— 1	$-1+\frac{CD_2}{BD_1}$	0		(h) Each element + 1 ..	+ 1	+ 1	+ 1	+ 1
	Row (g) =	1	$1-\frac{CD_2}{BD_1}$	0		(k) Hold A, C, — 1	0	$-\frac{C}{D_1} \cdot \frac{D_2}{B}$	— 1	$+\frac{C}{D_1}$
						(l) (h) + (k)	+ 1	$1-\frac{CD_2}{BD_1}$	0	
(6)	(h) Reduce B, row (g) to 1	$\frac{BD_1}{BD_1 - CD_2}$	1	0		(m) Reduce R, row (l) to 1	$\frac{BD_1}{BD_1 - CD_2}$	1	0	

GRADUATES' SECTION.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

GRADUATES' SECTION.

LONDON BRANCH.

OFFICERS FOR SESSION 1920—1921.

Chairman of General Meetings.

F. L. MARTINEAU, *Member of Council.*

Committee.

H. B. BENNY, *Chairman.*
W. E. BENBOW.
C. A. CHAPPELL.
H. DREW-JONES.
L. H. ROCHFORD.
T. W. WELDHEN.

Hon. Propaganda Secretary.

C. A. CHAPPELL.

Hon. Secretary.

D. J. MACKLIN,

54, Maldon Road, Acton, W.3.

The First Meeting of the Session was held at 28, Victoria Street, London, S.W.1, on Thursday, October 14th, 1920, at 8 p.m., Mr. F. Leigh Martineau, who had kindly consented to act as Chairman throughout the Session, being in the Chair, when a paper entitled "Factors affecting Power Output" was read by Messrs. E. Chatterton and L. F. Watson. There were present thirty-six Graduates and visitors.

The Second Meeting of the Session was held on Thursday, November 18th, 1920, at 8 p.m., Mr. H. B. Benny opened a discussion on "Salient Points of the Motor Show," in which fourteen Graduates and visitors joined. There were present twenty Graduates and visitors.

The Third Meeting of the Session was held on Thursday, December 16th, 1920, at 8 p.m., A paper was read by Mr. T. H. B. Whiting entitled "Carburation," followed by an interesting discussion in which Mr. R. W. A. Brewer, Major A. D. Owen, Dr. W. R. Ormandy, Capt. A. C. Burgoine and Messrs. Rochford, Benny, Cowell, Macklin, Drew-Jones, Wardall and the Chairman took part. There were present thirty-eight Graduates and visitors.

The Fourth Meeting of the Session was held on Thursday, January 20th, 1921, at 8 p.m., when a paper was read by Mr. W. H. Wardall, entitled "Cylinder and Piston Wear," followed by a discussion in which Dr. Ormandy, Major Owen, Messrs. E. Chatterton, A. E. Parnacott, H. B. Benny, E. L. Bass, F. R. Cowell, H. Drew-Jones, D. J. Macklin, E. la V. Parisot and the Chairman took part. There were present twenty-seven Graduates, Members and visitors.

The Fifth Meeting of the Session was held on Thursday, February 17th, 1921, at 7.30 p.m., when a paper entitled "Engine Lubrication" was read by Mr. E. L. Bass. There were present forty-nine Graduates, Members and visitors—a record attendance.

The Sixth Meeting of the Session was held on Thursday, March 17th, 1921, at 7.30 p.m., when Messrs. H. B. Benny and J. D. Macklin read a paper entitled "Modern Tendencies in Automobile Engine Design." There were present nineteen Graduates and visitors.

The Seventh Meeting of the Session was held on Thursday, April 21st, 1921, at 7.30 p.m., when Mr. A. O. Marshall read a paper entitled "Infinitely Variable Transmission." There were present thirty-nine Graduates, Members and visitors.

The Eighth Meeting of the Session was held on Thursday, May 19th, 1921, at 7.30 p.m., when a very interesting and instructive lecture, illustrated by the use of lantern slides, was delivered by Mr. Martineau (Chairman of the Section), entitled "Little Known Facts in Transport Development." At the conclusion of the lecture a number of questions were asked by various Graduates. There were present twenty Graduates and visitors.

Visits.

The following visits took place during the Session:—

1920.

October ..... The Associated Equipment Company, Ltd., Walthamstow.
November ... The Motor Show.
December.... Leyland Motors, Limited, Ham.

1921.

January .... J. A. Prestwich & Company, Ltd., Tottenham.
February .... C. A. Vandervell & Company, Ltd., Acton. (Mid-week Visit.)
,, ... D. Napier & Son, Ltd., Acton.
March ..... British Westinghouse Company.
April ..... London Electric Railway Company, Lillie Road.

The Graduates wish to express their thanks to the above firms for granting facilities for these visits, and to the various members of the firms who kindly conducted them round.

Dinner.

On Saturday, February 5th, 1921, the Annual Dinner and Concert was held at "The Vogue," 3, Victoria Street, S.W.1. Owing to illness, the President (Sir Henry Fowler, K.B.E.) was unable to attend, and the Chair was taken by Mr. F. Leigh Martineau, supported by Messrs. T. Clarkson and Basil H. Joy.

The Dinner was followed by an enjoyable musical programme, concluding with dancing.

BIRMINGHAM BRANCH.

OFFICERS FOR SESSION 1920—1921.

Committee.

W. H. Prestage, *Chairman.* C. A Gosling.
E. L. Every. K. B. Miller.
V. A. Ford. G. N. Wood.

Hon. Secretary.

T. G. Bradley,
Woodbine Cottage, Hampton-in-Arden, Warwickshire.

The First Meeting of the Session was held on Wednesday, October 20th, 1920, Sir Henry Fowler being in the Chair. A

paper was read by Mr. V. A. Ford on "Die Castings." There were present forty-five Graduates and visitors.

The Second Meeting was held on Wednesday, November 17th, 1920, which took the form of an inter-debate with the Coventry Branch, Mr. G. H. Lanchester being in the Chair. Four short papers were read by G. Martineau and C. Gore-Brown of Coventry, and A. K. Bennett and H. L. Cleverly of Birmingham. There were present sixty-eight Graduates and visitors.

The Third Meeting was held on Wednesday, December 15th, 1920, Major B. W. Shilson being in the Chair. A paper was read by Mr. J. Heard on "Transmission Systems for Light Cars." There were present thirty-eight Graduates and visitors.

On Wednesday afternoon, January 12th, 1921, a visit to the Austin Motor Co., Bromsgrove, was paid, when sixty Graduates took part.

The Fourth Meeting was held on Wednesday, February 16th, 1921, Mr. F. G. Woollard being in the Chair. A paper was read by Mr. W. H. Prestage on "The Relative Advantages of Worm and Spiral Bevel Final Drives for Automobiles." There were present thirty-nine Graduates and visitors.

The Fifth Meeting was held on Wednesday, March 2nd (postponed from January 18th), Mr. J. E. Cooper being in the Chair. A paper was read by Mr. T. R. Florance on "The Relative Advantages of Two-Stroke and Four-Stroke Engines." There were present forty-one Graduates and visitors.

The Sixth Meeting was held on Wednesday, March 16th, 1921, Mr. J. P. Hillhouse being in the Chair. A paper was read by Mr. T. Burkett on "Lubrication Systems." There were present thirty-eight Graduates and visitors.

On Saturday, April 2nd, a Dinner of the Birmingham Graduates took place at the White Horse Hotel, Birmingham, Sir Henry Fowler (President of the Institution) being in the Chair. There were present forty-five Graduates and visitors.

The Seventh Meeting was held on Wednesday, April 20th, 1921, Mr. T. C. Aveling being in the Chair. A paper was read by

Mr. E. P. Willoughby on "Petrol Engine Valves." There were present twenty-five Graduates and visitors.

The Eighth Meeting was held on Wednesday, May 18th, 1921, Major C. W. Jordan being in the Chair. A paper was read by Mr. C. A. Gosling on "The Mass Production of Motor Cycles." There were present twenty Graduates and visitors.

COVENTRY BRANCH.

OFFICERS FOR SESSION 1920—1921.

Committee.

C. Gore-Brown, *Chairman.* J. R. Hamilton.
L. Griffiths. F. P Cross

Hon. Propaganda Secretary.

R. V. Newton,
"Somerset House," St. Nicholas St., Coventry.

Hon. Secretary.

R. C. Barry,
165, Gulson Road, Coventry.

The Annual Business Meeting was held on Tuesday, September 28th, 1920, with Mr. Basil H. Joy, Secretary of the Institution, in the Chair. There were present twenty-one Graduates and visitors.

The Second Meeting was held on Tuesday, November 2nd, 1920, Major C. W. Jordan being in the Chair. A paper was read by Mr. R. V. Newton on "Applied Time Study." There were present thirty-one Graduates and visitors.

The Third Meeting, which took the form of an inter-debate with the Birmingham Branch, was held on Wednesday, November 17th, 1920, Mr. G. H. Lanchester being in the Chair.

The Fourth Meeting was held on Tuesday, November 30th, 1920, Major B. W. Shilson being in the Chair. A paper was read

by Lieut. Cooper on "Some Features of Design from a Repairer's Point of View." There were present forty-one Graduates and visitors.

The Fifth Meeting was held on Tuesday, December 14th, 1920, Mr. G. H. Lanchester being in the Chair. A paper was read by Mr. A. Hayward on "Combustion." There were present fifty-three Graduates and visitors.

The Sixth Meeting was held on Tuesday, January 11th, 1921, Mr. A. E. Berriman being in the Chair. A paper was read by Mr. J. Newey on "Cooling Systems." There were present forty-two Graduates and visitors.

The Seventh Meeting was held on Tuesday, January 25th, 1921, Major B. W. Shilson being in the Chair. A paper was read by Mr. J. Hacking on "Accessibility." There were present twenty-five Graduates and visitors.

The Eighth Meeting was held on Tuesday, February 8th, 1921, Mr. H. Ireland being in the Chair. A paper was read by Mr. L. Sterland on "The Repair and Maintenance of H.T. Magnetos." There were present thirty-three Graduates and visitors.

The Ninth Meeting was held on Tuesday, February 22nd, 1921, Sir Henry Fowler being in the Chair. A paper was read by Mr. L. H. Dawtry on "Some Problems we Meet." There were present forty-three Graduates and visitors.

The Tenth Meeting was held on Tuesday, March 8th, 1921, Major C. W. Jordan being in the Chair. A paper was read by Mr. G. M. Martineau on "Motor Cycle Frame and Chassis Design." There were present thirty-three Graduates and visitors.

The Eleventh Meeting was held on Tuesday, March 22nd, 1921, Mr. E. M. Boote being in the Chair. A paper was read by Mr. T. R. Speck on "Factory Planning and Design." There were present twenty-eight Graduates and visitors.

The Twelfth Meeting was held on Tuesday, April 12th, 1921, Mr. J. L. Milligan being in the Chair. A paper was read by Mr. F. P. Cross on "Graphical Design of Valve Cams." There were present eighteen Graduates and visitors.

SUMMER VISIT

SUMMER VISIT, 1921.

London.

The 1921 Summer Visit of the Institution must be pronounced another unqualified success, in spite of the alteration in the venue which was made at a late date. The original idea was to visit Birmingham, but the state of trade throughout the country made it evident that the majority of the works could not extend to visitors such a welcome as they would have liked, and it was decided to make London the headquarters.

On Tuesday, June 14th, the Associated Equipment Company very kindly placed a "K" Type 'Bus at the disposal of the party to visit the Royal Aircraft Establishment at Farnborough, and a wonderfully speedy and pleasant journey was made just in advance of the Ascot traffic. It is very remarkable that a vehicle designed for use on the London streets should form such an efficient and comfortable means of transport over the Hog's Back, which was the road taken on the return journey.

At the Royal Aircraft Establishment some very interesting new devices for starting up aircraft engines, together with some fire extinguishing apparatus were shown in operation on the aerodrome, while in the test house, among a multitude of other things, members were able to visualise, by means of a clever lighting device, the actual motion of a tappet roller in relation to its cam, and a most efficient exhaust gas driven supercharging air pump was compared with a corresponding German device for discounting the effect of reduction of atmospheric pressure at high altitudes.

After lunch the party journeyed to the R.A.S.C. Headquarters at Aldershot, where General Sir Evan Carter and Commandant Brooke had prepared a most interesting programme of inspection of captured German transport vehicles. The officers of the R.A.S.C. who conducted the party round made this a most instructive visit, and the trophies should be inspected by all automobile engineers. After the inspection, a very interesting display of manœuvring over rough ground was given by a wheel tractor and a caterpillar tractor: the latter proved its superiority in mounting almost vertical banks. After entertaining the members to tea Sir Evan Carter bade them farewell in a kindly speech, in

which he emphasised the necessity for the closest co-operation between the military and the civil side of motor transport.

On Wednesday, favoured by wonderful weather, the party proceeded by launch to Hampton Court, the morning being entirely devoted to recreation, while in the afternoon a visit was paid to the National Physical Laboratory, which, as always, proved to be of the greatest interest. Returning to London by launch, with a narrow escape of being stopped at Richmond Lock by the low tide, the party finished up at the Coliseum.

On Thursday, June 16th, members were introduced to what was a novelty to probably all of them, and by the kindness of the Port of London Authority a visit was paid to the warehouses in the London Docks, where the arrangements made for the storage of wool, ivory, wine, spices, etc., were explained. After lunch a most interesting two hours were spent in the laboratories of the G. P. O., where all the instruments used in the testing of telephone circuits and apparatus, wireless plant, and other instruments were demonstrated in actual operation. The trip was brought to a most fitting conclusion by a visit to the Mansion House, where the Lord Mayor received the members and himself showed them some of the treasures, and then entertained them to tea.

LIBRARY CATALOGUE.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

LIBRARY CATALOGUE.

September, 1921.

LIBRARY RULES.

The Library at the offices of the Institution is open to members on week-days from 10 a.m. to 5 p.m., except on Saturdays, when it closes at 1 p.m.

Books may be taken away from the Library on loan, subject to the following conditions:—

1. The period of loan for a volume is three weeks, but it may be prolonged for a further two weeks provided that the book is not required by another member.

2. Any member requiring a library book to be sent through the post must defray the cost of postage both ways.

3. Any member borrowing a book must undertake the financial responsibility for its loss, non-return, or damage.

PROCEEDINGS, &c.

THE INSTITUTION OF CIVIL ENGINEERS. Proceedings, 1865 to 1920, except the following volumes:—

31	38	44	53	58	64	70	187	192
32	39	45	54	59	65	72	188	193
35	40	46	55	60	66	82	189	194
36	41	49	56	61	68	98	190	
37	42	50	57	63	69	162	191	

THE INSTITUTION OF MECHANICAL ENGINEERS. Proceedings, 1888 to 1920, except the following volumes:—

1897 Parts 2, 3 and 4 1898 Parts 1—4 1899 Parts 1, 2 and 4
1904 Part 2 1909 Parts 1 and 2

THE INSTITUTION OF ELECTRICAL ENGINEERS. Journal, 1912, 1913, 1919 and 1920.

THE INSTITUTION OF AUTOMOBILE ENGINEERS. Proceedings, Vols. 1 to 14, 1906 to 1920.

THE INSTITUTION OF ENGINEERS AND SHIPBUILDERS IN SCOTLAND. Transactions, 1906 to 1920, except Vol. 60, 1916/17.

THE NORTH-EAST COAST INSTITUTION OF ENGINEERS AND SHIPBUILDERS. Transactions, 1884 to 1920, except Vol. 4, 1887/8.

THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS. Transactions, 1907 to 1919.

THE SOCIETY OF AUTOMOTIVE ENGINEERS, U.S.A. Transactions, 1911 to 1919, except Part I., 1916.

THE INSTITUTE OF METALS. Journal, 1909 to 1921.

THE CYCLE ENGINEERS' INSTITUTE. Proceedings, 1899 to 1904.

THE JUNIOR INSTITUTION OF ENGINEERS. Transactions, 1916 to 1920.

THE SOCIETY OF ENGINEERS. Transactions, 1918, 1919 and 1920.

THE INSTITUTE OF MARINE ENGINEERS. Transactions, Aug., 1915, to Feb., 1921 (in monthly parts).

THE NATIONAL PHYSICAL LABORATORY. Collected Researches, Vols. 2 to 15, 1907/1920.

THE FRANKLIN INSTITUTE (Philadelphia). Journal, 1920.

THE ROYAL METEOROLOGICAL SOCIETY. Quarterly Journal, 1910 to 1913.

THE ENGINEERING ASSOCIATION OF NEW SOUTH WALES. Proceedings, 1911/13 and 1916/17.

PERIODICALS.

The Engineer.—July, 1891, to June, 1894, inclusive.
1911 to 1920 inclusive.

Engineering.—1887 to 1899 inclusive.
1901 to 1907 inclusive.
Jan. to June, 1908.
1911 to 1920 inclusive.

The Automobile Engineer.—1911 to 1920 inclusive.

Internal Combustion Engineering.—Jan., 1913, to April, 1914.

The Autocar.—Jan., 1901, to June, 1902.
July/Dec., 1903.
1906 to 1919 inclusive.

The Motor.—Aug., 1909, to Jan., 1921, inclusive.

The Automotor Journal.—Oct., 1900, to Dec., 1902.
Jan., 1911, to June, 1919, inclusive.

Flight.—1909 to 1913 inclusive.

The American Machinist.—Jan., 1900, to June, 1903, inclusive.

Coachbuilders' Art Journal.—1909.

Light Car and Cyclecar.—Nov., 1915, to May, 1916.

Revue du Génie Militaire.—1909, two vols.

The Austin Advocate.—Vols. II. and III., 1913/14.

CLASSIFIED LIST OF VOLUMES AND PAPERS.

AUTOMOBILE ENGINEERING.

The Gasoline Automobile (Heldt).—
Vol. 1, The Gasoline Motor.
„ 2, Transmission, &c.
„ 3, Electrical Equipment.

The Gas, Petrol and Oil Engine (Clerk and Burls), two vols.

High-speed Internal Combustion Engines (Judge).
Text Book on Motor Car Engineering—Vol. 2, Design—(Graham Clark).
The Petrol Engine (Kean).
Automobile and Aero Engines (Devillers).
Internal Combustion Engines (Allen).
Scientific Determination of the Merits of Automobiles (Riedler).
Petrol Motors and Motor Cars (Hyler White).
The Two-stroke Engine (A. M. Low).
Motors in a Nutshell (Bramley-Moore).
Motors and Motoring (Spooner).
The Business Motor Handbook (G. Watson).
Motor Mechanics' Handbook (Rogers and Watson).
The Montagu Motor Book (Sherrin).
Limits of Thermal Efficiency in Internal Combustion Motors (Clerk).
Automobile Performance Formulæ (Kerr Thomas).
Some Recent Developments in Commercial Motor Vehicles (Clarkson).
The Internal Combustion Commercial Vehicle (Watt).
A New Theory of Plate Springs (Landau and Parr), Parts 1, 2, and 3.
The Care of Leaf Springs (Sheldon Axle Co.)
The Commercial Testing of Leaf Springs (Sheldon Axle Co.).
Transmission of Power by Chains (Hildage).
Liquid Fuels for Internal Combustion Engines (Moore).
Lubrication and Lubricants (Archbutt and Deeley).
Theory and Practice of Lubrication—The "Germ" Process (Wells and Southcombe).
Carburation (Brewer).
Theory and Practice of Carburation (Mantell).
Worm Gearing (Kerr Thomas).

ELECTRICAL ENGINEERING.

Electrical Engineering Practice (Meares).
Induction Coils in Theory and Practice (Austin).
Magnetos (Young).
Train Lighting Connections (Leitner).

AERONAUTICAL ENGINEERING.

Aerodynamics (Lanchester).
Aerodonetics (Lanchester).
Aircraft in Warfare (Lanchester).
The Flying Machine (Lanchester).
Dynamics of the Aeroplane (Devillers).
Art of Aviation (Brewer).
Aeroplane Rigging (Halliwell).
Aeronautical Engineering (Hele-Shaw).
Comparison of the Problems presented respectively by the Dirigible and the Aeroplane (Hele-Shaw).
The Flying Machine from an Engineering Point of View (Lanchester).
Analysis of the British Military Aeroplane Trials (Berriman).
Principles of Flight (Berriman).
Glossary of Aeronautical Terms (Faraday).

MARINE AND DIESEL ENGINEERING.

The Diesel Engine (Wells and Wallis-Tayler).
Diesel Engines (Chalkley).
Marine Oil Engines (Martin).
The Design and Construction of Oil Steamers (Montgomerie).
Electrical Power for Main Marine Propulsion (Durtnall).
Motori Diesel (Supino).
Motori Diesel (Fissi).

WORKS MANAGEMENT, ACCOUNTANCY, &c.

Factory Administration and Accounts (Elbourne).
The Costing Problem (Elbourne).
Depreciation and Wasting Assets (Leake).
The Management of Engineering Workshops (Barker).
The Question of Scientific Management (Richardson).
Fine Measurement in Engineering Workshops (Norbury Williams).

METALLURGY.

Report of the Steel Research Committee, 1920.
The Heat-Treatment of Tool Steels (Brearley).
Steels Used in Aero Work (Hatfield).
Shop Practice in respect to Aircraft Steels (Philpot).
Effect of Chromium and Tungsten upon the Hardening and Tempering of High-speed Tool Steel (Edwards and Kikkawa).
Initial Temperature and Critical Cooling Velocities of a Chromium Steel (Edwards).
Steel and its Defects from the Users' Point of View (Lantsberry).
The Testing of Steel (Brearley).
Cast Iron in the Light of Recent Research (Hatfield).
The Hardening of Metals with special reference to Iron and its Alloys (Edwards and Carpenter).
Some Properties of Ingots (A. W. and H. Brearley).
Ingots and Ingot Moulds (H. Brearley).
Ferrous Materials (Judge).
Billets for Drop Forgers (Brearley).
Some Troubles of a Drop Forger (Rowley).
Behaviour of Ductile Material under Torsional Strain (Larard).
Notes on Pyrometry from the Standpoint of Ferrous Metallurgy (Hatfield).
The Occlusion of Gases by Metals (Sir R. Hadfield).
Standardisation of Engineering Materials (Sir J. Wolfe Barry).
Practical Electric Welding (Swift).
The Brass Moulder (Purves).
Files and Filing (Fremont).
The Forging Temperature of Steels (Aitchison).
Notes on the Manufacture of Drop Forgings (Stubbs).

MISCELLANEOUS.

Machinery in Agriculture, with Notes on Lubrication (Veitch Wilson).
Oil Testers and How to Use Them (Veitch Wilson).

Nomography (Hezlet).
Calcul Graphique et Nomographie (M. d'Ocagne).
Dynamometers (Jervis-Smith).
Typecasting and Composing Machinery (Legros).
Molesworth's Pocket Book of Engineering Formulæ.
High-explosive Shell Manufacture (Hamilton).
Health of the Munition Worker (Ministry of Munitions).
Fatigue Study (Gilbreth).
The Motor Movement (Rees Jeffreys).
Choosing a Boy's Career (Devine).
Indexing (Brown).
Construction and Wear of Roads (Mallock).
Coal Resources of the United Kingdom: Economy and Waste (Lupton).
Industrial Engineering: Present Position and Post-war Outlook (Lanchester).
Practical Lessons of the War (*The Engineer*).
Composition of Power Gases (Tookey).
Science Abstracts (Physics and Electrical Engineering), two vols.
Industrial Fatigue in its relation to Maximum Output (Spooner).
Memoir of James Robson.
Standardisation and its relation to the Trade of the Country (Sir J. Wolfe Barry).
Standardisation and its Influence on Engineering Industries (Unwin).
A Record of Opportunity as to Careers and Training—Ministry of Labour Handbook.
Motor Union Legal Handbook.
C.M.U.A. Handbook of Approved Depôts.
C.M.U.A. Handbook of Night Shelter Accommodation.
Practical Treatise on Gearing (Brown and Sharpe Manufacturing Co.).
Practical Treatise on Milling (Brown and Sharpe Manufacturing Co.).
Construction and Use of Universal Grinding Machines (Brown and Sharpe Manufacturing Co.).
Construction and Use of Automatic Screw Machines (Brown and Sharpe Manufacturing Co.).
America's Munitions, 1917/18 (Crowell).

PUBLICATIONS OF THE CANADIAN DEPARTMENT OF MINES.

An Investigation of the Coals of Canada (Porter and Durley), two vols.
Location and Examination of Magnetic Ore Deposits (Haanel).
Mineral Production of Canada—Reports for 1914, 1916, 1917, 1918, 1919 (McLeish).
Value of Peat Fuel for Generation of Steam (Blizard).
Test of Some Canadian Sandstones to Determine Suitability as Pulpstones (Cole).
Iron Ore Occurrences in Canada (Lindeman and Bolton), two vols.
Mineral Springs of Canada (Satterley and Elworthy), two vols.
Investigation of the Peat Bogs and Peat Industry of Canada, 1910/14 (Anrep).
Report on the Explosives Industry in Canada, 1911 (Desborough).

Proceedings of Conference on Proposed Legislation to Regulate Manufacture, Importation and Testing of Explosives (1911).
Exploitation of Peat Bogs for the Production of Fuel for Domestic and Industrial Purposes (Haanel).
Peat, Lignite and Coal: Their Value as Fuels for Production of Gas and Power in the By-Product Recovery Producer (Haanel).
Products and By-Products of Coal (Stansfield and Carter).
Report of Analyses of Ores, Non-metallic Minerals, Fuels, &c., 1906/8 (Wait).
Description of the Laboratories of the Mines Branch, Ottawa, 1916.
Results of Investigation of Six Lignite Samples from Alberta (Haanel and Blizard).
Petroleum and Natural Gas Resources of Canada (Clapp and others), two vols.
Economic Minerals and Mining Industries of Canada, 1914.
Report on Utilisation of Peat Fuel for Production of Power (Haanel).
Graphite (Spence).
Report on Some Sources of Helium in the British Empire (McLennan).
Results of Forty-one Steaming Tests (Blizard and Malloch).
Report on Building and Ornamental Stones of Canada.
Potash Recovery at Cement Plants (Wilson).
Analyses of Canadian Fuels (Stansfield and Nicolls), five parts.
Summary Report of Mines Branch, 1916, 1917, 1918, 1919.
Smelter Treatment Rates, 1919.

PUBLICATIONS OF THE SMITHSONIAN INSTITUTION, WASHINGTON.

Hydromechanic Experiments with Flying Boat Hulls (Richardson).
Bibliography of Aeronautics (Brockett).
Researches and Experiments in Aerial Navigation (Langley).
Reports on Wind Tunnel Experiments in Aerodynamics.
Stability of Aeroplanes (Orville Wright).
The First Man-carrying Aeroplane (Zuhm).
Dynamical Stability of Aeroplanes (Hunsaker).
The Mechanics of the Earth's Atmosphere (Abbe).
Aviation in France in 1908 (Jourdain).
On the Positions Assumed by Birds in Flight (Beetham).
Travelling at High Speeds on the Surface of the Earth and Above it (Hele-Shaw).
Holes in the Air (Humphreys).
Review of Applied Mechanics (Lecornu).
International Air Map and Aeronautical Marks (Lallemand).
Recent Progress in Aviation (Chanute).
Langley Memoir on Mechanical Flight. Part 1 (Langley), Part 2 (Manly).

REPORTS, &c.

Technical Reports of Advisory Committee for Aeronautics, 1910 to 1913.

Advisory Committee for Aeronautics—
Report on Tests of Petrol Motors in Alexander Motor Prize Competition, 1911.
Abstracts of Papers, Nos. 1 to 19.
National Physical Laboratory, Reports, 1905 to 1920.
First Annual Report of National Advisory Committee for Aeronautics, U.S.A., 1915.
Fuel for Motor Transport—Interim Memoranda by Fuel Research Board, 1920.
Report of the Fuel Research Board for the years 1918 and 1919.
Report of the Lubricants and Lubrication Enquiry Committee, 1920.
Official Report of Auto-Cycle Union Six Days' Reliability Trial, 1919.
Report on the Lincoln Tractor Trials, 1919 (S.M.M.T.).
Report on Benzole Road Tests, 1919 (A.A. and M.U.).
British Engineering Standards Association—Reports of Work Accomplished, Nos. 3 to 13.
National Physical Laboratory, Metrology Department, Gauge Testing, 1921.
Report on the Compulsory Adoption of the Metric System in the United Kingdom (Conjoint Board of Scientific Societies, Metric Committee).

BOOKS OF REFERENCE.

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Encyclopædia Britannica, Ninth Edition, with additional volumes of 1902 Edition, 35 vols.
Knight's Dictionary of Mechanics, 1884, four vols.
Ure's Dictionary of Arts and Manufactures, 1878, four vols.
Dictionary of Technical Terms in French, German, Spanish, Italian, &c.
DIRECTORIES.
The Business Directory, London and Provinces, 1921.
The Motor Car Red Book, 1912 to 1920.
The "Trader" Handbook, 1920.
Directory of Shipowners, Shipbuilders and Marine Engineers, 1915.
"Engineering" Directory.
"Engineer" Directory.
British Engineers' Association, Official Directory of Members, 1917, 1918/19, 1920.
Directory of Danish Exporters, Importers, &c., 1919.
Advertisers A.B.C., 1921.
YEAR BOOKS, &c.
Automobile Engineer Year Book, 1920.
American Society of Mechanical Engineers, Year Books, 1910 to 1919.
Society of Automotive Engineers, Membership Roster, 1919/20.
Institute of Metals, List of Members, 1920.
Society of Engineers, List of Members, 1919.
Institution of Engineers in Australia, Constitution, 1919.
Institution of Engineers (India), Memorandum and Articles of Association.

A.B.C. Motor, Marine and Aircraft Guide, 1921.
Who's Who in Engineering, 1921/2.
Institute of Transport, Articles of Association.
Royal Society of Arts, List of Fellows, 1919/20.
Royal Automobile Club Year Books, 1907/16, 1921.
University of Ireland, Calendar, 1919.
University of Hong-Kong, Calendar, 1919, 1920.
Bradford Technical College, Calendar, 1920/21.

List of Papers Read before the Graduates' Section Available on Loan.

"Notes on the Suitability and Availability of Motor Spirit," W. D. Pile.
"Factors Affecting Power Output," E. Chatterton and L. F. Watson.
"Applied Time Study," R. V. Newton.
"Carburation," T. E. B. Whiting.
"Accessibility," J. T. Hacking.
"Two-stroke *v.* Four-stroke Motors," T. R. Florance.
"Cylinder and Piston Wear," W. H. Wardall.
"Repair and Maintenance of H.T. Magnetos," L. Sterland.
"Some Problems we Meet," L. H. Dawtrey.
"Relative Advantages of Worm and Spiral Bevel Final Drive for Automobiles," W. H. Prestage.
"Engine Lubrication," E. L. Bass.
"Valve-operating Mechanisms and their Effect upon Timing," C. Pole Wedmore.
"The Problem of Road Illumination for Automobiles," L. Griffiths.
"Modern Tendencies in Automobile Design," H. B. Benny and D. J. Macklin.
"Factory Planning and Design," T. R. Speck.
"Current Motor Cycle Frame Design," G. M. Martineau.
"Lubricating Systems," T. Burkett.

ON THE LIBRARY TABLE.

The Engineer.
Engineering Production.
The Industrial Engineer.
The Automotor Journal.
The Motor World.
The Motor Cycle and Cycle Trader.
The Motor Boat.
Aeronautics.
Engineering and Industrial Management.
Mechanical World.
Everyday Science.
Gas and Oil Power.
Faraday House Journal.
Journal of Industrial Administration.
Engineering.
Engineering World.
The Autocar.
The Car Illustrated.
Motor Transport.
The World's Carriers.
Flight.
The American Machinist.
British Engineers Home and Export Journal.
Coopers Vehicle Journal.
Fire.

Coventry Engineering Society's Journal.
The Motor Ship (U.S.A.).
The Woman Engineer.
Australian Official Journal of Patents.
The Automobile Engineer.
The Practical Engineer.
The Motor.
The Light Car and Cyclecar.
The Motor Trader.
The Commercial Motor.
The Motor Ship.
The Aeroplane.
Machinery.
Illustrated Official Journal of Patents.
The Technical Review.
The Motor Cycle.
Motor Cycling.
Tramway and Railway World.
B.E.A.M.A. Journal.
Institute of Transport Journal.
Journal of Royal Society of Arts.
Automotive Industries (U.S.A.).
South African Engineer.
S.A.E. Bulletin (U.S.A.).
Journal of the Franklin Institute (U.S.A.).

ANNUAL REPORT

AND

ACCOUNTS.

THE

INSTITUTION OF AUTOMOBILE ENGINEERS.

Report of the Council presented at the Annual General Meeting on March 2nd, 1921.

The Council beg to present to the Members the following Report of the activities and progress of the Institution for the year 1920.

Financial Position.

For the first time since 1910 the Council find themselves in the position of presenting a statement of accounts in which the balance is on the wrong side. It will be remembered, however, that in the Report presented last year it was anticipated that the increased expenditure arising from the appointment of a whole time Secretary would, when taken in conjunction with rising expenditure in other directions, "severely strain the resources of the Institution." The strain, however, has not been carried to the breaking point, and there still remains a balance on the credit side of the Accumulated Fund Account amounting to £135 12*s.* 0*d.*, in addition to £585 invested on account of Life Composition Fees. It is nevertheless obvious that the all-round increase of half a guinea in the rate of subscription which the Council felt compelled to ask Members to submit to was an absolute necessity, and thanks to the loyal manner in which the Members have accepted that additional burden and to the wonderful increase in Membership during the past year, the future may be looked forward to without any undue anxiety. The Council have, in fact, every hope that the additional revenue which will accrue from these two sources will be sufficient to show a small credit balance at the end of the year, even if it is found impossible to make any reduction in expenditure under any of the various headings. It may here be pointed out that the outlay in the case of the last volume of Proceedings was exceptionally heavy, although, owing to the overlapping of the volumes and the financial year this fact is not brought out in the Income and Expenditure Account. For instance, in the Accounts for the year 1919 the amount shown as actually spent on "Printing Proceedings" is £1,291 odd, but of that amount some £400 was on account of the previous volume, which was not issued until well into the year instead of in the previous October, and thus the actual cost of the volume for the year 1919 was roughly only some £900. On

the other hand, the amount of £1,211 shown under this heading in the present Balance Sheet does actually represent very closely the cost of Vol. XIV. Since this volume is much the largest ever issued, it is evident that there is a possibility of a reduction under this heading if Vol. XV. happens to be smaller, though no deliberate attempt will be made in that direction by limiting the length of papers submitted to the Institution.

Members may doubtless be puzzled to observe that in spite of the large increase in Membership already referred to, the amount received under the heading of Subscriptions is actually less in 1920 than in 1919. This is due to the exceptionally large amount of £416 which was received as arrears in 1919 (as against £146 in 1920) to which attention was drawn in last year's Report, and to the fact that the majority of the new Members were elected during the last three months of the year. The subscriptions of such Members, which amount to £509, are not credited to the year 1920 at all, but are shown as paid in advance for 1921.

TABLE.

	Hon. M.	M.	A.M.	G.	A.	Totals.	
Totals at Dec. 31, 1919.	3	408	562	204	70		**1247**
Additions during 1920..	—	47	122	124	11		
Transfers	—	27	15	—	—		
Total additions during 1920	—	74	137	124	11	**346**	
Deceased.....	—	2	2	—	1		
Resigned	—	6	4	2	—		
Transferred to Higher Grade	—	—	27	15	—		
Removed from list.....	—	2	4	8	2		
Total deductions during 1920	—	10	37	25	3	**75**	
Nett alterations during 1920	—	+64	+100	+99	+8		**271**
Totals at Dec. 31, 1920.	**3**	**472**	**662**	**303**	**78**		**1518**

Membership.

The Council are pleased to be able to report that the increase in Membership for the year is the greatest in the history of the Institution, amounting to no less than 271 of all grades, which brings the total Membership up to 1,518. In this connection it may be of interest to recall that it required seven years, from 1910 to 1917, to give an increase of 500, from 530 to 1073, whereas the next increment of 500 has only taken three years. The Council are particularly gratified to note the large increase in the number of Graduates who have joined the Institution, while the number of resignations remains at a satisfactorily low figure. The Council feel justified in accepting this record as an indication that the affairs of the Institution have been conducted on the lines which are best calculated to make it of the greatest use to the Members. They have, however, no intention of "resting on their oars," and are at all times most anxious to receive suggestions from the Members for increasing the value of the Institution in any direction.

Obituary.

The Council desire to tender their sincere sympathy to the relatives of those Members who have passed away during the year. Among these, Mr. W. G. B. Dollond, who was at one time Chairman of Committee of the London Graduates Section, died as a direct result of the hardships to which he was subjected while in His Majesty's Service in the War.

Examination.

The imposition of an examination qualification for admission to the Institution on all those born since January 1st, 1890, has had no deterrent effect upon the number of applications for Membership, and has undoubtedly been a very great assistance to the Council in coming to a decision as to the qualifications of a number of the candidates. It should be pointed out that the intention of the examination is by no means to keep down the Membership, but to encourage applicants to educate themselves up to the desired standard.

Increase in Subscriptions.

When dealing with the financial situation in 1919, the Council had every hope that the imposition of an entrance fee on new

Members would provide sufficient funds to meet the heavy increase in expenditure which was anticipated for the year 1920. As will be seen from the Balance Sheet, this anticipation has not been realised, and toward the end of the year the Council found it necessary to ask the Members to agree to an increase of half a guinea in the subscription of all grades of Membership; this amount was thought to be the minimum that would give the desired balance on the right side and just meet the expenditure which would be necessary, unless the activities of the Institution were to be restricted. A further reason for this step was that for some time negotiations had been in progress with the Society of Motor Manufacturers and Traders for a grant from that body to enable the Institution to be of even greater use to its Members and to the Industry generally, and it was a condition of such a grant that the Institution should be able to pay its way on the present basis. These negotiations are now practically completed, and it is hoped that still more meetings than ever will be held in 1921, and that other work of a useful character will be undertaken. The full details will be laid before the Members in due course, but it may be said here that the whole of the grant will be expended in enlarging the activities of the Institution.

Olympia.

The Council would like to record their very grateful thanks to the Society of Motor Manufacturers and Traders for the privilege which was accorded to the Members of the Institution of visiting Olympia on the Special View Day for both the Motor and Commercial Vehicle Shows. Such a privilege (of which a very large number of the Members availed themselves) gives quite a unique value to Membership of the Institution.

Meetings and Papers.

The increase in the number of Meetings from eight to twelve, eight in London and four in Birmingham, and the curtailing of the time taken in the reading of the papers, appear to have stimulated the interest taken in the Meetings, and the attendances have been very good. The Council would, however, like to point out that it is only by the continued interest of each individual Member that the standard of papers can be kept up to the high pitch of excellence of recent years. Although a Member may be unable for some reason to read a paper himself, he may, by keeping his eyes and ears open, find himself in the position of

being able to bring to the notice of the Council some interesting line of research which would be of great value to the Industry if put forward as a paper before the Institution, and the Council will be very grateful for assistance in this direction. It may not be realised that the preparation of a Season's programme of papers begins more than a year in advance, so that any suggestions should be forwarded without delay.

Scottish Centre.

The Council are very glad to report that the Scottish Centre continues to do very good work, and that the attendances at the Meetings indicate to the full the need for its existence. It is with great regret that they have to announce that Mr. G. E. McCaw, who has acted as Honorary Secretary of the Centre during the last six very difficult years, has found himself unable any longer to carry out the duties of that office. They desire to record their very sincere appreciation of the services which Mr. McCaw has rendered to the Institution.

Five Meetings have been held in Glasgow during the year.

Graduates' Centres.

The very great importance of the Graduates' Centres is fully appreciated by the Council, and it is with the utmost satisfaction that they are able to report that the Birmingham Centre has been resuscitated under most auspicious circumstances, while the total number of Graduates joining the Institution during the year and the attendances at the Meetings of the three Centres have exceeded all expectations. The Coventry Centre has once again carried off the Graduates' Prize and the Daimler Premium, making a record upon which they are to be heartily congratulated, and one which it will tax the energies of the other Centres to the utmost to beat. The Council wish to record their sincere thanks to the Committees and Local Honorary Secretaries of all the three Centres for their splendid work during the year.

The number of Meetings held during the year in each of the Graduates' Centres is as follows:—

London	8
Coventry	13
Birmingham	4

In addition to the above, seven Visits to Works were held in London.

Wolverhampton Centre.

The Members in the Wolverhampton district have held a number of very successful Meetings during the year at which papers by local Members have been read and discussed. The interest appears to be increasing day by day, and the Centre promises to be a very important field of activity and is deserving of the strongest support.

Annual Dinner.

It is most unfortunate that the threatened miners' strike caused the postponement of the first Annual Dinner to be organised by the Institution.

Informal Dinners.

In the endeavour to afford every possible facility for friendly intercourse among Members, a series of Informal Dinners has been arranged during the present Session to take place before the ordinary monthly Meetings. Here the Members can meet in the most informal manner and can seat themselves just as they like, so that a Graduate may find himself next to the President. Under such circumstances, Members are afforded unique opportunities of becoming better acquainted with each other, and of discussing to their mutual benefit problems of a character which would not be suitable for discussion at the more formal monthly Meetings. As the majority of the Members have to dine somewhere before the Meetings, they are urged to make every effort to attend these Informal Dinners and make them an even greater success than they have hitherto proved to be.

Steel Research Report.

The Council are pleased to be able to announce that the Report of the Steel Research Committee on the investigation to establish the mechanical properties of the ten standard automobile steels has now been published. The question of the proper steels to be used in automobile construction is one of such vast importance to the automobile engineer that every Member of the Institution should be in possession of a copy of this Report. The scope and nature of the Report have already been laid before the Members in the paper read by Mr. J. H. S. Dickenson in November. The price of the Report, which may be obtained from the offices of the Institution, is £1 13*s.* 0*d.* per copy, post free.

The Council would like to convey their most hearty congratula-

tions and cordial thanks to the Research Committee and to the Executive Research Sub-Committee on this most satisfactory conclusion of their very arduous task.

Summer Visit.

The first Summer Visit since the war was probably the most successful and enjoyable that has been held. The realisation of the importance of steel and steel alloys, due in large measure to the war, almost naturally led to the selection of Sheffield as the centre, and the city was found to be ready to extend the most cordial invitation to the Members, every one of the steel works being thrown open to their inspection. A Reception Committee was also formed locally, which entertained the Members generously and made excellent arrangements for their convenience throughout the visit. The hearty thanks of the Council are due to this Committee. Over 100 Members took part in the visit, and though it called for a draft on the funds of the Institution which could ill be afforded, the Council feel that the expenditure was fully justified both by the amount of information obtained by those who took part in the visit and the amount of publicity that the Institution obtained from the Press, to whom the Council desire to accord their sincere thanks.

Standardisation.

The lull in the work of standardisation of automobile parts, due to the disbanding of the Technical Committee, has now come to an end, and a strong Sectional Committee of the British Engineering Standards Association was formed at the end of the year on which the Institution is well represented, and of which the Secretary of the Institution is the Honorary Secretary. It is hoped that the year 1921 will see the work pushed forward with the greatest energy, and that much of the standardisation which has already been initiated will be brought to a successful conclusion.

Proceedings.

The Volume of Proceedings completed in October is the largest which has ever been issued, and contains a number of papers of the very highest value to the automobile engineer. The cost, as mentioned elsewhere, has been very high, but the Council have always looked upon this item as the one upon which no reasonable

expense should be spared, and the expenditure is very carefully watched throughout the Session. The Council propose in future to make a small saving by issuing the List of Members and the Articles of Association as a separate pamphlet, to be obtained by the Members only on special application; a copy of the List of Members and the Articles will be bound up in the Proceedings only every fifth year.

Corps Pay.

The Council very much regret that they are not able to report any satisfactory arrangement with the War Office in regard to Corps Pay, but the matter is still under negotiation, and no efforts have been or will be spared to obtain this benefit for the Members entitled to it.

Election to Council.

The steps taken during the year to increase the interest of Members in the election of Members of Council proved to be most successful, and no less than 38 per cent of the Members actually recorded their votes, a percentage which it is believed is considerably higher than in any other Institution. The Council sincerely trust that the interest which has thus been awakened may not be allowed to flag, and that all Members will realise the important part which each one can play in the conduct of the affairs of the Institution, both by nominating those whose services will be of value and by themselves voting and stirring up their friends to vote also.

Appointments Bureau.

The Institution has not been able to be of so much use to Members looking for a fresh appointment as in past years owing to the very few vacancies which have presented themselves during the year. No effort, however, has been or will be spared in this direction.

Graduates' Prize.

The winner of the Graduates' Prize, the value of which is £5 5*s*. 0*d*., was Mr. D. L. Prior with his paper entitled "Epicyclic Gearing in Automobile Practice," while the Second Prize of

£2 2*s*. 0*d*. was awarded to Mr. W. E. Benbow for his paper on "Chemical and Physical Properties of Iron and Steel."

Crompton Medal.

The Crompton Medal for the best paper read during the Session 1919—20 has been awarded to Mr. A. P. Young and Mr. H. Warren for their paper on "The Process of Ignition."

Daimler Premium.

The Daimler Premium of £25 offered by the Daimler Co., Ltd., for the best paper written by a Graduate of the Institution during the Session 1919—20 was unanimously awarded by the judges to Mr. D. L. Prior for his paper on "Epicyclic Gearing in Automobile Practice."

Additions to Library.

The thanks of the Council are tendered to the following for publications received during the year for addition to the Library of the Institution:—

Institution of Mechanical Engineers—Proceedings, Oct.—Dec., 1919, and Jan.—April, 1920.
Society of Automotive Engineers, U.S.A.—Transactions, Part 2, 1918.
" " " " Part 1, 1919.
American Society of Mechanical Engineers—Transactions, Vol. 41.
Institution of Civil Engineers—Proceedings—Vol. CCVI.
" " " List of Members, February, 1920.
" " " Some Aspects of Metropolitan Road and Rail Transit (H. H. Gordon).
Institute of Metals—Journal, Vol. XXII.
North-East Coast Institution of Engineers & Shipbuilders—Transactions—Vols. 35, 36.
Society of Engineers—Transactions, 1919.
Junior Institution of Engineers—Transactions—Vol. XXIX.
E. & F. N. Spon, Ltd.—"Practical Electric Welding" (H. Bevan Swift).
" "Dynamics of the Aeroplane" (R. Devillers).
Sir I. Pitman & Sons, Ltd.—"Aircraft & Automobile Materials—Ferrous" (Judge).
The Library Press, Ltd.—"Factory Administration & Accounts" (Elbourne).
" —"The Costing Problem" (Elbourne).
National Physical Laboratory—Collected Researches—Vol. XIV.
Steel Research Committee—"Report of Research on Standard Automobile Steels."
Automobile Association—Official Report on Benzol Road Tests.
Society of Motor Manufacturers & Traders—Report of Lincoln Tractor Trials.

Temple Press, Ltd.—"The Motor"—Vol. 38.
Auto-Cycle Union—Report of six days Reliability Trials.
Hong-Kong University—Calendar, 1920.
Canadian Department of Mines:—
Production of Coal and Coke in Canada, 1918.
Annual Report on Mineral Production of Canada, 1918.
Summary Report for 1918.
Production of Copper, &c. in Canada in 1918.
Association of Drop Forgers and Stampers:—
"Steel and its Defects" (F. C. Lantsberry).
"Billets for Drop Forgers" (Hy. Brearley).
James Watt, Esq.—"The Internal Combustion Commercial Vehicle" (Jas. Watt).
Hy. Brearley, Esq.—"Files and Filing" (Fremont).
Prof. H. J. Spooner—"Industrial Fatigue in its Relation to Maximum Output" (Spooner).
F. W. Halliwell, Esq.—"Aeroplane Rigging" (Halliwell).
Technical Publishing Co.—"The Motor Car Red Book."
Dr. H. S. Hele-Shaw—Reports of Royal Meteorological Society.
T. C. Harrison, Esq.—Proceedings of the Institution of Electrical Engineers.
Comptroller-General of the Patent Office—"Illustrated Official Patents Journal."
Comptroller-General of Australian Patent Office—"Australian Official Journal of Patents."
Royal Society of Arts—Weekly Journal.
Institute of Marine Engineers—Monthly Transactions.
Franklin Institute—Journal.

Also to the proprietors of the following journals for kindly presenting copies throughout the year for the Library Table:—

"The Engineer," "Engineering," "The Automobile Engineer," "The Practical Engineer," "Engineering Production," "The Motor," "The Autocar," "Car Illustrated," "Automotor Journal," "Motor Trader," "Light Car & Cyclecar," "Cooper's Vehicle Journal," "Cycle & Motor Cycle Trader," "The Autocycle," "The Motor Boat," "The Motor Ship," "Flight," "Aeronautics," "The Aeroplane," "Gas & Oil Power," "Tramway World," "Fire," "B.E.A.M.A. Journal," "American Machinist," "S.A.E. Bulletin," "South African Engineering," "Cassier's Engineering," "Industrial Engineer," "Foundry Trades Journal," "Mechanical World," "Machine Tool Review," "British Engineers Export Journal," "The Power User," "Technical Review," "The Metal Industry," "Popular Science," "Automotive Industries."

BALANCE SHEET

AND

INCOME AND EXPENDITURE ACCOUNT.

THE INSTITUTION OF AUTOMOBILE ENGINEERS.

BALANCE SHEET.—31*st December*, 1920.

LIABILITIES.	£ s. d.	£ s. d.	31st December, 1919. £ s. d.
To Sundry Creditors		412 13 9	*569 11 1*
„ Annual Dinner		84 6 0	... ...
„ Subscriptions received in advance for 1921		509 17 6	*173 12 6*
„ Prize Fund		201 18 6	*201 18 6*
„ Life Composition Fund		585 0 0	*445 0 0*
„ Income and Expenditure Account—			
Balance at 1st January, 1920	552 5 5		
Less: Deficit for Year ended 31st December, 1920	416 13 5		
		135 12 0	*552 5 5*
		£1,929 7 9	*1,942 7 6*

ASSETS.	£ s. d.	£ s. d.	31st December, 1919. £ s. d.
By Cash—			
Deposit Account	200 0 0		... ...
Current Account	286 16 3		*6 6 9*
In Hand	0 2 4		
		486 18 7	
„ Sundry Debtors		3 5 0	*21 45 0*
„ Office Furniture and Fittings—			
Balance at 1st January, 1920	130 13 5		
Additions	87 8 0		
	218 1 5		
Less: Depreciation at 15 per cent.	32 14 3		
		185 7 2	*130 13 5*
„ Stock of Proceedings at Secretary's Valuation		41 12 0	*38 12 0*
„ Stock of Badges at Secretary's Valuation		16 5 0	*7 1 9*
„ Investments, at or under Cost—			
£1,000 5 per cent. War Loan, 1929-47	950 0 0		
£100 Institute of Mechanical Engineers 4 per cent. Debenture	100 0 0		
£200 Canadian Government 3½ per cent. Stock	146 0 0		
		1,196 0 0	*1,196 0 0*
		£1,929 7 9	*1,942 7 6*

I have prepared and audited the Balance Sheet of the Institution of Automobile Engineers dated the 31st day of December, 1920, as above set forth, and have obtained all the information and explanations I have required. In my opinion, such Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Institution's affairs according to the best of my information and the explanations given me, and as shown by the Books of the Institution.

1, Queen Victoria Street, London, E.C.4.
5th January, 1921.

HY. FOWLER, Chairman of Finance Committee.
GEO. WATSON, Member of Finance Committee.
BASIL H. JOY, *Treasurer.*

(Signed) J. STEWART MALLAM,
Chartered Accountant.

INCOME AND EXPENDITURE ACCOUNT.

For Year ending 31st December, 1920.

EXPENDITURE.	£ s. d.	31st December, 1919. £ s. d.
To Expenses of Meetings	107 19 4	*119 3 10*
,, Examination Expenses	27 15 0	.....
,, Summer Visit	44 0 0	.....
,, Postages and Telegrams	238 3 2	*170 13 3*
,, Stationery and Printing	300 18 10	*231 17 2*
,, Sundry Expenses	32 10 2	*28 5 7*
,, Reporting Meetings	67 7 4	*55 3 4*
,, Printing Proceedings	1,211 13 9	*1,291 19 5*
,, Salaries, Office Rent, &c.	1,673 4 2	*1,028 15 8*
,, Coventry Branch Expenses	29 19 5	*43 15 0*
,, Scottish Section Expenses	64 7 6	*19 13 10*
,, Wolverhampton Branch Expenses	4 15 5	*1 13 6*
,, Birmingham Branch Expenses	16 6 2	.....
,, *November Session Expenses*	—	*6 7 8*
,, Audit Fee	10 10 0	*10 10 0*
,, Legal Expenses	1 1 0	.....
,, Telephone Charges	14 17 4	*13 5 2*
,, Depreciation on Office Furniture and Fittings	32 14 3	*23 1 2*
,, Graduate Prize	7 7 0	*8 9 3*
,, *Technical Committee*	—	*15 14 2*
,, Steels Committee	5 13 11	.....
,, *Balance, Surplus on Year*	—	*86 18 6*
	£3,891 3 9	*3,155 6 6*

INCOME.	£ s. d.	31st December, 1919. £ s. d.
By Entrance Fees	460 19 0	*187 19 0*
,, Subscriptions	2,716 16 0	*2,749 10 1*
,, Sale of Publications	148 14 3	*117 0 10*
,, Certificates (Registration Fees)	50 7 0	*42 19 0*
,, Interest on Investments	71 1 6	*43 8 0*
,, Sundry Receipts	21 14 3	*13 8 4*
,, Profit on Sale of Badges	4 18 4	*1 1 3*
,, Balance: Deficit for Year	416 13 5	.....
	£3,891 3 9	*3,155 6 6*

LIST OF MEMBERS.

OBITUARY.

The Council deeply regret to have to record the loss of the following Members:—

A. S. Hill .....Member.
R. Reid .....Associate Member.
T. Ford .....Graduate.

The Institution of Automobile Engineers.

OFFICERS.

1920–21.

President:

SIR HENRY FOWLER, K.B.E. ..... Derby.

Vice-Presidents:

Lt.-Col. D. J. SMITH ..... London.
GEO. W. WATSON ..... London.

Members of Council:

Sir HERBERT AUSTIN, K.B.E. ..... Birmingham.
Brig.-Gen. BAGNALL-WILD, C.M.G. ..... London.
A. E. BERRIMAN ..... Coventry.
HY. BREARLEY ..... Sheffield.
Lt.-Col. T. B. BROWNE ..... London.
H. G. BURFORD ..... London.
T. CLARKSON ..... Chelmsford.
Capt. J. S. CRITCHLEY ..... London.
Col. R. E. CROMPTON, C.B. ..... London.
J. H. DICKENSON ..... Sheffield.
Sir ROBT. HADFIELD, Bart., D.Sc., F.R.S. ..... London.
J. F. HENDERSON ..... Glasgow.
L. H. HOUNSFIELD ..... London.
W. J. IDEN ..... London.
F. W. LANCHESTER ..... London.
G. H. LANCHESTER ..... Birmingham.
MAX R. LAWRENCE ..... London.
G. DOUGLAS LEECHMAN ..... London.
L. A. LEGROS ..... London.
F. L. MARTINEAU ..... London.
Lt.-Col. J. S. NAPIER ..... Coventry.
Lt.-Col. M. O'GORMAN ..... London.
Dr. W. R. ORMANDY ..... London.
T. C. PULLINGER ..... Dumfries.
A. A. REMINGTON ..... Birmingham.
H. R. RICARDO ..... London.
A. J. ROWLEDGE ..... London.
G. J. SHAVE ..... London.
Major B. W. SHILSON ..... Coventry.
F. R. SIMMS ..... London.
H. KERR THOMAS ..... London.
W. J. THOMSON ..... Edinburgh.
Major CHAS. WHEELER ..... London.
E. B. WOOD ..... London.
F. G. WOOLLARD ..... Birmingham.
T. H. WOOLLEN ..... London.

Hon. Treasurer:

W. REES JEFFREYS.

Secretary and Treasurer:

BASIL H. JOY, 28, Victoria Street, Westminster, S.W.

Telegraphic Address: "Autinst, Vic., London." *Telephone:* 6076, *Victoria.*

The Institution of Automobile Engineers.

COMMITTEES OF THE INSTITUTION.

Midland Centre Committee.

Chairman: G. H. LANCHESTER.

Hon. Secretary: MAJOR B. W. SHILSON.

F. A. S. ACRES.
J. E. COOPER.
D. S. HEATHER.
J. P. HILLHOUSE.
F. G. WOOLLARD.

North of England Centre Committee.

Chairman: A. T. J. KERSEY.

Hon. Secretary: H. ALBAN BRAYSHAW.

W. W. CONSTANTINE.
A. KENYON.
J. OKILL.
W. E. PHILLIPS.
H. W. ROBERTS.
G. E. SCHOLES.
C. TURTLE.
W. D. WILLIAMSON.

Scottish Centre Committee.

Chairman: D. KEACHIE.

Hon. Secretary: G. H. CUTBUSH.

J. F. HENDERSON.
G. E. McCAW.
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J. RICHARDSON.
R. J. SMITH.
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W. J. Iden.
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L. A. Legros.

HONORARY MEMBERS.

1919. †Prof. H. L. Callendar, M.A., LL.D., F.R.S., Imperial College of Science, Exhibition Road, S. Kensington, S.W. (Elected an Associate in 1907.)

1913. Howard Marmon (*President of the Society of Automobile Engineers*, 1913)—Nordyke & Marmon Co., Indianapolis, U.S.A.

1916. Major W. G. Wilson, Charton, Farningham, Kent.

The Institution of Automobile Engineers.

M.=Member. | G.=Graduate.
A.M.=Associate Member. | A.=Associate.

The names printed in clarendon type are members of the Council.

† = Has read a Paper before the Institution.
‡ = Has read a Paper before the Cycle Engineers Institute.
* = Has read a Paper before the Graduates Section.
c = Awarded the Crompton Medal.
d = Awarded the Daimler Premium.
g = Awarded the Graduates Prize.
l = Life Member.

Date of Election.			
1919.		ABRAHAM, A.	Woodcote, Four Oaks, Birmingham.
† 1911.	A.M.	ACRES, F. A. S.	Star Engineering Co., Ltd., Wolverhampton.
1919.	M.		
1907.		ADAM, M. A.	57, Lincoln's Inn Fields, W.C.2.
1918.		ADAM, W. F.	3, Kitson Road, Barnes, S.W.13.
†* 1908.	G.	AINSWORTH, Lieut. B.	c/o Locomotive Company of America,
1913.	A.M.	W.	Bridgeport, Conn., U.S.A.
1917.	M.		
† 1920.		AITCHISON, Dr. L. ...	Amalgams Co., Ltd., New Eldon Chambers, Cherry Street, Birmingham.
1912.		ALDEN, HERBERT W. ..	Detroit, Mich., U.S.A.
1919.		ALSTON, C. H. T.	St. Albans, Stirling.
† 1907.		ARTER, A. M.	15, Cecil Court, Redcliffe Gardens, S.W.10.
‡ 1907.		**Austin, Sir Herbert,** K.B.E., M.P.	Austin Motor Co., Northfield, Birmingham.
1907.		AVELING, T. C.	91, New Canal Street, Birmingham.
1917.	A.M.	ATKINSON, R. M.	43, Ossington Street, Bayswater, W.2.
1920.	M.		
1919.		BACHMAN, B. B.	The Autocar Co., Ardmon, Penn., U.S.A.
†**c* 1910.		**Bagnall-Wild, Brig.-Gen. R. K.,** C.M.G., C.B.E.	15, Acacia Road, St. John's Wood, N.W.8.
1907.		BAGULEY, E. E.	Baguley Cars, Ltd., Burton-on-Trent.
† 1909.		BAILEY, R.W. HARVEY, M.B.E.	The Bungalow, Sunny Hill, Derby.
† 1907.		BAILLIE, G. H., M.B.E.	82, Victoria Street, London, S.W.1.
1909.	G.	BALE, Major F. H. ...	17, St. Andrew's Road, Golders Green,
1910.	A.M.		N.W.4.
1915.	M.		
1911.		BAMBER, H. W.	166, Piccadilly, W.1.
1918.		BARNES, F. W.	Rex House, St. Andrew's Square, Surbiton.
1919.		BARNWELL, E. A.	38, Bower Mount Road, Maidstone, Kent.
1917.		BATENBURG, P. J. F. ..	1654, Wisconsin Street, Racine, Wisconsin, U.S.A.

Date of Election.			
1918.		BAUME, H.	Cubitts Engineering Co., Ltd., Aylesbury.
1919.		BEAUCHAMP, W. H. ...	50, Dora Road, Small Heath, Birmingham.
1909.		BEAUMONT, Major E. G. E.	10, Bentinck Mansions, Bentinck Street, W.13.
‡ 1907.		BEAUMONT, W. WORBY.	222, Strand, London, W.C.2.
1907.		BEGBIE, S. D.	Aster Engineering Co., Ltd., Wembley, Middlesex.
1914.		BELL, J.	81, The Grove, Wandsworth, S.W.18.
1909.		BELL, L.	16, Barnton Gardens, Davidsons Mains, Edinburgh.
1912.		BELL, P. A. G.	5, Deansgate, Manchester.
1914.		BELL, R.	13, Earl Street, Westminster, S.W.1.
1907.		BENNETT, F. S.	Lewgars, Kingsbury, Middlesex.
1913.		BENSON, CARL T. F. ..	Speedwell Ironworks, Coatbridge, N.B.
† 1910.		**Berriman, A. E.**	Chief Engineer, The Daimler Co., Ltd., Coventry.
1907.		BIRCHAM, F. R. S. ...	Wyndham Club, St. James' Square, S.W.1.
* 1908.	G.	BIRD, A. W.	69, Goldsmith Road, King's Heath, Birmingham.
1915.	A.M.		
1921.	M.		
1908.		BLAKE, F. C.	Kew Motor Works, Station Avenue, Kew Gardens, S.W.
1912.	A.M.	BLAMEY, Lt.-Col. E.	Bulford.
1920.	M.	H., D.S.O., R.A.S.C.	
1921.		BONALLACK, W.	St. George's Works, Cable Street, E.1.
1919.		BOOTE, E. M.	11, Lydgate Road, Coventry.
1920.		BOOTH, J. H.	Fletton Spring House, Peterborough.
1920.		BORTHWICK, T. A. ...	c/o Turner Hoare & Co., Ltd., Supari-bang Road, Parel, Bombay.
1912.	A.M.	BOSTOCK, F. J.	Bostock & Bramley, Netherton, Huddersfield.
1919.	M.		
1908.		BOWN, C. H.	Messrs. Merryweather, Ltd., Greenwich, S.E.
1919.		BOYD, J. H.	Schloss Hotel, Kapellan, nr. Coblentz, Germany.
† 1920.		BRAMLEY-MOORE, Capt. S.	61, Trinity Street, Huddersfield.
1914.	A.M.	BRANDER, Lt.-Col. M.	A.D.T. War Office, S.W.1.
1918.	M.	S.	
1916.		**Brearley, Hy.**	Brown Bayley's Ltd., Sheffield.
1916.		BRETHERTON, F. J. ...	West Cottage, Drury Lane, Lincoln.
1907.		BREWER, Capt. R. W. A.	55, Mt. Vernon Avenue, Buffalo, New York, U.S.A.
1917.		BRIGGS, R.N., Engr.-Commr. W.	24, Lytton Grove, Putney, S.W.15.
1920.		BRISTOW, Lt.-Col. W. A.	104, High Holborn, W.C.1.
1920.		BROOK, R. V. C.	Shirley, Birmingham.
1920.		BROWN, F. J.	Camden, Norton Way S., Letchworth.
† 1910.		BROWN, G. W. A. ...	St. George Carriers, Ltd., 7, Hertford Street, Mayfair, W.1.
1919.	A.M.	BROWN, W. FERRIER...	57, Barras Lane, Coventry.
1921.	M.		
† 1907.		**Browne, Lt.-Col. T. B., O.B.E.**	32, Onslow Square, S. Kensington, S.W.7.

Date of Election.			
1909.	A.M.	BROWNE, G. W.	The Garage, Mostyn Broadway, Llandudno.
1911.	M.		
1908.	A.M.	BUCKLEY, F.	The Nook, Melyd Avenue, Meliden Road, Prestatyn, N. Wales.
1918.	M.		
1918.		BUCKNEY, E. J. D. ...	2, Old Fallings Lane, Bushbury, Wolverhampton.
1910.		BUDGE, JOHN	Standard Motor Co., Ltd., Foleshill Road, Coventry.
1918.		BULLOCK, W. E.	Singer & Co., Coventry.
1921.		BUNN, G.	Newton Villa, Droitwich, Worcs.
1908.		**Burford, H. G.**	16, Regent Street, S.W.1.
1919.		BURGESS, F. T.	16, Ravensdale Avenue, N. Finchley, N.
1920.		BURGOINE, Capt. A. C.	25, Pembridge Gardens, W.2.
† 1911.		BURLS, G. A.	Margarets, Manuden, Essex.
1908.		BURNSIDE, F. M.	c/o Lawson and Jardine, 4, St. James' Buildings, William Street, Melbourne.
1910.	A.M.	BURNSIDE, G. M.	Royal Automobile Club, Pall Mall, S.W.1.
1920.	M.		
1910.		BURT, EDWIN J.	19, Grosvenor Road, Westminster, S.W.1.
1918.		BURTON, P. G.	Sussex Motors Ltd., Cromwell Road, Hove.
1912.	A.M.	BURTON, J.	84, Moulsham Street, Chelmsford.
1917.	M.		
1907.		BUTTEMER, R. W.	St. Mary's, Godalming.
l 1918.	A.M.	BUTTERFIELD, W. H. ..	c/o Butterfields Ltd., Stechford, Birmingham.
1920.	M.		
1910.		CADETT, J.	291, Upper Richmond Road, East Sheen, S.W.14.
1911.		CAILLAT, J.	Zenith Carburettor Co., 40—42, Newman Street, W.1.
1908.		CANNELL, W.	Gwynnes, Ltd., Church Wharf, Chiswick, W.4.
1917.		CARLAW, D.	11, Finnieston Street, Glasgow.
1919.		CARTY, S. W.	c/o McKenzie & Co., Ltd., Rangoon, Burma.
1917.		CASTLE, GEO.	12, Marlborough Place, Brighton.
1913.		CHARLES, C. R.	"Warren Down," Warren Road, Guildford.
l 1918.		CHATER-LEA, WM. ...	74/84, Banner Street, E.C.1.
1910.		CHEESEMAN, A. B. E.	3, Earl Street, Horseferry Road, S.W.1.
† 1916.		CHORLTON, A. E. L. ..	36, Victoria Street, S.W.1.
1916.		CHURCH, H. D.	Hare's Motors, Inc., 16, West Sixty-first Street, New York City, U.S.A.
1914.		CLARE, E.	Fernside, Seymour Road, Hampton Wick, Middlesex.
† 1910.		CLARK, Major A. G. ..	The Technical Review, 53, Victoria Street, Westminster, S.W.1.
1911.	A.M.	CLARK, DAVID	42, Audley Road, West Hendon, N.W.
1920.	M.		
1912.		CLARK, GEO.	The Cedars, Chesterfield.
1919.		CLARKE, J.	181, Bristol Road, Edgbaston, Birmingham.
† 1913.		**Clarkson, T.**	Woodlands, Galleywood, Chelmsford.
† 1911.		CLAYDEN, A. LUDLOW.	Brewster & Co., Long Island City, New York.

	Date of Election.			
	1913.	A.M.	CLAYTON, R. F.	Union Works, Huddersfield.
	1918.	M.		
	1912.	A.M.	CLEAVER, Lieut. C. F.	Slough Trading Estate, Slough, Bucks.
	1914.	M.		
	1910.	A.M.	CLENCH, E. C.	The Aster Engineering Co., Ltd.,
	1916.	M.		Wembley, Middlesex.
†‡*c*	1907.		CLERK, Sir DUGALD, K.B.E., F.R.S., F.C.S.	57, Lincoln's Inn Fields, W.C.2.
	1916.		CLERKE, Major A. B.	Hadfields, Ltd., Sheffield.
	1909.	A.M.	CLIFFORD, A. C.	Castle Rainey, Sutton Green, Guildford.
	1916.	M.		
	1913.		COATALEN, L.	c/o Sunbeam Motor Car Co., Ltd., Moorfield Works, Wolverhampton.
	1907.		COCHRANE, Lt.-Col. T. H., M.V.O.	Horse Guards, Whitehall, S.W.1.
	1918.		COMERY, WM.	Comery Gears, 228, Castle Boulevard, Nottingham.
	1921.		CONROY, T. M.	Harper Bean, Ltd., 50, Pall Mall, S.W.1.
	1912.	G.	CONSTANTINE, W. W.	c/o Crossley Bros., Ltd., Erwood Park
	1916.	A.M.		Works, Heaton Moor, Stockport.
	1919.	M.		
	1920.		COOLE, THOS.	"Birchfield," 18, Elson Road, Formby, Lancs.
	1911.	A.M.	COOMBES, G.	57, Clifford Road, East Finchley, N.2.
	1921.	M.		
	1910.		COOPER, J. E.	49, Oxford Road, Moseley, Birmingham.
	1910.	A.M.	COPSEY, W. A. B.	c/o "Shamrock," Grove Road, Rayleigh,
	1918.	M.		Essex.
	1915.		CORMACK, Prof. J. D.	Engineering Dept., The University, Glasgow.
	1915.	A.M.	COSTIGAN, J. C.	Sunnyside, Monument Green, Weybridge, Surrey.
	1918.	M.		
*	1908.	G.	COSWAY, Major L. H.	113, Geraldine Road, Wandsworth,
	1911.	A.M.	BASKERVILLE, O.B.E.	S.W.18.
	1920.	M.		
	1914.	A.M.	COURTNEY, T.	London General Omnibus Co., Ltd.,
	1915.	M.		Electric Railway House, Westminster, S.W.1.
	1907.		COVENTRY, F. C. A. ...	Office of Superintendent of Line, Great Western Railway, Paddington Station, W.2.
	1914.		COYNE, J. HEBER	Packard Motor Car Co., Detroit, Mich., U.S.A.
†‡	1907.		CRAIG, ALEX.	Maudslay Motor Co., Parkside, Coventry.
	1909.		CRAIG, Capt. A. E. S.	Cox & Co., 7, Rue Jeanne d'Arc, Rouen, France.
†	1907.		**Critchley, Capt. J. S.**	3, George Street, Hanover Square, W.1.
	1907.		CROMPTON, CLAUD	61, Vanbrugh Park, Blackheath, S.E.3.
†‡*c*	1907.		**Crompton, Colonel R. E.**, C.B., R.E.	Thriplands, Kensington Court, W.8.
	1909.		CROWDY, A. E.	The Old Bell House, Northfield, Birmingham.

Date of Election.			
1914.		DAVIDGE, A. W.	Rosebank, Steel Road, Northfield, Birmingham.
1910.		DAVIDSON, Lt.-Col. A. E.	11, The Pryors, Hampstead, N.W.3.
1908.		DAVIES, CHAS.	33, King Edward Avenue, Blackpool, N.S.
1914.	A.M.	DAWSON, A. J.	Witten Lodge, Stoke Green, Coventry.
1918.	M.		
1920.		DAWSON, W. J.	Hadfields Ltd., Hecla Works, Sheffield.
1916.		DAY, B. I.	Rolls Royce, Ltd., Nightingale Road, Derby.
1907.		DEW, J. W. H.	
†c 1916.		**Dickenson, J. H.**	Vickers, Ltd., River Don Works, Sheffield.
1919.		DICKSEE, C. B.	906, Wood Street, Wilkinsburg, Pa., U.S.A.
1920.		DONALD, R.	Macrae & Dick, Inverness.
1912.		DONALDSON, A.	Albion Motor Car Co., Ltd., Scotstoun, Glasgow.
1912.		DONKIN, H. J.	3, Victoria Street, S.W.1.
1913.		DONOHUE, Lt.-Col. W. E., C.B.E.	Union Club, Alexandria, Egypt.
1914.		DOUGILL, A. W.	Victoria Lodge, Victoria Road, Stechford, Birmingham.
1910.	A.M.	DOUGILL, E.	Old Woking Road, West Byfleet, Surrey.
1918.	M.		
1907.		DOVER, H. W.	Dover, Ltd., Northampton.
1919.		DOYLE, D.	33, Maitland Road, Sydenham, S.E.
1911.		DRAYSON, W. F.	Commercial and Marine Engine Co., Ltd., Oak Works, Church Road, Hanwell, W.
1908.		DRUMMOND, D. M. ...	Gordon Cottage, Dumfries.
1920.		DUCKETT, W.	Moss Gear Co., Ltd., Crown Works, Thomas Street, Aston, Birmingham.
1916.		DUNHAM, G. W.	Militor Motor Co., 111, Broadway, New York, U.S.A.
1919.		DYKES, F. J.	The Loke House, West Road, Cambridge.
1913.		EARP, A. CLIFFORD ...	c/o Cox & Co., Calcutta, India.
1909.	A.M.	EDMONDS, R. W.	Berkley Cottage, Bray, Berks.
1920.	M.		
1911.		EDWARDS, H. J.	Willys Overland Co., Elyria Plant, Elyria, Ohio, U.S.A.
1918.		ELLIS, WM.	Coventry Acme Co., Earlsdon, Coventry.
1919.	A.M.	ELVIDGE, HY.	Speedwell Ironworks, Coatbridge, Scotland.
1920.	M.		
1914.		ENDACOTT, N.	Endora, Winchester Road, Basingstoke.
1921.		ERADE, C. J.	213, South Croxted Road, S.E.21.
1915.	A.M.	ESSE, C. E.	28, Station Road, Barnes, S.W.13.
1920.	M.		
1908.		EVANS, A. F.	2, Castleton Mansions, Barnes, S.W.
1920.		EVANS, A. T.	Outspan, Down View Road, Worthing.
1920.		EVANS, Major C. B. ...	The Bank, Knighton, Radnorshire.
1919.		EVANS, G. E.	58, Alexander Road, Acocks Green, Birmingham.

Date of Election.			
1916.		FEARNLEY, A. R.	Corporation Tramways, Division Street, Sheffield.
1910.	A.M.	FEDDEN, A. H. R. ...	Bristol Aeroplane Co., Ltd., Filton House, Bristol.
1919.	M.		
1917.		FEENY, V. F.	60, Queen Victoria Street, E.C.4.
1916.	A.M.	FISHER, W. L.	20, Tudor Street, E.C.4.
1920.	M.		
‡ 1907.		FLATHER, DAVID	Messrs. W. T. Flather, Ltd., Standard Steel Works, Sheffield.
1907.		FLATHER, G. D.	Messrs. W. T. Flather, Ltd., Standard Steel Works, Sheffield.
†* 1916.		**Fowler, Sir Hy.,** K.B.E.	Midland Railway, Chief Mechanical Engineer's Office, Derby.
1921.		FOWLER, H. T.	The White Cottage, Worting Road, Basingstoke.
1913.		FULTON, N. O.	Albion Motor Car Co., Ltd., Scotstoun, Glasgow.
1920.		GALBRAITH, Capt. J. A.	29, Gibson's Road, Heaton Moor, nr. Stockport.
‡ 1907.		GARRARD, C. R.	107, High Street, Sydenham, S.E.26.
1907.		GIBBONS, N. B.	Shelbourne Hotel, Upper Bedford Place, Russell Square, W.C.1.
1910.		GIBBS, J. E.	Sycamore Cottage, Clifton, York.
1909.		GILL, J. H. W.	c/o Gill Propeller Co., Ltd., Blackfriars Street, King's Lynn.
1914.	A.M.	GILLETT, THOS.	The Hayloft, Weston Green, Esher, Surrey.
1917.	M.		
1917.		GILLETT, E. H. J. C.	British Ensign Motors, Ltd., Hawthorn Road, Willesden Green.
1917.		GLASER, H.	77, Corringham Road, Golders Green, N.W.4.
1911.		GRAY, J.	Government Factory, Colombo, Ceylon.
1915.	A.M.	GREEN, Engr.-Commr.	H.M.S. " Concord," c/o G.P.O., London.
1917.	M.	E. C., D.S.O.	
1920.		GREEN, F. M.	Armstrong-Whitworth Aircraft Co., Ltd., Coventry.
1920.		GREENLAND, Major H.	11, Watling Street, Radlett, Herts.
1918.		GREENWOOD, J. E. ...	Stubbs Road, Wolverhampton.
1912.		GREGORY, H. H.	Technical Department, R.A.C., Pall Mall, S.W.1.
1916.		GRENFELL, E.	Essener Hof, Fried. Krupp's Hotel, Essen-Ruhr, Germany.
1917.		GRICE, A. G.	c/o G. W. K., Ltd., Maidenhead.
1920.		GUISE, H. T.	25, Park Road, Redditch.
1909.		HACKING, C. W.	19, Oakwood Court, Kensington, W.
1918.		**Hadfield, Sir Robert,** Bart., D.Sc., D.Met., F.R.S.	22, Carlton House Terrace, S.W.1.
1907.		HALL, H. E.	Messrs. H. E. Hall & Co., 90, High Street, Tonbridge.
1916.	A.M.	HALL, H. SCOTT	663, Alexandra Park Road, Alexandra Park, N.22.
1920.	M.		
1914.		HALLIDAY, HIGLEY	12, Bolton Road, Chiswick, W.
1916.	A.M.	HALSALL, C. F.	80, North Side, Wandsworth Common, S.W.
1919.	M.		
1907.		HAMILTON, D. W.	58, Compton Street, London, E.C.1.
1917.		HANCOCK, A. J.	Vauxhall Motors, Ltd., Luton, Beds.

Date of Election.			
1911.		HATFIELD, E. W.	147, Norfolk Street, Sheffield.
† 1916.		HATFIELD, Dr. W. H.	Brown-Firth Research Laboratory, Sheffield.
1911.		HAWORTH, J. W.	232, Deansgate, Manchester.
* 1908.	G.	HAYTER, Major G. W.,	57, Chancery Lane, W.C.2.
1911.	A.M.	O.B.E.	
1920.	M.		
1909.		HEATH, A. R. N.	J. & E. Hall, Ltd., Dartford Ironworks,
1920.		(Re-elected.)	Kent.
†*c 1907.		HELE-SHAW, H. S., D.Sc., LL.D., F.R.S.	64, Victoria Street, London, S.W.1.
1907.		HEMMINGS, H. F.	c/o Willans & Robinson, Victoria Works, Rugby.
†l 1914.		**Henderson, J. F.**	Albion Motor Car Co., Ltd., Scotstoun, Glasgow.
1912.		HENSON, G. H.	Daimler Motor Co., Ltd., Coventry.
1920.		HESSE, Major J. H. B.	J. I. Thornycroft & Co., Ltd., Pulford Street, Pimlico, S.W.1.
1920.		HEY, GEO.	50, Orchard Road, St. Margaret's-on-Thames.
1914.		HICKSON, S. C. S.	3, Piccadilly, W.1.
1909.	G.	HILLHOUSE, J. P.	1, Vicarage Road, King's Heath, Birmingham.
1911.	A.M.		
1918.	M.		
1917.		HINGSTON, W. H.	50, Pall Mall, S.W.1.
1921.		HISCOCKS, S. W.	Grove Park House, Kingsbury, N.W.9.
1913.	A.M.	HODGSON, Major DUN-	Hodgson's Motor Garage, 121, North-
1918.	M.	CAN.	umberland Street, Newcastle-on-Tyne.
1917.		HODGSON, HERBERT ...	Messrs. John Fowler & Co., Steam Plough Works, Leeds.
1916.		HODSDON, G. C.	J. & E. Hall, Ltd., Dartford, Kent.
1911.		HOLDEN, Brigadier-General Sir CAPEL, K.C.B., F.R.S.	Gifford House, St. John's Park, Blackheath, S.E.3.
1909.	A.M.	HOLLOWAY, S. C.	Minerva Motors, Ltd., Chenies Street,
1919.	M.		W.C.1.
1916.	A.M.	HOLMES, H.	32, St. Albans Road, Moseley, Birmingham.
1920.	M.		
1919.		HOLROYD, J.	James Whiteley, Ltd., Halifax.
1918.		HOLROYD, V. A.	40, Warwick Place, Leamington.
1907.		HOOPER, P. N.	Ager Point, Salcombe, S. Devon.
1909.		HOOYDONK, J. VAN	Phœnix Motors, Ltd., Letchworth,
1918.		(Re-elected.)	Herts.
1914.		HOPKINS, W. H.	c/o Vulcan Motor and Engineering Co., Ltd., Crossens, Nr. Southport.
1916.		HOPPS, BERNARD	The Lodge Sparking Plug Co., Ltd., Rugby.
† 1907.	A.M.	**Hounsfield, L. H.** ...	Trojan, Leyland Motors, Ltd., Kingston-on-Thames.
1909.	M.		
1912.	A.M.	HOWELL, J. C.	Lindsay Swan Hunter, Ltd., Wear Engine Works, Sunderland.
1920.	M.		
*g 1908.	G.	HUBBARD, Capt. R. K.	Officers' Club, Aldershot.
1912.	A.M.		
1916.	M.		
1908.	A.M.	HUDLASS, F. W.	26, Westmoreland Road, Barnes, S.W.
1909.	M.		
1914.		HUMPHREY, BARNARD.	Great Western Railway, Motor Car Dept., Slough, Bucks.

Date of Election.			
1916.		HUNT, J. H.	General Motors Research Corporation, Dayton, Ohio, U.S.A.
1916.		HUNT, O. E.	102, Rhode Island Avenue, Detroit, U.S.A.
1908.		HUTCHINSON, F.W. H.	Grove Lawns, St. Alban's.
1915.		HUTCHINSON, H. W. ..	Woolston, Southampton.
l 1911.		HUTCHINSON, Col. T. M.	Chief Inspector, M.T., R.A.S.C., Horse Guards, S.W.1.
1920.		HYDE, J. H.	8, Claremont Road, Teddington, Middlesex.
1907.		**Iden, W. J.**	106, Sutton Court, Chiswick, W.4.
1907.		INSTONE, A. C.	Bank of Australasia, Sydney, N.S.W.
1910.		IONIDES, A. G.	278, East Park Road, Leicester.
1918.		IRELAND, H. W. F. ..	Lucas Electrical Co., Ltd., Well Street, Birmingham.
1920.		IRVING, Capt. J. S. ..	129, Tettenhall Road, Wolverhampton.
1907.		JAMES, J. M.	Waveney Works, Freemantle Road, Lowestoft.
1907.		JAMES, W. G.	14, Mortimer Street, Regent Street, London, W.1.
1908.		JANNAY, R.	21, Claremont Avenue, New York, U.S.A.
1907.	A.M.	JOHNSON, A. E.	18, Woodstock Road, Redland, Bristol.
1918.	M.		
1917.	A.M.	JONES, H.	7, Belvedere Road, Coventry.
1921.	M.		
1920.		JORDAN, Major C. W.	Alldays & Onions, Ltd., Great Western Works, Small Heath Station, Birmingham.
1907.		JOY, BERTRAM C.	4, Holmedale Mansions, Holmdale Road, N.W.6.
1910.		JOY, BASIL H.	Quayside, Burnham-on-Crouch, Essex.
1918.		KAY, C. B.	"The Hollins," Goldthorn Road, Wolverhampton.
1914.		KEACHIE, D.	92, Camperdown Road, Scotstoun, Glasgow.
1918.		KEILLER, C. M.	G. W. K., Ltd., Maidenhead.
1915.	A.M.	KELLAR, W. S.	11765, Ilene Avenue, N.W.S. Detroit,
1920.	M.		Mich., U.S.A.
* 1908.	G.	KENNEDY, D.	215, Selhurst Road, South Norwood, S.E.
1909.	A.M.		
1920.	M.		
† 1912.		KENNEDY, Lt.-Col. D. S., D.S.O.	Bar Close Barn, Henley Road, Caversham, Oxon.
1912.		KENNEDY, S. S.	"Pembroke Lodge," Harlesden Road, Willesden, N.W.
1908.	A.M.	KENNINGTON, W. O. ..	c/o W. H. Johnson, 14 Panton St.,
1920.	M.		S.W.1.
1911.		KERSEY, A. T. J.	Royal Technical College, Peel Park, Salford.
1912.		KIDNER, P. C.	Vauxhall Motors, Ltd., Luton.
1918.		KING, H. G.	Works Engineer, Vickers, Ltd., Erith.

Date of Election.			
1910.		KINGSTONE, H. G. ...	39, Wilton Road, S.W.1.
1919.		KIPLING, H. S.	Wolseley Motors, Ltd., Adderley Park, Birmingham.
1920.		KIRKALDY, Major A. H.	Birch Hanger, Godalming.
1907.		KNOWLES, G. J. F. ...	4, Park Row, Knightsbridge, S.W.1.
1910.		LAMPLOUGH, F.	The Old Tan House, Bray-on-Thames, Berks.
1907.		LANCASTER, E. H. ...	The Service Motor Co., 245, Oxford Street, W.1.
†c 1907.		**Lanchester, F. W.,** D.Sc.	41, Bedford Square, W.C.1.
1907.		**Lanchester, G. H.** ...	Annesley, Dyott Road, Moseley, Birmingham.
1917.		LANDAU, D.	1779, Broadway, New York, U.S.A.
1920.		LANDSTAD, H.	113, Divinity Road, Oxford.
1907.		**Lawrence, Max R.** ..	252A, Gloucester Terrace, Hyde Park, W.2.
1914.		LAYCOCK, A. M.	Sheldon Axle Co., Wilkes-Barre, Pa., U.S.A.
1919.		LEE, A. C.	30, Doris Road, Sparkhill, Birmingham.
† 1907.		**Leechman, G. Douglas.**	Lamb Buildings, Temple, London, E.C.4.
1914.		LEEDING, E. J.	Wolseley Motors, Ltd., Adderley Park, Birmingham.
†cl 1907.		**Legros, L. A.,** O.B.E.	25, Cumberland Park, Acton, W.
1910.		LEITNER, HENRY	Regent House, Kingsway, W.C.2.
1917.		LEWIN, Major H. W.	Bath Club, Dover Street, W.1.
1912.		LISLE, R.	Star Engineering Co., Ltd., Wolverhampton.
1916.		LITTLER, L.	25, Winchelsea Avenue, Newark-on-Trent.
1920.		LOW, Dr. A. M.	The Low Engineering Co., Ltd., 92A, High Street, Kensington, W.8.
1920.		MACDONALD, G.	Halley's Industrial Motors, Ltd., Yoker, Glasgow.
1907.		MACFARLANE, W. M. ..	25, Hampstead Hill Gardens, N.W.
1920.		MACLEAN, J. B.	107, Bishopsgate, E.C.2.
1918.		MALLINS, C. W.	Hollyburn House, 122, Roe Lane, Southport.
1910.		MANVILLE, E., M.P. ..	St. Stephen's House, Victoria Embankment, London, S.W.1.
1910.		MARCHANT, H. G.	Acre Mills, Lindley, Huddersfield.
† 1907.		MARKHAM, Lt.-Col. R. G. L.	Cromwell House, High Holborn, W.C.1.
‡ 1907.		MARPLES, S. A.	104, High Holborn, W.C.1.
1907.		MARRIOTT, E. J.	Vauxhall Motors, Ltd., Luton.
1907.		MARSDEN, ALFRED ...	3, Regent House, Wirtemburg Street, Clapham, S.W.4.
1909.		MARSHALL, C. E.	322, Alcester Road, Moseley, Birmingham.
1911.		MARSHALL, C. F. B. ..	2, St. Mary's Place, London Road, Newbury, Berks.
1910.	A.M.	MARSHALL, Major JNO.	73, Cathedral Road, Cardiff.
1920.	M.		
1907.		MARSTON, CHAS.	The Villiers Co., Blakenhall, Wolverhampton.

Date of Election.			
† 1907.		MARTIN, PERCY	Daimler Motor Co., Ltd., Coventry.
†*ℓ 1907.		**Martineau, F. L.** ...	3—7, Old Queen Street, S.W.1.
1916.		MASURY, A. F.	International Motor Co., West End Avenue and 64th Street, New York, U.S.A.
1919.		MAUDSLAY, R. W.	1, Vicarage Road, Lillington, Leamington.
1918.		MAWER, S.	Arscott & Mawer, Danesgate, Lincoln.
1920.		MCCANCE, Dr. A.	63, Lauderdale Gardens, Hyndland, Glasgow.
1914.	A.M.	MCCAW, G. E.	Halley's Industrial Motors, Ltd., Yoker, Glasgow.
1918.	M.		
1913.		MCCORMACK, A., C.B.E.	Wolseley Motors, Ltd., Adderley Park, Birmingham.
1911.		MCMURTRY, A. L.	State Capitol, Hartford, Conn., U.S.A.
1907.	A.M.	MIDDLEMISS, E. T. ...	23, Fountain Street, Manchester.
1909.	M.		
* 1913.		MILLIGAN, J. L.	13, Lydgate Road, Coventry.
1911.		MILLS, Lt.-Col. G. P., D.S.O.	Aster Engineering Co., Ltd., Wembley, Middlesex.
1918.		MILLS, J. W.	Halley's Industrial Motors, Ltd., Yoker, Glasgow.
1920.		MITCHELL, GARDINER..	83, Blenheim Place, Aberdeen.
1921.		MITCHELL, R. J.	"Westfield," Upton Road, Slough.
1916.		MONTAGUE, Capt. GRAHAM.	c/o Mechanical Dept., Public Works Ministry, Cairo.
† 1907.		MORCOM, R. K.	Messrs. Belliss & Morcom, Ltd., Birmingham.
1907.		MORGAN, B. H.	7, Prince's Street, S.W.1.
† 1908.		MORGAN, Prof. W. ...	26, Stoke Lane, Westbury, Bristol.
1920.		MORISON, W. J.	Savernake Lodge, Moulsham, Chelmsford.
1907.		MORRIS, G. L.	27, Lloyd Square, London, W.C.1.
1913.		MORTON, T. M. GRAY.	2, Hamilton Drive, Bothwell, N.B.
1911.		MOSSES, A.	21, Mercer Street, Long Acre, W.C.2.
1919.		MURRAY, GEO.	St. Gabriels Cottage, Mareschal Road, Guildford.
1911.		MURRAY, Dr. T. BLACKWOOD.	Conford Park, Liphook, Hants.
1919.		MYERS, C. T.	Eastern Office, Box 25, Avenel, N.J., U.S.A.
† 1907.		**Napier, Lt.-Col. John S.**	Aldersyde, Kenilworth, Warwickshire.
1907.		NAPIER, MONTAGUE S.	Villa des Cistes, Chemin du Roi Albert, Cannes, France.
1913.		NASMITH, JOHN	Oakmere, Royal Road, Sutton Coldfield.
ℓ 1909.	A.M.	NESBITT, Major E. A.	S. & T. Corps, D.A.D.T. (M.T.), G.H.Q., E.E.F., Cairo.
1911.	M.		
1909.		NEVILL, Lt.-Col. S. S., O.B.E.	79A, Parkhurst Road, Holloway, N.7.
1908.		NEW, V. G.	11, Victoria Street, S.W.1.
1916.		NEWSOM, H. H.	Standard Parts Co., Standard Welding Division, Cleveland, Ohio, U.S.A.
1921.		NEWTON, F.	4, Amherst Avenue, Ealing, W.5.
1907.		NIBLETT, A. H.	Springfield House, Keresley, near Coventry.

Date of Election.			
1911.		NIBLETT, Lt.-Col. HERBERT, D.S.O.	Gorse Cottage, Hook, near Basingstoke.
1917.	A.M.	NOBLE, A.	82, Kings Avenue, New Malden, Surrey.
1919.	M.		
1914.	A.M.	NORTH, O. D.	76, Elm Park Mansions, Park Walk, Chelsea, S.W.
1921.	M.		
1918.		NORTON, J. L.	Phillip Street, Aston, Birmingham.
1919.		NORWOOD, C. R.	6, Marlborough Road, Richmond, Surrey.
†‡ 1907.		**O'Gorman, Lieut.-Col. Mervyn, C.B.**	Athenæum Club, Pall Mall, S.W.1.
1908.		OKILL, JOHN	8, College Drive, Old Chester Road, Rock Ferry, Cheshire.
1911.	A.M.	O'MAHONY, Major C. S.	R.A.S.C. Training College, Aldershot.
1920.	M.		
† 1908.		OPPERMAN, CARL	98, Tollington Park, N.4.
† 1913.		**Ormandy, Dr. W. R.**	Oakley House, Bloomsbury, W.C.1.
1914.	A.M.	OSLER, J. B.	49, Ridgmount Gardens, W.C.1.
1921.	M.		
1910.		OWEN, Major A. D. ..	No. 4 Hut, Horse Guards Parade, S.W.1.
1915.	A.M.	OWEN, R. S.	"Lyndhurst," Davenport Park, Stockport.
1918.	M.		
1918.	A.M.	PALMER, A. E.	Experimental Dept., Wolseley Motors, Ltd., Adderley Park, Birmingham.
1920.	M.		
1917.		PARNACOTT, A. E.	Penge Lane, Penge, S.E.20.
1920.		PARTRIDGE, Major E.	H.Q., 21st Vehicle Reception Park, Champs de Manœuvres, Rouen.
1915.	A.M.	PATE, Jr., GEO.	c/o D. Napier & Son, Ltd., Acton Vale, W.3.
1917.	M.		
1913.	A.M.	PEARE, W. F.	Dunlop Rubber Co., Ltd., Tyburn, nr. Birmingham.
1919.	M.		
1920.		PEARSALL, R. H.	Avon Cottage, The Crescent, Solihull, Birmingham.
1915.		PENTONY, R.	"Medhurst," St. George's Road, Twickenham.
1921.		PERKINS, C. B.	Electric Steel Castings Co., Ltd., 56, Cambridge Street, Sheffield.
1911.		PERROT, HENRI	10, Rue Philibert Delorme, Paris, XVII.
1912.		PETARD, RENE M.	4 bis Rue Gustave Zédé, Paris.
1909.	G.	PHILLIPS, W. E.	Brookfield House, Burnage Lane, Levenshulme, Manchester.
1912.	A.M.		
1920.	M.		
†c 1917.		PHILPOT, Prof. H. P...	39, Birchington Road, Crouch End, N.8.
1920.		PICKARD, Major J. A. A., D.S.O.	9, Carlisle Mansions, Westminster, S.W.1.
1918.		PITT, A. G.	"Barfield," Fellows Road, Farnborough, Hants.
1919.		PITT, H. W.	37, Stanhope Gardens, Highgate, N.
1916.		PITT, J. D.	c/o Edgar Allen & Co., Ltd., Sheffield.
1917.		PITT, P. S.	New Court, Temple, E.C.4.
1911.		PLAISTER, W. E.	Pembury House, Tottenham, N.
1920.		POINTON, J. E.	"Brackley," Park Crescent, Peterborough.
1908.		POLLARD, G.	Royal Automobile Club, Pall Mall, S.W.1.

Date of Election.			
1920.		PONSFORD, G. L.	35, Esmond Road, Bedford Park, W.4.
†*c 1910.		POMEROY, L. H.	2800, Harvard Avenue, Cleveland, Ohio, U.S.A.
1918.		POOLE, S. C.	"Beechurst," Radford, Coventry.
1911.	G.	POPE, Capt. C. R.	35, Lynmouth Road, E. Finchley, N.2.
1919.	M.	EGERTON.	
1908.		POPPE, P. A.	Messrs. White & Poppe, Lockhurst Lane, Coventry.
1915.		POUNTNEY, F.	4, Mount Road, Penn, Wolverhampton.
1907.	A.M.	PRANCE, Major H.	The Garage Bungalow, Caroline Place, Queen's Road, W.
1918.	M.	WAYMOUTH.	
1908.		PRESTWICH, J. A.	Northumberland Park, Tottenham, N.17.
1919.		PRIESTLEY, P.	65, Allerton Road, Liverpool.
1910.	A.M.	PRITCHARD, W. O. ...	The Autocar Co., Ltd., Tunbridge Wells.
1913.	M.		
1908.		PUGH, J. VERNON	Guiting House, Allesley, near Coventry.
l 1907.		**Pullinger, T. C.,** C.B.E.	Arrol Johnston Ltd., Dumfries, N.B.
1909.		RAINER, E. A.	14, Fenchurch Street, E.C.3.
1907.		RAINFORTH, W. F.	1, Acacia Road, Acton, W.
1911.		RALLINGS, E. H.	The Osiers, Point Pleasant, Wandsworth, S.W. 15.
1917.		RALLS, G. E. D.	312 & 314, Bradford Street, Birmingham.
1908.	A.M.	RAWLINGS, H. S.	14, Spencer Road, Cottenham Park, Wimbledon, S.W. 19.
1913.	M.		
1907.		REECE, J. GRAHAM ...	12, Croxteth Road, Liverpool.
1913.	A.M.	REEVE, E.	Wolseley Motors, Ltd., Adderley Park, Birmingham.
1914.	M.		
† 1908.		**Remington, A. A.,** O.B.E.	New Eldon Chambers, Cherry Street, Birmingham.
† 1917.		**Ricardo, H. R.**	Penstone, Lancing, Sussex.
1909.	A.M.	RICHARDS, G. T.	Wm. Beardmore & Co., Naval Construction Works, Dalmuir, Dumbartonshire.
1916.	M.		
1918.		RICHARDSON, J.	Wm. Beardmore & Co., Naval Construction Works, Dalmuir, Dumbartonshire.
1907.		RIDLEY, Capt. C.	27, Middleborough Road, Coventry.
1916.		RIKER, A. L.	Fairfield, Conn., U.S.A.
1907.		RILEY, VICTOR	Riley Cycle Works, Coventry.
1907.		RIVETT, A.	236, High Street, Leytonstone, N.E.
1916.		ROBINSON, E.	Glebelands, N. Farnborough, Hants.
1912.		ROBINSON, M. SCOTT..	31, Lombard Street, E.C.3.
1907.		ROSE, Lt.-Col. E. A., C.B.E.	16, Pembridge Place, W.
1917.		ROSENHAIN, Dr. W. ..	National Physical Laboratory, Teddington.
1911.	G.	ROSS, Capt. A. A.	Aeronautical Inspection Dept., 3—6, Clement's Inn, W.C.2.
1915.	A.M.		
1917.	M.		
1918.		ROSSITER, ALFRED	Milton House, Milton Road, Luton.
1920.		ROWELL, H. S.	58, Airedale Avenue, Chiswick, W.4.
1908.		**Rowledge, A. J.**	c/o Rolls Royce, Ltd., Nightingale Road, Derby.
1913.		ROWSE, A. A.	c/o Morris Motors, Ltd., Cowley, Oxon.
1910.		ROYCE, F. H.	(c/o J. de Looze), The Laurels, Empress Road, Derby.
1911.		RUSH, Lt. C. H. E. ..	40, Castleton Mansions, Barnes, S.W.
1920.		RUSHTON, G.	78, Palewell Park, Sheen, S.W.14.

Date of Election.			
1919.		SALMON, P.	Ingleside, Lynchford Road, S. Farnborough, Hants.
1918.		SCOTT, A. A.	Springfield Works, Springfield Place, Bradford, Yorks.
1910.		SEARLE, Col. F.	"Bartika," Lower Teddington Road, Hampton Wick.
1908.		SELZ, R.	44, Oval Road, Erdington, Birmingham.
1908.		SHARMAN, P. A.	Fishponds Road, Hitchin.
†‡ 1907.		SHARP, Prof. A.	231—2, Strand, London, W.C.2.
1910.		**Shave, G. J.**	Engineer, London General Omnibus Co., Ltd., Electric Railway House, Broadway, Westminster, S.W.1.
1908.		SHAW, B. J. A.	Wm. Beardmore & Co., Ltd., Underwood Works, Paisley, N.B.
†*l 1907.	A.M.	**Shilson, Major B.**	Armstrong-Siddeley Motors Ltd.,
1910.	M.	**W.**, O.B.E.	Coventry.
1908.	G.	SHORTER, L. J.	Humber, Ltd., Coventry.
1909.	A.M.		
1912.	M.		
1907.		**Simms, Frederick R.**	Percy Buildings, Gresse Street, Rathbone Place, W.1.
1911.	A.M.	SIMPSON, A. T.	71, Prospect Street, Hull.
1920.	M.		
1912.	A.M.	SIMPSON, E.	69, Talgarth Road, West Kensington, W.14.
1914.	M.		
1907.		SMALLMAN, H. S.	Globe Tube and Engineering Co., Wednesbury.
† 1907.		**Smith, Lt.-Col. David J.**, O.B.E.	40, Woodberry Grove, Finsbury Park, N.4.
1919.		SMITH, F. W.	Enfield Cycle Co., Ltd., Redditch.
1910.		SMITH, P. FROST	Tillings-Stevens, Ltd., Victoria Works, Maidstone.
1914.	A.M.	SMITH, P. W.	Claud Hamilton, Ltd., 254, Union Street,
1917.	M.		Aberdeen.
1919.		SMITH, Capt. P. W...	22, St. Albans Avenue, Bedford Park, Chiswick, W.4.
1907.		SMITH, R. R.	4, Great Marlborough Street, W.1.
1913.		SMITH, R. W.	Stoneleigh, Redditch.
1914.	A.M.	SMITH, Major W. H...	Imber Court Engineering Works, East
1920.	M.		Molesey, Surrey.
1918.		SMITH, W. T.	The Crest, Kenley, Surrey.
† 1918.		SMITH-CLARKE, Capt. G. T.	"Glenroy," Waverley Road, Kenilworth.
1919.	M.	SNOWDEN-SMITH, Lt.-Col. R. T.	c/o Sir C. R. McGrigor & Co., Panton Street, Haymarket, S.W.
1920.		SOMMERS, T. B.	19, Leslie Street, Pollokshields, Glasgow.
1908.		SPOONER, Prof. H. J...	The Polytechnic, 309, Regent Street, W.1.
1907.		STARLEY, J. K.	The Manor House, Ryton-on-Dunsmore, Coventry.
1917.		STEAD, Dr. J. E.	11, Queens Terrace, Middlesbrough.
1912.	G.	STEINHEIL, G.	1, Lesney Park Road, Erith, Kent.
1917.	A.M.		
1920.	M.		
1910.		STENT, C. E.	Admiralty Office, Rolls Royce, Ltd., Nightingale Road, Derby.
1920.		STEVENS, A. J.	A. J. Stevens & Co. (1914), Ltd., Graiseley House, Penn Road, Wolverhampton.

Date of Election.			
1916.		STEVENS, H. C. M. ...	Sunbeam Motor Car Co., Ltd., c/o Maison Darracq, 33, Quai du Generale Gallieni, Suresnes (Seine), France.
1912.		STEVENSON, A. Creery.	"Streeve," Church Circle, Farnborough, Hants.
1910.	A.M.	STEWART, P. A. N. ...	Bombay Motor Car Co., Ltd., Post Box 135, Bombay.
1913.	M.		
1911.		STIRLING, J.	Rock Thorpe & Co., Grosvenor Road, Tunbridge Wells.
1918.		STRAKER, S.	Straker Squire, Ltd., Angel Road, Edmonton, N.
1909.		STRICKLAND, F.	8, St. Albans Road, W.8.
1916.		STRICKLAND, W. R. ...	Peerless Motor Car Co., Cleveland, Ohio, U.S.A.
† 1914.	A.M.	STUBBS, A.	"Sinnocke," Lichfield Road, Four Oaks, Sutton Coldfield, Birmingham.
1916.	M.		
1908.		SULLY, R. J.	Herbert's Engineering Works, Caversham, Reading.
1919.		SUMNER, W. H.	8, Lowther Road, Barnes, S.W. 13.
1916.		SWEENEY, J. G.	25, Broad Road, Acocks Green, Birmingham.
1912.		SWINTON, A. A. Campbell, F. R. S.	66, Victoria Street, S.W.1.
1907.		TALBOT, E.	James Talbot & Davidson, Ltd., Horn Hill, Lowestoft.
†* 1908.	G.	TAYLOR, G. Campbell.	24, Church Road, Barnes.
1913.	A.M.		
1921.	M.		
1913.		TAYLOR, S. L.	Town Quay, Falmouth.
1910.		TENNANT, W. J.	111, Hatton Garden, E.C.1.
1920.		THOMAS, E.	57, Leagrave Road, Luton, Beds.
1916.		**Thomas, H. Kerr** ...	Associated Equipment Co., Ltd., Walthamstow, E. 17.
1912.		THOMAS, J. G. P.	Leyland Motors, Ltd., Leyland, Lancs.
1908.		THOMAS, W. P.	5, Woodville Road, Ealing, W. 5.
1912.		THOMPSON, E. F.	32, Princess Road, West Kilburn, N.W.6.
1912.		THOMSON, K. J.	14, Leonard Place, High Street, Kensington, W.8.
1911.		**Thomson, W. J.**	Scottish Motor Traction Co., Ltd., East Fountainbridge, Edinburgh.
1920.		THORNBURN, G.	Crossley Bros., Ltd., Erwood Park Works, Heaton Moor, Stockport.
1916.		THURSTON, Dr. A. P.	29, Southampton Buildings, Chancery Lane, W.C.2.
1919.		TILLING, R. M.	Thos. Tilling, Ltd., Bull Yard, High Street, Peckham, S.E.15.
† 1918.		TILSTON, E.	c/o Tilston Engines, Ltd., 59, New Oxford Street, W.C.1.
1920.		TOLLEY, Capt. R. G....	c/o C.I.M.T., Horse Guards Parade, S.W.1.
1911.		TOOKEY, W. A.	39, Victoria Street, Westminster, S.W.1.
1916.		TREDGOLD, C. F. H....	Hughes-Johnson Stampings, Ltd., Langley Green, near Birmingham.
1918.		TRICKETT, J. H.	Humber, Ltd., Coventry.
1910.		TULLOCH, Capt. T. G.	Bank Buildings, St. James' Street, S.W.1.
1907.		TURRELL, C. McR. ...	Thanet House, Temple Bar, W.C.2.

Date of Election.			
1916.		UTZ, J. G.	Detroit Athletic Club, Detroit, Mich., U.S.A.
1916.		VINCENT, J. G.	Packard Motor Car Co., Detroit, Mich., U.S.A.
1919.		WADE, F. R.	1, Dr. Johnson's Buildings, Inner Temple, E.C.
1912.	A.M.	WALFORD, J. B.	Ortona Motor Co., Ltd., Hills Road, Cambridge.
1920.	M.		
1909.	A.M.	WALTHALL, Capt. H.	c/o Hayter, Ltd., 82, Victoria St., S.W.
1920.	M.	D.	
1915.		WANS, O.	Washingboro, Lincoln.
1914.		WARDELL, T.	Glenmore, Watson Road, Worksop, Notts.
1919.		WARE, E. B.	Trevena, Trulock Road, Northumberland Park, Tottenham, N.
1920.		WARWICK, G.	I.R.G.P. & T.W. Co., Ltd., Horninglow Road, Burton-on-Trent, Staffs.
1916.		WATERHOUSE, L. M....	113—117, Charing Cross Road, W.C.2.
1920.		WATKINS, M.	149, Somerville Road, Small Heath, Birmingham.
† 1907.		**Watson, G. W.**	50, Pall Mall, S.W.1.
1910.		Watson, O. W. J.....	Greylands, Bolton Road, Chiswick, W.4.
1912.	A.M.	WATT, J.	Designing Office, Albion Motor Car Co.,
1919.	M.		Ltd., Scotstoun, Glasgow.
1920.		WEBB, W. L.	Crossley Bros., Ltd., Openshaw, Manchester.
1911.	A.M.	WEBSTER, T. L.	"Ernevale," 184, Nithsdale Road, Pol-
1920.	M.		lokshields, Glasgow.
1919.		WELLS, W. H.	Hendee Manfg. Co., 366—368, Euston Road, N.W.1.
‡/ 1907.		**Wheeler, Major Chas.,** O.B.E.	Casula, Bushey Grove Road, Bushey, Herts.
1909.		WHITE, D'O. McC. ...	c/o Lafayette Motors Coy., Mars Hill, Indianapolis, Ind., U.S.A.
1914.		WHITE, H. T.	J. Tylor & Sons, Ltd., Belle Isle, York Road, N.
1918.		WHYTE, S.	Earlswood Road, Redhill, Surrey.
1913.		WILD, Mark	29, Beauchamp Avenue, Leamington.
1912.		WILDER, Lt.-Col. H. C.	c/o "J" Branch, Cox & Co., 16, Charing Cross, S.W.1.
/ 1908.		WILDING, J. A.	3, Wilson Street, Drury Lane, W.C.2.
1921.		WILKINSON, G. S. ...	15A, Willow Road, Hampstead, N.W.3.
† 1913.	A.M.	WILLIAMSON, W. D....	43, Park View, Wigan.
1914.	M.		
1919.		WILLSON, H. C.	50, Calthorpe Road, Edgbaston, Birmingham.
1912.		WINDSOR, A. W.	Leyland Motors, Ltd., Leyland, Lancs.
1907.		WINN, Rowland	Woodhouse Lane, Leeds.
1908.	A.M.	**Wood, E. B.**	Brockley Hill House, Stanmore.
1919.	M.		
1907.		WOOLLAN, S. W.	176, Caversham Road, Reading.
1915.		**Woollard, F. G.**	E. G. Wrigley & Co., Ltd., Foundry Lane, Soho, Birmingham.
1907.		**Woollen, T. H.**	56, St. Mark's Road, North Kensington, W. 10.
1913.		WRIGHT, H. T.	c/o Reiss & Co., 52, Ta Fang Chia, Hutung, Peking.

Date of Election.			
†c 1915.	A.	YOUNG, A. P., O.B.E.	British Thomson Houston Co., Ltd., Lower Ford Street, Coventry.
1919.	M.		
1911.		YOUNG, G. M.	Albion Motor Car Co., Ltd., 21, Upper Rathbone Place, Oxford Street, W.1.
1921.		YOUNG, W.	Karrier Motors, Ltd., Karrier Works, Huddersfield.
1916.		YOUNGER, John	13515 Lake Shore Boulevard, Cleveland, Ohio, U.S.A.

2. *Associate Members.*

1918.		ABELL, C. F.	c/o Lloyds Bank, Ltd., Leamington Spa.
1913.		ABBOTT, J. D.	79, Lombard Street, E.C.3.
1916.		ABRAHAMS, W. H. ...	4, Ruthin Road, Blackheath, S.E.
1911.	G.	ADAMS, C. H.	Cutbush House, Shinfield, Reading.
1920.	A.M.		
1912.		ADAMS, James	Reid & Adams, Belleville Works, Stranraer, N.B.
1920.		ADDIS, J.	Bamford & Martin, Ltd., 53, Abingdon Road, Kensington, W.8.
1920.		AINSWORTH, A. G. ...	21, Granville Road, Hove, Sussex.
1918.		AKED, Wm.	"Kassauli," Devonshire Road, St. Anne's-on-Sea.
1918.		ALLDAY, E. H.	100, Lexham Gardens, Kensington, W.8.
1919.		ALSTEAD, T.	66, Park Street South, Blakenhall, Wolverhampton.
1919.		ANDERSON, D. M. ...	1115, Bristol Road South, Northfield, near Birmingham.
1913.		ANDREW, J. H.	H. Andrew & Co., The Garage, Athenæum Place, Plymouth.
1920.		APPLEGATE, H. F.	"Holme," 4, Gledholt Road, Huddersfield.
1914.		APPLEYARD, E.	c/o E. Haines Jones, Farnborough, Kent.
1919.		APPLEYARD, J. E. ...	Ashlea, Bramley, Leeds.
1921.		ARMSTRONG, F. G. G.	St. Giles Croft, Beverley, Yorks.
1913.		ARMSTRONG, P. A. E.	c/o Ludlum Steel Co., Watervliet, New York, U.S.A.
1919.		ARNOLD, T. J.	"Brooklands," Fairlawn Park, Chiswick, W.4.
1911.		ARNOLD, W.	Upper Brook Street, Manchester.
1915.		ARNOLD, W. R. H. ...	12, Thornton Road, East Sheen, S.W.14.
1920.		ARROW, P.	12A, Clarendon Road, Putney, S.W.15.
1914.		ARTER, Erland	33, Pembridge Villas, Bayswater, W.2.
1921.		ASHBY, F. C.	42, Clarendon Road, West Croydon, Surrey.
1919.		ASHER, Capt. R. S....	18, Louisville Avenue, Aberdeen.
1919.		ATHERTON, J. H.	Addison Motor Co., 18—20, Addison Street, Liverpool.
1909.		ATKINSON, R.	57, Kimberley Drive, Great Crosby, Liverpool.
1918.		AUSTIN, A. C.	Coventry Simplex Engines, Ltd., Payne's Lane Works, Coventry.
1916.		BAKER, J. A.	2, Ashton Grove, Ashton-on-Mersey, Cheshire.

Date of Election.			
1916.		BALES, F. G.	25, Avenue Road, Hammersmith, W.
1910.		BALL, J. H.	c/o London Joint City & Midland Bank, 70, South Audley Street, W.1.
1920.		BAMFORD, R.	36—40, Pelham Street, S. Kensington, S.W.7.
1915.	G.	BARGMAN, J. F.	Inglefield, Osborne Road, South Farnborough, Hants.
1920.	A.M.		
1921.		BARKER, A.	"Stradbrooke," Doddington Road, Swallowbeck, Lincoln.
1909.		BARKER, P. W.	Barker's Garage, Lowesmoor, Worcester.
1908.		BARKER, W. J.	30, Cecil Mansions, Balham, S.W.
1919.		BARLOW, A. E.	"Modowaska," Monmouth Road, Pill, near Bristol.
1920.		BARNETT, E. P.	93, Brighton Grove, Newcastle-on-Tyne.
1916.		BARRETT, Capt. N. ...	A.I.D., 3—6, Clement's Inn, W.C.2.
1919.		BARTON, A. D.	21—27, Mortimer Market, Tottenham Court Road, W.C.1.
1919.		BAXTER, C.	Cleveland House, Bradford Road, Shipley, Yorks.
1916.		BAXTER, L. E.	27, Kanul Road, Hataitai, Wellington, New Zealand.
* 1911.	G.	BAYLIS, W. E.	287, Monument Road, Edgbaston, Birmingham.
1915.	A.M.		
1921.		BAYLISS, A.	"Woodly House," 45, Crescent Road, Nether Edge, Sheffield.
1907.		BEANLAND, H.	Bryngelli, Garnant, R.S.O., Carmarthenshire.
1918.		BECHTELL, G. H.	6, Palmeira Avenue Mansions, Hove, Sussex.
1919.		BECKER, Chas. J.	Earlsdon House, Earlsdon, Coventry.
1916.		BEDFORD, J.	"Ivydene," 3, Anderton Road, Sparkbrook, Birmingham.
1917.		BEERE, W. J. E.	99, Standen Road, Southfields, S.W.18.
1920.		BENEST, (Miss) C. de H.	7, Tudor Road, Sutton Coldfield.
1908.		BENNETT, A. E.	25, Thrale Road, Streatham, S.W.16.
1920.		BENNETT, J. W.	44, Upper Mall, Hammersmith, W.
* 1912.	G.	BENNY, H. B.	29, Aldbourne Road, Uxbridge Road, W.12.
1921.	A.M.		
1920.		BERTRAM, W. R. J. ...	44, Thornwood Avenue, Partick, Glasgow.
1921.		BEST, F. A.	T. Firth & Sons, Norfolk Works, Sheffield.
*g 1909.	G.	BETTERTON, W.	Universal Die Castings & Metal Co., Ltd., 7—9, Farm Lane, Walham Green, S.W.6.
1915.	A.M.		
† 1917.		BIRCH, Capt. E. W....	149, Foleshill Road, Coventry.
1920.		BIRCHALL, C.	100, Brookdale Road, Wavertree, Liverpool.
1913.		BIRRELL, E.	Engineering Depôts, Ltd., Wide Street, Selby.
* 1907.	G.	BISHOP, Major P.,	22, Arlow Road, Winchmore Hill, N.21.
1915.	A.M.	O.B.E.	
1918.		BISHOP, W. H.	12, Corporation Road, Dudley.
* 1916.		BLACKFORD, H. O. ...	3, Chipstead Street, Fulham, S.W.6.
1918.		BLAKE, E. H. O.	69, Willows Crescent, Cannon Hill, Birmingham.

Date of Election.			
1909.	G.	Blanch, T. A.	61—71, Church Street, Chelsea, S.W.
1920.	A.M.		
1919.		Bohringer, A. J.	50, Castlenau, Barnes, S.W.13.
1917.		Bolton, D. C.	The Little House, Nelson Road, Bournemouth West.
1920.		Bond, F. W.	Thorncliffe, Brighouse.
1919.		Bonn, C. R. H.	56, Gibson Street, Hillhead, Glasgow.
1917.		Boor, A.	3232, Collingwood Avenue, Toledo, Ohio, U.S.A.
1918.		Booth, A. G.	27, Allen Road, Wolverhampton.
1919.		Boucher, J. W.	38, Finniemore Road, Little Bromwich.
1911.		Boult, E. F.	Wolseley Motors, Ltd., P.O. Box 1175, Melbourne.
1910.	G.	Bower, G. S.	7, Bampton Street, Loughborough.
1918.	A.M.		
1919.		Bowkett, N. W.	36, Hordern Road, Wolverhampton.
1920.		Bownes, E.	Regal Castings Co., Ltd., Burton Street, Peterborough.
1920.		Boyd, R. H.	Hyatt, Ltd., Thurloe Place, S. Kensington, S.W.7.
1917.		Bradfield, W. S.	Avondale, Burton Road, Lincoln.
* 1910.	G.	Bradley, A. P.	Light Car & Cyclecar, 7—15, Rosebery
1917.	A.M.		Avenue, E.C.1.
1920.		Bradley, R. P.	41, Deans Road, Hanwell, W.7.
1919.		Bradshaw, G. G.	38, Hartington Street, Derby.
1909.	G.	Brayshaw, H. A.	5, College Lane, Paradise Street, Liver-
1918.	A.M.		pool.
1908.		Breakell, F. B.	159, Waterloo Road, Preston.
1909.		Brearley, J. W.	11, Elphinstone Road, Poona, India.
1920.		Bretton, H. J.	91, Brick Lane, E.1.
1919.		Broadsmith, H. E. ...	Australian Aircraft Engineering Co., 12, Bridge Street, Sydney, N.S.W.
1913.		Brock, F. G.	"Montmore," 7, Sommers Crescent, Ilfracombe.
1914.		Bromhead, Capt. E. G., R. E.	10, Warwick Chambers, Corporation Street, Birmingham.
1918.		Brough, G.	Hadyn Road, Nottingham.
1917.		Brown, Lieut. M. H.	The Palace Hotel, W.C.1.
1918.		Browning, B. E.	54, Elm Road, Hale, Altrincham.
1913.		Bryan, L. C.	Station Works, Clacton-on-Sea.
1913.		Buchanan, W. M. ...	Enfield, Campbell Street, Wishaw, N.B.
1920.		Buckingham, W. R....	"Fairhaven," Whitton Road, Twickenham.
1914.		Buckley, A. E.	
1913.		Buckley, J. F.	166, Queens Road, Buckhurst Hill, Essex.
1912.	G.	Bull, A. A.	Westlawn Boulevard, Strathmoor, Detroit, Mich., U.S.A.
1918.	A.M.		
1915.		Bull, C. W.	The Gables, Windmill Road, Brentford.
†* 1908.	G.	Burchall, Lt.-Col. H.,	"Tintern," Lime Tree Avenue, Esher,
1913.	A.M.	D.S.O.	Surrey.
1921.		Burleigh, W. H.	The Daimler Co., Ltd., Highgate Hill, Upper Holloway, N.
1908.	G.	Burney, C. S.	c/o Amac, Ltd., Aston Cross, Birmingham.
1915.	A.M.		
1920.		Burt, S.	15, Conduit Street, W.1.
1916.		Burton, R. B.	Rolls Royce of America, Inc., Springfield, Mass., U.S.A.

Date of Election.			
1919.		BURTON, Wm.	100, Willow Avenue, Edgbaston, Birmingham.
1910.		BUYER, G.	36, Queenswood Road, Forest Hill, S.E.23.
1919.		CALVERT-HARRISON, Capt. J. A.	The Tower House, Rothley, Leicester.
1908.	G.	CAREY, C. M.	Casilla, 284, Vind-del-Mar, Chili.
1913.	A.M.		
1914.		CAREY, F.	Hazledean, Kingswear, Devon.
1914.		CARLAW, R. H.	11, Finnieston Street, Glasgow.
1920.		CAROLIN, C.	c/o Mrs. Woods, Cakeham Road, West Wittering, nr. Chichester.
* 1913.	G.	CARPENTER, T. D.	35, Airedale Road, Balham, S.W.12.
1915.	A.M.		
1911.		CARROLL, H. E.	Huntington, Chertsey, Surrey.
1918.		CARTER, W. G.	Car & General Insurance Corporation, Ltd., 88, Bath Street, Glasgow.
1915.		CASTELLO, W. E.	28, Dryburgh Road, Putney, S.W.15.
1918.		CATHERWOOD, W. A..	20, Woodbourne Avenue, Streatham, S.W.15.
* 1913.	G.	CAUDWELL, E.	5, Cornwall Road, Coventry.
1917.	A.M.		
1915.		CAVE, H. A. S.	164, St. Paul's Road, Moseley, Birmingham.
1911.		CHALMERS, A.	102, Bathurst Gardens, Willesden, N.W.10.
1914.		CHALMERS, R. P.	72, Comiston Drive, Edinburgh.
*g 1912.	G.	CHALMERS, T. E. B....	The Dingle, Shirley, Birmingham.
1914.	A.M.		
1920.		CHAMBERS, H. J.	c/o A. Milton & Co., Ltd., 156, Dharamtala Street, Calcutta.
1912.		CHAPMAN, A. W.	Putney Bridge Station Garage, S.W.15.
1914.	G.	CHARGE, H. R.	19, Lynford Gardens, Goodmayes, Essex.
1920.	A.M.		
1920.		CHARLES, H. N.	Grove House, Hadley, Barnet, Herts.
1920.		CHATFIELD, W. T.	Box 213, Salisbury, Rhodesia.
1920.		CHATTERTON, P. B. ...	c/o 103, Rugby Road, Leamington Spa.
1913.		CHILLINGWORTH, P. ...	The Broadway, Newbury.
1920.		CLARE, L. J.	Dulce Domum, Weston Park, Thames Ditton.
1915.		CLARK, F. A.	40, Iron Mill Lane, Crayford, Kent.
1921.		CLARK, V. I. R.	6, Tyne Terrace, North Shields.
1920.		CLAUSEN, H. C.	R.E.D., 3, Wydnor, South Bethlehem, Penn., U.S.A.
1919.		CLEASE, A. G. D.	19, Hertford Street, Coventry.
1915.		COCKERILL, J. F.	c/o F. Huth & Co., 12, Tokenhouse Yard, E.C.
1919.		COLLIER, H. A.	44, Plumstead Road, Plumstead, S.E.
1915.		COLLINS, A. J.	11, Elm Park Gardens, N. Acton, W.
1917.		COLLINS, W. F.	91, Wrottesley Road, Harlesden, N.W.
1919.		CONINGSBY, H. R.	Wolseley Motors, Ltd., Globe House, 5, Hout Street, Capetown, c/o P. O. Box 2493.
1909.		COOK, A. E.	136, Pitt Street, Leith.
1912.	G.	COOPER, Lieut. H. J...	R.A.S.C., Officers' Mess, Buller Barracks, Aldershot.
1920.	A.M.		
1912.		CORNFIELD, M.	Ranipet, Madras Pres., India.
1917.		COSTLEY, J. P.	Belhaven, Ltd., Wishaw, N.B.

Date of Election.			
1920.		COTSWORTH, G. W. ...	Field View, Charlton Road, Shepperton.
* 1919.		COULSON, R.	Rosecroft, Kenilworth.
1919.	G.	COWELL, F. R.	9, Blenheim Square, Blackman Lane,
1920.	A.M.		Leeds.
1919.		COX, C. H.	"Kingshurst," 19, St. Mary's Road, Leamington Spa.
1921.		COZENS, E. N.	26, St. Jude's Road West, Wolverhampton.
1920.		CRAMP, S. G.	Inglewood, Howard Road, S. Yardley, Birmingham.
1910.	G.	CRAWLEY, Sub-Lieut.	Sheffield Simplex Motor Works, Tinsley,
1913.	A.M.	R. C.	Sheffield.
1920.		CREAGER, F. L.	Fairbanks, Morse & Co., Beloit, Wisc., U.S.A.
* 1912.	G.	CROMIE, W. H.	110, Barcombe Avenue, Streatham Hill,
1914.	A.M.		S.W.2.
1914.		CROOK, Walter	Hotel Stuart, Richmond Hill, Richmond, Surrey.
1909.		CROSHER, C. R.	47, Henry Road, West Bridgford, Notts.
1920.		CROSS, J.	35, Clegg Street, Oldham.
1919.		CROWTHER, C.	Jno. Croall & Sons, Ltd., 310A, St. Vincent Street, Glasgow.
1916.		CRUICKSHANK, G. M..	6, Blakesley Avenue, Ealing, W.5.
1911.		CUMMINGS, H. T.	35, Quayside, Newcastle-on-Tyne.
1920.		CUNDY, F. G.	22, George Street, Hanover Square, W.1.
1920.		CURRIE, J.	151, Penn Road, Wolverhampton.
1919.		CURRIN, S. A.	"Queenslea," 23, Brixton Hill, S.W.2.
1908.		CUTBUSH, G. H.	"The Motor World," 73, Dunlop Street, Glasgow.
1918.		DALLEY, P. H.	15, Seagrave Road, Coventry.
1917.		DANIELL, J. A.	Rudge Whitworth, Ltd., Coventry.
1914.		DANIELS, W.	88, South Brae Drive, Jordanhill, Glasgow.
1912.		DARNELL, H.	Severus House, York.
1918.		DAVEY, A. G.	E. G. Wrigley & Co., Foundry Lane Works, Soho, Birmingham.
1911.	G.	DAVEY, A. T. H.	6, St. James's Square, Holland Park,
1914.	A.M.		W.11.
1909.		DAVIES, Capt. E. K.	11, Egliston Road, Putney Common, S.W.15.
1910.	G.	DAVIES, E. O.	Owen Davies Engineering Co., Ltd.,
1919.	A.M.		Ballards Lane, North Finchley, N.12.
1911.		DAVIES, W. D.	3, Goat Street, Swansea.
1913.		DAVIS, Capt. S. H. ...	15A, High Street, St. John's Wood, N.W.
1909.		DAVISON, E. G.	5, Esplanade, Lowestoft.
1911.		DEACON, A. E.	Imperial Buildings, Vaughan Street, Llandudno.
1912.		DEAKIN, A. W.	Middlesex Autocar Co., West Heath Drive, Golder's Green, N.W.4.
1907.		DEAKIN, W.	Thornleigh, Oakfield Road, Selly Park, Birmingham.
1914.	G.	DE LA GARDE, L. A....	Progressive Motor Co., 93, Mortimer
1917.	A.M.		Street, W.1.
1914.		DERMIGNY, H.	19A, Carlyle Square, Chelsea, S.W.3.
1907.		DE SILVA, Louis	3, Belsize Place, Belsize Lane, Hampstead, N.W.

Date of Election.			
1918.		DEVERSON, F. C.	Car Genl. Insce. Corp., 7, Bank Buildings, St. Mary Street, Cardiff.
1914.		DEWAR, P.	Harperfield, Sandilands, Lanarkshire.
1915.	G.	DEWHURST, F.	c/o 402, Firth Park Road, Pitsmoor,
1916.	A.M.		Sheffield.
1917.		DEWHURST, Frank	Hublet, Harry & Co., Westwood, Peterborough.
1918.		DICKINS, W. E. B. ...	15, George's Street, Northampton.
1919.		DOBSON, C. O.	Dobson & Young, Plunkett Street, East Sydney, N.S.W.
1912.		DOMMETT, Capt. W. E.	14, Cranes Park, Surbiton.
1920.		DON, G.	1, Haylyn Street, Whiteinch, Glasgow.
* 1909.		DORLING, J. A.	32, Tollington Road, Holloway Road, N.7.
1920.		DORMER, W. Fred	14, Chase Court Gardens, Enfield.
1913.		DRAKE, L. S.	Town Hall, Wellington, New Zealand.
1916.		DRAPER, H.	Icknield, Wellington Street, Lytham.
1915.		DROVER, Capt. David..	c/o The Royal Automobile Club, Pall Mall, S.W.1.
1919.		DUDLEY, J. A.	27, Lower Villiers Street, Wolverhampton.
1918.		DUFFY, J.	857, Stratford Road, Hall Green, Birmingham.
1920.		DUNSMUIR, A. C.	61, Westmoreland Street, Crosshill, Glasgow.
1920.		DURLACHER, C.	c/o Seymour Bingham & Co., *via* Melzo 12, Milan, Italy.
1919.		DUXBURY, S.	
1919.		DYE, A. H.	
1907.		EASTAUGH, Gordon	240, Evering Road, Upper Clapton, N.E.5.
1920.		EASTWOOD, Geo.	Lochar House, Locharbriggs, Dumfries, N.B.
1920.		EDMOND, E. W.	3, Mill Vale Villas, Coronation Avenue, Alvaston, Derby.
1917.		ELLIOTT, E. S.	31, Riffel Road, Cricklewood, N.W.
1919.		ELLIOTT, P. L. D. ...	9, Ingoldsby Avenue, Manchester, S.E.
1921.		ELLIS, H. C.	Cole Allen, Edenbridge, Kent.
1921.		EMERSON, K. B.	Fiat Motors, Ltd., 5, Albemarle Street, W.1.
1920.		EMTAGE, E. L. M. ...	Room A. 243, Directorate of Research, Air Ministry, Kingsway, W.C.2.
* 1915.	G.	EUGEN, C. M. van	72, Darlington Street, Wolverhampton.
1917.	A.M.		
1911.		EVANS, C. W.	Al Fresco, Ray Lea Road, Maidenhead.
1919.		EVERNDEN, H. I. F....	"Camacha," West Wittering, nr. Chichester, Sussex.
1914.		EVINSON, C. M.	3, West Hill Avenue, Mansfield, Notts.
1916.		FAIRBROTHER, Lieut. E.	11, Syon Crescent, Spring Grove, Isleworth.
1918.		FAIRRIE, Geo.	253, Vauxhall Road, Liverpool.
*g 1911.	G.	FARNELL, M.	50, Manningham Lane, Bradford.
1920.	A.M.		
1918.		FENN, A. P.	15, Second Avenue, Selly Park, Birmingham.
1913.		FERGUSON, Harry	May Street, Belfast.
1920.		FERGUSON, P.	Mauldslie, Craigie, Perth.

Date of Election.			
1919.		FINCKEN, Geo.	Conham Hall, Hanham, Glos.
1919.		FIRTH, E. L.	The Grove, Hartlebury, nr. Kidderminster.
1912.		FISHER, H. R.	27, Lawn Crescent, Kew Gardens.
1914.		FLEMING, S.	Armstrong & Fleming, King Street, Penrith.
1920.		FLOOD, C. J. M.	5, Ruvigny Mansions, Putney, S.W.15.
1919.		FORBES, V. A.	The Palace Garage, Jodhpur, India.
1919.		FORD, W. V.	The D. F. M. Engineering Co., Drummond Works, Vane Street, Wolverhampton.
1915.		FORREST, H. R.	50, Fleet Street, E.C.4.
1907.		FORSTER, G. E.	5, New Burlington Place, Regent Street, W.
1907.		FOSTER, T. M.	Lindum House, Aldershot.
1910.	A.	FOX, R. S.	"The Old Orchard," Harefield, nr.
1914.	A.M.		Uxbridge, Middlesex.
1919.		FRY, Capt. H. D.	Ashcroft, Cirencester, Glos.
* 1916.		FUNCK, Geo.	Violette, Quinton Road, Coventry.
1920.		GACCON, F.	Albion House, Charles Street, Cardiff.
1920.		GALBRAITH, T.	35, Marley Road, Levenshulme, Manchester.
1920.		GALLAGHER, Capt. H. N.	Restharrow, Link Lane, Wallington, Surrey.
1908.	G.	GAMBELL, H. C.	5, Broadwater Road, Worthing.
1917.	A.M.		
1917.		GARBUTT, Hy.	Charlton House, Old Charlton, S.E.7.
1919.		GARTSIDE, G. S.	179, Carholme Road, Lincoln.
1920.		GAWKRODGER, W.	859, Ashton New Road, Clayton, Manchester.
1908.		GAYDON, H. A.	"Medstead," Sefton Road, Addiscombe, Croydon.
1912.		GIBLETT, Major R. H.	Bryngwyn, Milton-u.-Wychwood, Oxford.
1920.		GILBERT, Geo.	Gilbert & Son, Ltd., Lindum Works, Pelham Street, Lincoln.
1921.		GIRLING, A. H.	c/o Ferodo, Ltd., 222, Tottenham Court Road, W.1.
1919.		GLASS W.	The Corner House, St. James' Avenue, Hampton Hill, S.W.
1907.		GNAPP, G. C.	49, Old Bond Street, W.1.
1921.		GODDARD, G.	"The Ashes," Icklesham, Sussex.
1907.		GOLDING, H. A.	River View, 8, Maidstone Road, Chatham.
1916.		GOTSHALL, R. W.	Holt Manufacturing Co., Peoria, Ill., U.S.A.
1914.		GOULD, Major A. H. B., D.S.O., M.C.	c/o A. E. Gould, Ltd., 55—57, Shaftesbury Avenue, W.C.
1921.		GOWRING, E. C.	"Penrith," Cambridge Road, Farnborough, Hants.
1913.		GRACE, Capt. C. H....	51, Trafford Road, Thornton Heath, Surrey.
1917.		GRANT, Lieut. R. J.	235, Prospect Road, Woodford Green, Essex.
1907.		GRAVENOR, J.	"Lyndale," Firgrove Hill, Farnham, Surrey.
1911.		GRAY, A.	A. Gray & Co., 7—8, Woodbridge Road, Guildford.

Date of Election.			
1920.		GRAY, Gordon	244, Langside Road, Glasgow.
1918.		GRAY, W. H.	Bankside, Ferry Road, Teddington, Middlesex.
1916.		GREEN, A.	...Engineering School, Nagpur, C. P., India.
1911.	G.	GREEN, H. S.	201, Mount Road, Madras, India.
1912.	A.M.		
1921.		GREENING, S. M.	J. A. Prestwich & Co., Ltd., Northumberland Park, Tottenham, N.17.
*d 1917.	G.	GRIFFITHS, L.	18, Grafton Street, Coventry.
1920.	A.M.		
1919.		GRINHAM, E.	St. Kilda, No. 3 Flat, 1, Latham Road, Southport.
1918.		GRONE, A. A.	Chatsworth House, Chatsworth Gardens, Acton Hill, W.3.
1914.		GROSE, A. M.	
1917.		GROSE, W. T.	Northampton Motor Omnibus Co., Ltd., Pike Lane, Marefair, Northampton.
1914.		GROVES, Lt. R.	Mechanical Transport Training School, Dehra Dun, India.
1917.		GUILDFORD, C. H. ...	St. Helens, Ivy Road, Cricklewood, N.W.2.
1919.		GUNN, Wm.	c/o Wolseley Motors, Adderley Park, Birmingham.
1919.		HADLEY, A. A.	Brierley Motor Transport Engineering Co., Mill Street, Brierley Hill.
1917.		HALLET, A.	33, Dawson Street, Dublin.
* 1913.	G.	HALLIWELL, F. W. ...	c/o Swift of Coventry, Cheylesmore, Coventry.
1918.	A.M.		
1911.	G.	HAMILTON, Major J.	Tomabil, Strathfield, Sydney, Australia.
1919.	A.M.		
1908.	G.	HAMILTON-GRAPES, G.	P. O. Box 1128, Melbourne, Victoria,
1911.	A.M.		Australia.
1912.		HAMMOND, W. R.	
1919.		HANDS, L. N.	38, Dalgarno Gardens, W.10.
1909.	G.	HANNAH, W. J.	97, Hill Street, Garnethill, Glasgow.
1914.	A.M.		
1913.		HARDWICKE, J. A. W.	Caixa, 76, Santos, Estado de S. Paulo, Brazil.
1913.	G.	HARDY, W. G.	Whyke Hut, East Wittering, nr.
1919.	A.M.		Chichester, Sussex.
1916.		HARGRAVES, A.	Firestone Tire Co., Akron, Ohio, U.S.A.
1918.		HARLING, T. F.	259, Padiham Road, Burnley, Lancs.
1914.		HARRINGTON, L. A. ...	Court Lodge, Ewhurst, Sussex.
1920.		HARRISON, J.	41, The Grove, Hammersmith, W.6.
1920.		HARRISON, R. N.	Box 201, Royal Automobile Club, Pall Mall, S.W.1.
1908.		HARRISON, T. Crespin	59, The Drive, Fulham Park Gardens, S.W.6.
1909.		HART, E. T.	12, New Cartwright Street, Wolverhampton.
1907.	G.	HARVEY-TOMEO, W. ...	103, Standen Road, Southfields, S.W.
1915.	A.M.		
1916.	G.	HASLER, Pilot Officer	Lucyville, Somerset Road, Tunbridge
1917.	A.M.	H. N.	Wells, Kent.
1912.		HAWES, S. H.	127, Wentworth Road, Golder's Green, N.W.11.

Date of Election.			
1918.		HAWKES, W. D.	19, Pennard Mansions, Goldhawk Road, W.12.
1911.		HAYDON, F. G.	"Fairview Lodge," Osborne Road, Farnborough, Hants.
1908.		HAYES, H. G.	4, Maurice Road, Bishopston, Bristol.
1908.	G.	HAYLES, R. B.	12, Winchester Road, Andover, Hants.
1914.	A.M.		
1920.		HAYWARD, W. G. C....	The Cottage, St. Margarets Road, St. Margarets-on-Thames.
†*g 1910.	G.	HEATHER, D. S., B.Sc.	B.S.A. Co., Ltd., Montgomery Street, Sparkbrook, Birmingham.
1918.	A.M.		
l 1918.		HEATON, C. B.	Oxford Lodge, Ewell Road, Surbiton.
1920.		HEBBLETHWAITE, A. C.	c/o W. G. Heath & Co., Mill Street, Plymouth.
1921.		HEDGCOCK, F. W.	7, Carlton Road, Stroud Green, N.4.
1920.		HELLON, K. A.	43, Napier Road, Ponder's End, Middx.
1909.		HEMENS, H. A.	129, Lyham Road, Brixton Hill, S.W.2.
1915.	G.	HENDERSON, E. H. ...	Firth-Derihon Stamping Works, Carbrook Road, Sheffield.
1917.	A.M.		
1918.		HENDERSON, L.	Aero Works, Fitzwilliam Street, Sheffield.
1915.	G.	HENDERSON, T.	15, Syon Street, Tynemouth, Northumberland.
1918.	A.M.		
1913.		HERBERT, A. G. A. ...	2, Cecil Road, Harlesden, N.W.10.
1916.		HERON, S. D.	Power Plant Section, Air Service, Engineering Divn., McCook Field, Dayton, Ohio, U.S.A.
1916.		HIGGS, G. G.	34, Cambridge Avenue, Kilburn, N.W.6.
1919.		HIGHAM, W. H. M. ...	3, Regent Road, Harborne, Birmingham.
1914.		HILL, Capt. R. B. ...	Kampala, Uganda.
1911.		HILL, S. F.	Lloyds Bank Chambers, Darlington.
1913.		HILL, W. B.	23, Stagg Lane, Edgware.
1920.		HOARE, Capt. H. G....	c/o C. R. McGrigor & Co., 39, Panton Street, S.W.1.
1918.		HOLLIDAY, H. O.	P. O. Box 509, Rangoon, India.
1919.		HOLLOWAY, R.	27, Royal Exchange Square, Glasgow.
1919.		HOLT-WHITE, Major F. R.	8, Mares Garden, Cheyne Walk, Chelsea, S.W.
1920.		HOPE, Capt. A. R., R.A.S.C., I.M.T.	c/o C.I.M.T., 2, Lake Buildings, St. James' Park, S.W.1.
†* 1907.	G.	HOUSE, C. E. G.	Board of Trade, Dept. of Overseas Trade, 4, Queen Anne's Buildings, Old Queen Street, S.W.1.
1913.	A.M.		
1918.		HOYLAND, S.	The Old Hall, Nessfield, nr. Ilkley.
1920.		HUDSON, A.	9, San Remo Parade, Westcliff-on-Sea.
1919.		HUDSON, Birt.	
1921.		HUGHAN, W. J.	24, Auckland Road, Sparkbrook, Birmingham.
1913.		HUMM, R.	R. Humm & Co., Plymouth.
1921.		HUNTER, A.	95, Cavendish Road, Clapham Park, S.W.
1908.		HUNTER, A. R.	Welbeck, Worksop, Notts.
1907.	G.	HUNTER, M. C.	Hunter, Scott & Pearce, 1, Tower Street,
1908.	A.M.		York.
1920.		HUNTLEY, Lt. G. E....	80, Roach Road, Sheffield.
1918.		HUSBAND, J.	15, Thomson Avenue, Johnstone.
1919.		HUTCHINGS, R. R.	22, First Avenue, Acton, W.3.
1920.		HUTCHINSON, T.	8, Ebor St., Heaton, Newcastle-on-Tyne
1907.	G.	HUTT, A. C.	Tudor House, West Malling, Kent.
1919.	A.M.		

Date of Election.			
1920.		HUTTON, J. M.	205, Talbot Road, Stretford, Manchester.
1909.		HYLAND, A.	Hughes Road, Bombay, India.
1917.		HYLAND, Capt. C.	Woodlands, London Road, St. Albans.
1921.		INCE, T. A.	127a, Murray Road, South Ealing, W.
1921.		INGRAM, J.	St. Leonard's, Hayling Island, Hants.
1911.	G.	IRELAND, A. J.	14, Winthorpe Road, Putney, S.W.
1917.	A.M.		
1917.		JACK, R. K.	604, Kalamazoo Street, Lansing, Mich., U.S.A.
1916.		JACK, R. W.	Lawsons Garages, Girvan.
1920.		JACKSON, C.	21, Stainton Road, Endcliffe, Sheffield.
1910.	G.	JACKSON, E. A.	"Highcliffe," Doncaster.
1915.	A.M.		
1919.		JACKSON, E. W.	Motor Section, Post and Telegraph Dept., Wellington, New Zealand.
1919.		JACKSON, G. G.	11, Wilson Road, Wallasey, Cheshire.
1914.	A.	JACKSON, P. L.	234—3, Lower Circular Road, Calcutta.
1919.	A.M.		
1918.		JACKSON, W.	36, Temple Grove, Golder's Green, N.W.4.
1912.		JACOBS, G. H.	May & Jacobs, Ltd., Guildford, Surrey.
1920.		JACQUES, A. V.	34, Weston Park, Crouch End, N.8.
1920.		JAGGER, F. G.	L. & Y. Rly., Chief Mechl. Engrs. Office, Newton Heath, Manchester.
1917.		JEHLE, F.	Aluminium Manufacturers Incr., 2800, Harward Avenue, Cleveland, Ohio, U.S.A.
1921.		JOHNSON, E. W.	3, Spencer Mansions, Queen's Club Gardens, W.14.
1919.		JOHNSON, F. E. H. ...	Wolseley Motors, Ltd., 77—88, Avenue Road, Toronto, Canada.
1909.		JOHNSON, H.	61, St. Kilda's Road, Stoke Newington, N.16.
1909.	G.	JOHNSON, N. W.	Armstrong-Siddeley Motors, Ltd., Gt.
1919.	A.M.		Northern Garage, Watson Street, Manchester.
1918.		JOHNSON, T. N.	c/o Mr. T. Wight, 81, Stanley Street, Berhampore, Wellington, New Zealand.
1921.		JOHNSON, W. P.	Technical College, Loughborough.
1913.	G.	JOHNSTON, Major C. H.	Barkarby, Sweden.
1916.	A.M.		
1909.		JOLLY, J. C.	3, Brompton Avenue, Sefton Park, Liverpool.
1916.		JONES, A. E.	Jones & Collett, 17, Cleveland Street, Wolverhampton.
1917.		JONES, D. A.	178, Elms Road, Clapham Common, S.W.
1913.		JONES, J. H.	Belmont Engineering Co., Ltd., Wellington Road North, Stockport.
1913.	G.	JONES, O. T.	628, Coventry Road, Small Heath, Birmingham.
1919.	A.M.		
1907.	G.	JONES, P. Muir	80, Maidstone Road, Rochester.
1915.	A.M.		
1916.	G.	JONES, Lt. T. P.	c/o Sir C. R. McGrigor & Co., Panton
1917.	A.M.		Street, Haymarket, S.W.1.

Date of Election.			
1916.	G.	JOYCE, P. J.	101, Avondale Road, Sparkhill, Birmingham.
1918.	A.M.		
1914.		JUDGE, Lieut. A. W.	Wroxham, Cambridge Road, Bromley, Kent.
*g 1911.	G.	JUNNER, G. M.	c/o The Commercial Motor, 7—15, Rosebery Avenue, E.C.1.
1919.	A.M.		
1913.		KEEP, T. B.	69, Bishops Mansions, Fulham, S.W.6.
1919.		KELLY, W. F.	Central Service Motor Co., 54, Upper Baggot Street, Dublin.
1908.		KEMP, A. F.	"Brown Down," Datchet, Bucks.
1912.		KEMP, E.	70, Canford Road, Clapham Common, S.W.
1920.		KENT, S. T.	Glenroyd, Mansfield.
1912.		KENYON, A.	126, Cecil Street, Moss Side, Manchester.
1918.		KENYON, J.	342, Spon Lane, West Bromwich, Staffs.
1917.		KESTERTON, H. M. ...	Ivy Nook, Hampton-in-Arden, Birmingham.
1919.		KING, A.	Glen Lyon, Compton Road, N.21.
1921.		KING, C. E.	Vauxhall Motors, Ltd., Kimpton Road, Luton.
1914.		KING, C. F. L.	"Coverdale," Bristol Road, Edgbaston, Birmingham.
1919.		KING, W. R.	6, Dryburgh Gardens, Kelvinside, N., Glasgow.
1920.		KNAGGS, M. J.	133, Holland Road, Harlesden, N.W.10.
1920.		LAMB, J. C.	Rose Villa, W. Newport, Fife.
1920.		LAMBERT, H. G. R. ...	166, Maldon Road, Colchester.
1911.		LAMBERT, Major A. W., O.B.E.	87, Ennerdale Road, Richmond, Surrey.
1917.		LAMPARD-VACHELL, B. E.	Pembroke College, Cambridge.
1919.		LAMPLUGH, H. A.	Bowden Brake Co., Ltd., Tyseley, Birmingham.
1915.		LANDER, Chas.	66, Foxbourne Road, Upper Tooting, S.W.
1912.		LANDMARK, J. S.	29, Norfolk Road, Thornton Heath, Surrey.
1913.		LAWRENCE, R. M. ...	c/o F. Sage & Co., Ltd., Casilla Correo, 1699, Buenos Aires, Argentine.
1921.		LEADBETTER, T. A. N.	The Commercial Lorry & Engineering Co., Ltd., Ashburton Road, Trafford Park, Manchester.
1919.		LECHMERE, W. F. ...	75, Gladstone Road, Sparkbrook, Birmingham.
1910.	G.	LEE, F. H.	A. H. Wheeler & Co., Elgin Road, Allahabad, India.
1918.	A.M.		
1916.		LESSELLS, J. M.	809, South Avenue, Wilkinsburg, Pa., U.S.A.
1920.		LESTER, H. G.	59, Pembroke Crescent, Hove, Sussex.
1914.		LEWIS, A. E.	Tremenheere, Gyllyngvase Road, Falmouth, Cornwall.
1913.		LIDDELL, C. B.	62, Park Street, W.1.
1916.		LINDSAY, R. J.	Burn Cottage, Longtown, Cumberland.
1908.		LOMAX, S.	Rivington House, Woodford Wells, Woodford Green, Essex.

Date of Election.		
1921.	Longstaffe, B. H. ...	26, Hadley Gardens, Turnham Green, Chiswick, W.4.
1911.	Lonsdale, H. M.	79, Station Road, Market Harborough.
1918.	Lord, A. O.	Cambridge House, Twickenham.
1908.	Loughborough, T. W.	The Mill House, Warlingham, Surrey.
1920.	Love, L. A.	89, Chesnut Road, Plumstead, S.E.18.
1918.	Low, E. H.	187, Lansdowne Road, N.17.
1918.	Lowe, J., B.Eng. (Lond.)	210, Walthall Street, Crewe.
1918.	Lower, W. P.	22, Airlie Place, Harehills, Leeds.
1914.	Luyks, E. S.	159, Alexander Road, Acock's Green, Birmingham.
1912.	Lynden, Lester A. ...	
1918.	Lyth, C. J.	148, Earlsdon Avenue, Coventry.
1918.	McCall, A. K.	c/o R. Boby, Ltd., Engineers, Bury St. Edmunds.
1917.	McCallum, D. A. ...	65, East Claremont Street, Edinburgh.
1921.	McConnell, H. P. ...	199, Piccadilly, W.
1920.	McFarlane, D.	257, Mears Street, Sunnyside, Pretoria, Transvaal.
1919.	MacDonagh, F. M. ...	Walker Bros., Ltd., Motor Dept., Pagefield Ironworks, Wigan.
1918.	MacFarlane, D.	38, Cheltenham Terrace, Heaton, Newcastle-on-Tyne.
1920.	Macdonald, W. H. ...	3, Philbeach Gardens, S.W.
1911.	McGillivray, J. A...	77A, Alexandra Road, Swiss Cottage, N.W.
1920.	McIntosh, D. C.	78, Marlborough Avenue, Broomhill, Glasgow, W.
1911.	Mackay, W. M.	8, West Park, Harrogate.
1919.	Mackenzie, James	Caledonian Motor Works, La Scala Buildings, Inverness.
1912.	Mackrow, G. H.	c/o Grindley & Co., 54, Parliament Street, S.W.1.
1919.	Maclachlan, L. A. ...	
1920.	Maclean, M.	51, Keisland Terrace, Hillhead, Glasgow.
1920.	McMillan, A. L.	Ardgoil Engineering Co., Ltd., Tarbet Street, Gourock, N.B.
1910.	Maidens, Lieut. J. R.	61, Dartmouth Road, Hendon, N.W.2.
* 1913. G. 1920. A.M.	Manning, P. J.	36, Chester Street, Coventry.
1914.	Mansfield, A. W. ...	Caffyns, Ltd., Motor Dept., Seaside Road, Eastbourne.
1913.	Marner, S. S.	The Den, Whitgift Street, Croydon.
1919.	Marris, R. W.	c/o G. Mackenzie & Co., Ltd., Rangoon.
1908.	Martin, H. P.	c/o Martin & Handasyde, Brooklands Aviation Ground, Weybridge.
1920.	Martin, J. A.	6, Kirklee Road, Kelvinside, Glasgow.
1917.	Martin, T.	678, Alexandra Parade, Dennistoun, Glasgow.
1919.	Marvin, E. G.	70, Strafford Road, Barnet, N.
1919.	Mathers, J. H.	"Brinscall," Manor Court Avenue, Nuneaton, Warwickshire.
1914.	Mathias, Capt. J. S.	Carlton House, Regent Street, S.W.1.
1917.	May, N. A.	24, Balshagray Avenue, Partick, W., Glasgow.
1920.	Mead, G. J.	Wright Aeronautical Corporation, 238, Lewis Street, Paterson, N.J., U.S.A.

Date of Election.			
1921.		MELDRUM, C.	Albion Motor Car Co., Ltd., Stirling Chambers, Campo Lane, Sheffield.
1911.		MELDRUM, H. J.	Heather Lea, Barrington Road, Altrincham.
1912.		MEYER, Lieut. T. B...	10, Crossfield Road, N.W.3.
1913.		MILLAR, Jackson, Jr..	c/o Halley's Industrial Motors, Ltd., Yoker, Glasgow.
1918.		MILLS, Major H. C....	4N, Bickenhall Mansions, Gloucester Pl., W.1.
1912.		MILTON, W. H.	Floyers House, Haven Road, Exeter.
1911.		MILWARD, W. F.	W. S. Laycock, Ltd., Victoria Works, Millhouses, Sheffield.
1909.	A.	MIRZA, P. D.	Bhavnagar State, Kathiawar, India.
1910.	A.M.		
1919.		MITCHELL, A. P.	"Belgrave," Thornycroft Lane, Fallings Park, Wolverhampton.
1917.		MITCHELL, J. B.	Sussex House, Tudor Road, Upper Norwood, S.E.19.
1907.		MOBBS, Herbert	3, Sheep Street, Northampton.
1908.		MORGAN, F. J.	Honeyvale, Balur, Kadur District, Mysore, India.
1920.		MORGAN, R. T.	60, St. Oswald's Road, Small Heath, Birmingham.
1913.	G.	MORTIMER, J. B.	British & American Motors, Ltd., 81, Avenue Road, Toronto, Canada.
1919.	A.M.		
1912.		MOSSAY, P. A.	7, Princes Street, Westminster, S.W.1.
* 1914.	G.	MOYES, E. H.	51, Lyndhurst Road, Luton.
1918.	A.M.		
1919.		MUIR, R. S.	69, Camberwell Grove, S.E.
1912.	G.	MUIRHEAD, D. P.	24, Kilmalling Road, Cathcart, Glasgow.
1913.	A.M.		
1920.		MUSKETT, H. E.	64, Finnemore Road, Ideal Village, Birmingham.
1920.		MYERS, E. R.	61, Kremlin Drive, Stoneycroft, Liverpool.
1921.		NASH, Capt. A. Fraser	G. N., Ltd., East Hill, Wandsworth, S.W.18.
1918.		NASH, Capt. E. C. W.	c/o Sir C. R. McGrigor, Bart., & Co., 39, Panton Street, S.W.1.
1919.		NAYLOR, J. T.	22, Union Street, Leyland, Lancs.
* 1912.	G.	NEALE, F.	Beaconsfield Place, Kirkcudbright, N.B.
1919.	A.M.		
1920.		NEEDHAM, S. O.	Argson Engineering Co., 3, Kendall's Mews, George Street, Baker Street, W.1.
1919.		NELSON, H.	"Westray," Yarborough Road, Lincoln.
1917.		NEW, C. T.	Hylside, Gretton, Winchcombe, Glos.
1911.	G.	NEW, Capt. R. H.....	29, Leith Mansions, W.9.
1918.	A.M.		
1918.		NEWEY, G. E.	132a, Christchurch Road, Boscombe, Bournemouth.
1918.	G.	NEWTON, R. V.	32, Sixth Street, Radford Estate, Coventry.
1920.	A.M.		
1917.		NICHOLSON, J.	34, Plane Street, Hull.
1920.		NICHOLSON, T. M. ...	147, Anlaby Road, Hull.
* 1914.	G.	NIVEN, A. M.	41, Thornwood Drive, Partick, W., Glasgow.
1920.	A.M.		

Date of Election.			
1912.		NOEL-STORR, C. E....	139, Links Road, S.W.17.
1919.		NOLAN, J.	Lower Lodge, Tidenham, near Chepstow.
* 1915.	G.	NORRIS, Hy.	21, Avondale Road, Coventry.
1920.	A.M.		
1915.		OSMAND, Lt. G.	"Berrycote," 33, Ennerdale Road, Kew Gardens.
1920.		OUTRAM, W. C.	13, Friars Terrace, Stafford.
1920.		OVENSTONE, J. K.	99, Norse Road, Scotstoun, Glasgow, W.
1919.		OWEN, W. J.	51A, St. George's Road, Wimbledon, S.W.19.
1914.		PACKER, C. A.	24, Linn Terrace, Muirend, Glasgow.
1920.		PADGEN, W. J.	J. A. Wilding & Co., Ltd., 3, Wilson Street, Drury Lane, W.C.2.
* 1910.	G.	PAGE, N. V.	34, Cranley Gardens, Palmers Green, N.
1918.	A.M.		
1920.		PAGE, W. H.	83, Alexander Road, Acock's Green, Birmingham.
1919.		PAGIS, A.	70, Elspeth Road, Clapham, S.W.
1908.		PARISH, C.	Southlands, Beverley Road, Hull.
1918.		PARKER, Lt. R. G....	Parker's Garage, Station Road, St. Ives, Hunts.
1920.		PARKER, J. B.	Tees-side Railless Traction Board, Middlesbrough Road, South Bank, S.O.
1917.		PARKES, J. D.	Albion Motor Car Co., Ltd., Scotstoun, Glasgow.
1916.		PARNELL, F. G.	21, Suffolk Street, Pall Mall, S.W.1.
1920.		PARR, H.	20, Cavendish Road, Birkdale, Southport.
1919.		PARSON, H. W.	38, Grafton Street, Stoke, Coventry.
1911.	G.	PARSONS, W. W.	Rolls-Royce, Ltd., 83, Boulevard Gouvion, St. Cyr, Paris.
1918.	A.M.		
1919.		PATERSON, B.	Mathias & Paterson, Carlton House, Regent Street, S.W.1.
1915.		PATERSON, C. C.	
1918.		PATERSON, T. W.	77, Penny Lane, Sefton Park, Liverpool.
1918.		PAUL, F. E.	The Rookery, Blakehall Road, Carshalton, Surrey.
1914.		PAYNE, G. S.	7, Ranelagh Avenue, Hurlingham, S.W.6.
1912.		PAYNE, Lieut. L. T....	c/o Cowey Engineering Co., Ltd., Kew Gardens, Surrey.
1908.		PEARMAIN, W. J.	
1915.		PEARSE, H.	County Chambers, Marton Road, Middlesbrough.
1920.		PEARSON, G. H.	Bois Hall Tractor School, Navestock Side, Brentwood.
1919.		PEEL, W. E.	c/o British Ensign Motors, Ltd., Willesden Green, N.W.10.
1913.	G.	PELLEW, N.	
1914.	A.M.		
*g 1912.	G.	PENDRELL, A. G.	2A, Forfield Place, Leamington Spa.
1918.	A.M.		
* 1919.		PETERS, M. M.	
* 1908.	G.	PETTIT, Lieut. A. R....	13A, Ranelagh Mansions, Hurlingham, S.W.6.
1919.	A.M.		
1920.		PICKERING, G. F.	18, St. Johns Road, Golders Green, N.W.

Date of Election.			
1914.	G.	PIGOTT, C. O.	County Ground Hotel, Eaton Road,
1919.	A.M.		Hove, Sussex.
1916.		PIKE, Lieut. F. E. ...	Glen Cottage, Whyteleafe, Surrey.
* 1913.	G.	PILE, W. D.	Dun Griffan, Sydney Parade, Dublin.
1920.	A.M.		
1918.		PIZZEY, A. E. R.	Anglo-American Oil Co., St. Stephen's Chambers, Baldwin Street, Bristol.
1913.	A.M.	PLANT, A. E.	Leyland Motors, Ltd., Leyland, Lancs.
1913.		PLAYER, G. R.	"Mayfield," Cromer Road, Moseley, Birmingham.
1920.		PODD, J. K.	8, Beverley Road, Colchester.
1915.		POLLITT, Jr., J.	Rover Repair Works, 2, Primrose Hill, Dale Street, Liverpool.
1915.		POPPLEWELL, F. B. ...	E. W. Jackson & Sons, Ltd., French Gate, Doncaster.
1917.		PORTER, S. H.	2, Waterloo Street, Calcutta.
1919.		POTTINGER, A.	11, Salisbury Terrace, Devonport.
1917.		POTTINGER, S. F.	Drawing Office, Rolls Royce, Ltd., Derby.
1907.		POTTS, J. L.	Toledo Steel Works, Sheffield.
1919.		POWELL, E. M.	Jersey Cottage, Stoke Green, Coventry.
1918.		PRATT, A. C.	
1919.		PRATT, J. H.	487, Coventry Road, Birmingham.
1920.		PRESCOTT, G.	19, Station Road, West Croydon.
1912.		PRICE, F. C.	"Ingleside," Gaskell Road, Altrincham, Cheshire.
*d 1914.	G.	PRIOR, D. L.	31, Waverley Road, Kenilworth.
1920.	A.M.		
1918.		PROSSER, S. T. E. ...	Station Road, Hampton-in-Arden, Birmingham.
1913.		PUGH, Major Rhys ...	Royal Automobile Club, Pall Mall, S.W.1.
1920.		PULLINGER, (Miss) D.	Galloway Motors, Ltd., Tongland, Kirkcudbright.
1917.		PURVES, Wm. W.	Horsham Garage and Automobile Engineers, Brighton Road, Horsham.
1911.		PUTLAND, E. E.	55, Rosenthal Road, Catford, S.E.6.
1917.		RANKIN, J.	56, Grange Road, Langside, Glasgow.
1909.		RAWLINGS, F.	11, Flanders Mansions, Bedford Park, W.4.
1919.		RAYNER, F.	Rayner & Co., 5, Chancery Lane, W.C.2.
1919.		REED, Hy.	Dot Motors, 38, Ellesmere Street, Hulme, Manchester.
1919.		REED, J.	Corbridge House, Corbridge-on-Tyne.
1920.		REED, J. S.	14, Stanmore Road, Belvedere, Kent.
1918.		REEVE, L. W. C.	The Homestead, Botesdale, Suffolk.
1919.		REEVES, J. H.	The Manor, Malahide, Co. Dublin.
1921.		REID, W. J.	16, Howard Street, Gloucester.
1917.		REILLY, F. M.	Fairhall, Forest Road, Broadwater, Worthing.
* 1913.	G.	RENDALL, W. R.	73. Downs Park Road, Hackney Downs, E.8.
1919.	A.M.		
1921.		RENWICK, W. S.	4, Hale Gardens, W.3.
1917.		REYNOLD, T.	4, Richmond Grove, Byron Avenue, Levenshulme, Manchester.

Date of Election.			
1918.		RHODES, J.	Education Dept., Sudan Government, Khartoum.
1918.		RICHARDS, R. W.	824, Coventry Road, Smallheath, Birmingham.
1920.		RICHARDSON, D. H. G.	51, Flanders Mansions, Flanders Road, Bedford Park, W.4.
1920.		RICHARDSON, H.	23, Staple Hill Road, Fishponds, Bristol.
1920.		RICKARDS, J. G.	110, Hatfield Road, Ipswich.
1915.		ROBBINS, S. T.	59, Queen's Road, Wimbledon, S.W.19.
1917.		ROBERTS, H. W.	Century Buildings, 199, Deansgate, Manchester.
1916.		ROBERTS, W. St. Clare	46, Woodstock Road, Walthamstow, N.E.
1912.		ROBERTSON, Capt.H.J.	"Briarfield," Leamington.
1911.		ROBINSON, Capt.G.H.	47, Sudworth, New Brighton, Cheshire.
1920.		ROBINSON, P. S.	Rollright Engineering Co., Ltd., Chipping Norton, Oxon.
1916.		RODWAY, G. H.	Electric and Ordnance Accessories Co., Ltd., Rews Lane, Ward End, Birmingham.
1911.		RONCA, J. F.	176, Elms Road, Clapham Common, S.W.4.
1908.		ROSCOE, Capt. E. C. ..	Rushen Cottage, Gerrards Cross.
1920.		ROSE, Lt.-Col. H. de M.	
1919.		ROSS, A. H.	Alex. Ross & Co., Motor Engineers, Halley's Lane, Wellington, N.Z.
1914.		ROSS, R. H.	c/o Challands, Ross & Co., Ltd., Canal Street, Nottingham.
1920.		ROY, R. C.	71, Cromwell Grove, Levenshulme, Manchester.
1921.		ROYLE, R. S.	Anglo-American Oil Co., Ltd., Cunard Buildings, Liverpool.
1913.	G.	RUNNELS-MOSS, N. A.	97, Ealing Road, Wembley, Middlesex.
1920.	A.M.		
1913.		RUSSELL, A. A.	Brookside, Dorking, Surrey.
1918.		RUSSELL, F.	Dept. of Automobile Engineering, Technical College, Loughborough.
1918.		RUSSELL, JAMES	Messrs. Croall & Croall, Hawick.
1921.		RYAN, D. V.	33, Victoria Road, Falling's Park, Wolverhampton.
1920.		SALTER, F. E.	23, Wincham Grove, Clapham Common, S.W.
1920.		SAMSON, Major A. O.	P. O. Box 1628, Alexandria, Egypt.
1921.		SATTERFORD, H. T. ...	6, Ethelbert Square, Westgate-on-Sea, Kent.
1918.		SAUNDERS, S. C.	"Aldbury," 7, Chandos Road, Harrow.
1919.		SAUNDERS, W. H.	214, Newhampton Road West, Wolverhampton.
† 1920.		SAVAGE, G. H.	Mapperley House, Hale End, Woodford Green, Essex.
1915.		SAVAGE, W. P.	Air Ministry, Kingsway, W.C.2.
1918.		SAXELBY, F. A.	London Road, Stretton-on-Dunsmore, Rugby.
1919.		SCHOLES, G. E.	Engineering Laboratories, Liverpool University.

Date of Election.			
1911.	G.	Scholte, Lieut.-Col. L. F.	Shaw & Kilburn, Ltd., 114, Wardour Street, W.1.
1914.	A.M.		
1916.		Scott, J. Morse	13, Baronsmead Road, Barnes, S.W.13.
1918.		Scott, S. B.	L.C.C. Tramways, 19, Belvedere Road, S.E.
1909.		Scott, W. C. H.	46, Broomwood Road, Wandsworth Road, S.W.18.
1918.		Sellars, C. A.	C. A. Sellars, Ltd., Harrogate Road, Leeds.
1919.		Sennitt, F. J.	67, Mersea Road, Colchester.
1921.		Serve, A. J.	52, Waterloo Avenue, Leiston, Suffolk.
1907.		Sharp, R. H.	34, Great St. Helens, E.C.
1915.		Shave, R.	c/o Dalgety & Co., Ltd., William Street, Perth, W. Australia.
1914.		Shaw, F.	Armstrong-Siddeley Motors, Ltd., Church Street, Lisson Grove, N.W.8.
1919.		Shaw, G. Livingstone	
1920.		Shaw, R. S.	Genl. Accident, Fire & Life Assce. Corp., Ltd., General Buildings, Perth.
1920.		Shaylor, S. E.	6, Rockdove Gardens, Tollcross, Glasgow.
1920.		Sherratt, L.	211, Pershore Road, Edgbaston, Birmingham.
1908.		Sherrin, G. C.	St. Ethelburga's, Ingatestone, Essex.
1910.		Shire, L.G.Wyndham.	547, Bearwood Road, Smethwick, Birmingham.
1921.		Shoebottom, W. H. ..	Imber Court Engineering Works, Thames Ditton, Surrey.
1920.		Shurrock, W. B.	220, St. Saviours Road, Saltley, Birmingham.
1915.		Simonelli, L.	Palace Garage, Mysore, India.
1913.	G.	Simpson, C. M.	30, Street 1, Radford Estate, Coventry.
1921.	A.M.		
1909.	G.	Sinstadt, J.	3, Rue de Valenciennes, Paris (10e).
1914.	A.M.		
1910.	G.	Smallwood, H. F. ...	29, Vicarage Road, East Sheen, Surrey.
1918.	A.M.		
1908.		Smith, C.	Renfrew House, Church Street, Willingden, Eastbourne.
1907.		Smith, Edgar	73, Northgate, Halifax.
1918.		Smith, Lt. E. Seton..	c/o Cox & Co., Bankers, Charing Cross, W.C.
1920.		Smith, F. G.	44, Upper East Smithfield, E.
1917.		Smith, G. A.	30A, Sincil Street, Lincoln.
1912.	G.	Smith, S.	73, Northgate, Halifax.
1918.	A.M.		
1920.		Smitheman, A. H.	17, Clifton Mount, Rotherham.
1920.		Snarry, J. W.	9, Farcroft Avenue, Handsworth, Birmingham.
1909.	G.	Soanes, E. G.	1, Seagrave Road, London Road, Coventry.
1915.	A.M.		
1909.	G.	Speck, T. R.	23, Wordsworth Road, Small Heath, Birmingham.
1920.	A.M.		
1912.	A.	Spence, W. L.	44, Ormidale Terrace, Murrayfield, Edinburgh.
1917.	A.M.		
1921.		Spencer, Orville R...	Beechcroft, 48, Russell Road, Hall Green, Birmingham.
1914.		Spivey, G.	2, Belmont Villas, The Nothe, Weymouth.

Date of Election.			
1919.		SPLITTER, B.	Claw Engineering Works, 16, Woodberry Down, N.4.
1907.		SPONG, J. L.	
1920.		SPRUCE, E. U.	52, Kinnerley Street, Walsall, Staffs.
1911.		STAFFORD, C. E.	6, Sylvan Avenue, Central Avenue, Levenshulme, Manchester.
1908.		STAFFORD, E.	16, Garrick Avenue, Golders Green Road, Golders Green, N.W.
1909.		STAGG, W. S.	Fernleigh, Weston Park, Thames Ditton.
1917.		STAITE, E.	Chesterfield, Ockbrook, Derby.
1919.		STANLEY, G. E.	The Triumph Cycle Co., Ltd., Coventry.
1921.		STEPHENSON, C. H. ...	Bayshill Cottage, Elstree, Herts.
1918.		STEUART, A.	16, Clarence Street, Edinburgh.
1918.		STEVENS, G.	A. J. Stevens & Co., Graiseley House, Penn Road, Wolverhampton.
1918.		STEVENS, HY.	A. J. Stevens & Co., Graiseley House, Penn Road, Wolverhampton.
1918.		STEVENS, J.	Oaklawn, Oaklands Road, Wolverhampton.
1920.		STEVENSON, D.	"Thames View," Flackwell Heath, nr. High Wycombe, Bucks.
1912.		STEWART, C. J.	Room 464, Central House, Kingsway, W.C.2.
1914.		STOKES, E.	4, Vaughan Avenue, Stamford Brook, W.6.
1909.		STRACHAN, J. M.	Holland Gate, High Street, Kensington, W.8.
1913.		STURCH, H. A.	Hazeldene, Kenilworth Road, Berkswell, Coventry.
1910.		SUMMERS, L. F.	Bradfield Tractor Co., Bradfield, Essex.
1920.		SUMNER, F.	Albion Motor Car Co., Scotstoun, Glasgow.
1912.	G.	SURMAN, E. J.	c/o Jardine, Mathesson & Co., Shanghai.
1916.	A.M.		
1920.		SWEETING, C. P.	c/o H. Stewart & Co., Central Bldgs., Nuneaton.
1918.		SWIFT, T. R.	8, Milton Road, Belvedere, Kent.
1917.		SYMONDS, Major J. C.	58, Portland Place, W.
1914.		TAIT, J. J.	227, Onslow Drive, Dennistoun, Glasgow.
1916.		TAMPLIN, A. T.	"Green World," Ferndown, Dorset.
1920.		TANKARD, HY.	18, Wellington Avenue, S. Tottenham, N.15.
* 1909.	G.	TAPP, E. T.	7, Manville Road, Tooting Common, S.W.17.
1919.	A.M.		
1920.		TATE, W. H.	7, Westbourne Grove, Barker's Lane, A. on M., Manchester.
1918.		TAYLOR, B. G.	"Malsall," Ferrier Street, Rockdale, N.S.W.
1919.		TAYLOR, GEO.	Middlegate, Penrith.
1908.		TAYLOR, H. J.	118, Laburnum Grove, North End, Portsmouth.
1911.	G.	TEAGE, Capt. H. D. ...	The Motor Cycle, Hertford Street, Coventry.
1917.	A.M.		
1918.		THOMAS, A.	25, Cleveland Street, Wolverhampton.
1920.		THOMAS, A. P.	409, Reddings Lane, Hall Green, Birmingham.

Date of Election.			
1920.		THOMAS, F. A.	Auto Engineering and Car Co., Fisher Street, Swansea.
1912.		THOMAS, R. G.	The Kingfishers, Wraysbury, Bucks.
1918.		THOMAS, W. J.	13, Edgbaston Road, Smethwick, Birmingham.
1920.		THOMSON, A. F. W. ..	United Glass Bottle Manufacturers (Charlton), Ltd., Anchor and Hope Lane, Charlton, S.E.7.
1914.		THOMSON, R. ST. CLAIR.	7, East Bank, Stamford Hill, N. 16.
1911.	G.	THONGER, Lt. J. R. ...	31, Oakdale Road, Streatham, S.W.
1918.	A.M.		
1913.	G.	THORPE, Lt. T. H.	2, Queen's Road, Tunbridge Wells.
1914.	A.M.		
* 1917.	G.	TILT, A.	16, Westminster Road, Coventry.
1920.	A.M.		
1916.		TIMBERLAKE, H. H. ..	Automobile Engineer, Wigan.
1920.		TITTERTON, W.	Fleetwood Motors, Ltd., 21, Bold Street, Fleetwood.
1907.		TOBY, A. I.	Ardmont House, Abbey Drive, Jordanhill, Glasgow.
*g 1913.	G.	TOWNS, H. L.	787, Scotswood Road, Newcastle-on-Tyne.
1919.	A.M.		
1907.	G.	TRAVERS, J. L.	c/o Minister of War & Marine, Santiago, Chile.
1912.	A.M.		
1917.		TROUGHTON, S. H.	39, Barras Lane, Coventry.
1911.		TURNER, A. B.	32, Beverstone Road, Thornton Heath, Surrey.
1911.		TURNER, E. A.	163, Kingston Hill, Surrey.
1917.		TURNER, H.	2, Seagrave Road, London Road, Coventry.
1918.		TURTLE, CHAS.	Elmwood, Prince's Road, Heaton Moor, Stockport.
1918.		TWELLS, J.	Galloway Engineering Co., Kirkcudbright.
1919.		VINCENT, E. T.	5, Arundel Mansions, Fulham Road, S.W.6.
1909.	G.	WALKER, F. C.	31, Marlborough Road, Coventry.
1919.	A.M.		
1917.		WALKER, J. S. A.	Walker Bros., Ltd., Pagefield Ironworks, Wigan.
1920.		WALLING, S.	6, Priory Gardens, Barnes, S.W.13.
1916.		WARD, C. W.	31, Rosebank Avenue, Sudbury Hill, Harrow.
1916.		WARING-BROWN, R. ..	22, Queen Victoria Road, Coventry.
1917.		WARNER, Capt. L. W.	3—6, Clement's Inn, W.C.2.
†*cdg* 1915.	G.	WARREN, H. W. H. ..	"Lynn," St. Andrew's Road, Earlsdon, Coventry.
1918.	A.M.		
1910.	G.	WATERLOW, C. B.	31, Rue Charles Laffitte, Nevilly-sur-Seine, Paris.
1914.	A.M.		
1918.		WATT, CHAS.	
1920.		WEALLEANS, H.	41, Portland Street, Lincoln.
1915.		WEBB, GEO.	Dixtow Road, Monmouth.
1920.		WELLINGHAM, H. J., M.C.	"Southacre," 1, Percy Road, Winchmore Hill, N.21.
1916.		WELLS, R. G.	Hibbert Lane, Marple, Cheshire.
1920.		WHITAKER, H.	117, Woodgrove Road, Burnley.

Date of Election.			
1920.		WHITE, PERCIVAL	993, Charles River Road, Cambridge, Mass., U.S.A.
* 1908.	G.	WHITE, T. E. B.	35, Rowfant Road, S.W.17.
1919.	A.M.		
1919.		WHITELEGG, J. F.	The National Gas Engine Co., Ltd., Ashton-under-Lyne.
1917.		WHYTE, R.	Cubitts Engineering Co., Ltd., Southern Works, Aylesbury.
* 1908.	G.	WICKHAM, W. E.	50, Thurlby Road, Wembley, Middlesex.
1915.	A.M.		
1916.		WILKINS, Capt. G. B.	c/o Bank of England, Western Branch, Burlington Gardens, W.1.
1919.		WILKINS, Lieut. H. J.	24, Rugby Road, Brighton.
1919.		WILLIAMS, H.	67, Station Road, Handsworth, Birmingham.
1918.		WILLIAMS, H. L.	Wyncot, Stocton Road, Guildford, Surrey.
1919.		WILLIAMS, J.	40, Apollo Bunder Road, Bombay.
1913.		WILLIAMS, J. LLOYD ..	2, Wood Lane, Headingley, Leeds.
* 1913.	G.	WILLIAMS, P. A.	The Forge, Bassalog, nr. Newport, Mon.
1917.	A.M.		
1919.		WILLIS, H.	37, Highfield Avenue, Golders Green, N.W.4.
* 1910.	G.	WILSON, A. D.	"Holmelea," 19, Richmond Avenue,
1913.	A.M.		Fartown, Huddersfield.
1915.		WILSON, W. B.	Ryner Wilson, Ltd., Western Road, Merton Abbey, S.W.19.
1908.		WILSON, W. L.	North View, St. John's Road, Boxmoor, Herts.
1912.		WILTON, H. L.	Bisley Lodge, Boyne Hill, Maidenhead.
1918.		WITARD, S. L.	32, Offerton Lane, Stockport.
1919.		WOOD, F.	6, Cecil Road, Gravelly Hill, N., Birmingham.
1921.		WOOD, H. O.	34, Oak Lane, Bradford.
1917.		WOOD, Thos.	69, High Road, South Tottenham, N.15.
1911.		WOOD, W. J.	36, Milestone Road, Upper Norwood, S.E.
1913.		WOODALL, J. C.	"Kinrara," West Street, Reigate, Surrey.
1917.		WOODCOCK, Major R. R.	c/o Sir C. R. McGrigor, Bart., & Co., 39, Panton Street, S.W.1.
1919.		WOODFORD, H.	94, Crewe Street, Derby.
1907.	G.	WOOLLEN, Harry	118, Maybury Road, Woking.
1910.	A.M.		
1914.		WORKMAN, W. H. ...	Associated Equipments Co., Ltd., Blackhorse Lane, Walthamstow, N.E.
1908.		WRIGHT, T. G.	
1919.		WYKE, C. J.	10, Corporation Street, Manchester.
1911.		WYSE, M. N.	Beaumont Road, Karachi, India.
1917.		YOUNG, S. J.	182, Gladstone Street, Bradford.
1916.		YOUNG, W. F. C.	Young's Motor Repair and Transport Base, Park Street, Arbroath.
1919.		ZANNOS, J. C.	36, Rue Homère, Athens.
1919.		ZWALLY, E. L.	Automotive Corporation, 5864, Baum Blvd., Pittsburgh, Pa., U.S.A.

3. *Graduates.*

Date of Election.		
1921.	ABBOTT, E. D.	"Highfield," Victoria Road, Stechford, Birmingham.
1918.	ALCOCK, J. F.	
1919.	ALCOCK, R.	
1909.	ALLARD, F. W.	Broad Lane, near Coventry.
1919.	§ALLEN, J. R.	Linden Lodge, Runcorn.
1914.	ANDERSON, D.	P.O. Box 899, Johannesburg, S. Africa.
1920.	ARMSTRONG, J. W.	10, Mount Street, Lincoln.
1920.	ARMSTRONG, K. P. F.	11, Levington Street, W.9.
1920.	ATTREE, F.	21B, Park Square, Luton.
1919.	§ATTREE, J. R.	4, Chisholm Road, Stoke Newington, N.16.
1920.	AVELINE, R. N.	The Firs, 4, Chester Street, Coventry.
1908.	BAIRD, R. G.	G. F. Carter, Ltd., Alfred Street Mills, Nottingham.
1920.	BALDWIN, E.	Lyndhurst, Coundon Road, Coventry.
1920.	BALLARD, C.	23, Lincoln Street, Coventry.
1920.	BARAGWANATH, E. G.	Carthew, Croxteth Grove, Liverpool, S.
1915.	BARKER, H. G.	Lynwood, Brunswick Road, Coventry.
* 1918.	BARLOW, B. W.	82, Hockley Hill, Birmingham.
1921.	BARLOW, F. H.	Broadway, Accrington.
1920.	BARRY, R. C.	165, Gulson Road, Coventry.
1917.	BARTLE, H.	45, William Cook Road, Ward End, Birmingham.
1920.	§BARTLETT, J. C.	92, Church Road, Moseley, Birmingham.
1920.	BASS, E. L.	207, Lavender Hill, S.W. 10.
1921.	§BASSETT, C.	"Hopelands," Ellis Road, Crowthorne, Berks.
1920.	§BENBOW, H.	7, Ralph Road, Saltley, Birmingham.
* 1919.	BENBOW, W. E.	"Ombersley," Carrington Road, Dartford, Kent.
1920.	BENNETT, A. B.	9, Page Street, Swansea.
1921.	BENTLEY, J.	10, Dewhirst Street, Greenside, Cleckheaton.
1918.	BERRY, J. F.	"Honnington," 34, Adria Road, Sparkhill, Birmingham.
1921.	BERRY, J. H.	14, Grosvenor Drive, New Brighton, Cheshire.
1920.	BETTERIDGE, C. D.	c/o Chief Engineer's Dept., Daimler Co., Ltd., Coventry.
1919.	BICKELL, S. F.	47, Caversham Road, N.W.5.
1920.	§BIRMINGHAM, A. J.	2, Carnarvon Road, Leyton, E.10.
1921.	BODEN, H.	25, Elmfield Avenue, Crouch End, N.8.
* 1911.	BOLLACK, L.	45, Rue Copernic, Paris.
1921.	BOLTON, R.	47, Powell Street, Derby.
1909.	BOLTON, W.	"The Shack," Jobs Lane, Tilehill, near Coventry.
1921.	§BOOTH, J. F.	80, Upper Walthamstow Rd., Walthamstow, E.17.
1920.	BORLAND, D. D.	21, Coombe Road, Croydon, Surrey.
1920.	§BOWDEN, S. H.	Ferndale, Newbridge Crescent, Wolverhampton.
1920.	BOYD, J. M.	c/o Mrs. McAnsh, 14, Stanley Street, Glasgow.

§ Probationary Graduate.

Date of Election.		
1917.	BRADLEY, T. G.	Woodbine Cottage, Hampton-in-Arden, Warwickshire.
1921.	BRAIMBRIDGE, H.	7, Marine Park Mansions, New Brighton.
1920.	§BRENT, L.	40B, Golders Way, Golders Green, N.W.4.
1920.	BRIGGS, H.	25, Severn Street, Leicester.
1921.	§BRIGHT, J. E.	16, Judge's Street, Loughborough.
1913.	§BROCKLEHURST, J. W. ...	11, Milverton Road, Erdington, Birmingham.
1920.	BROOKE, G. V.	15, Melville Road, Coventry.
1920.	BROWN, A.	425, Richmond Road, E. Twickenham, Middlesex.
* 1918.	BROWN, C. G.	36, Portland Street, Leamington Spa.
1921.	§BROWN, L. W.	"The Clock House," Margate.
1920.	BROWNE, H. E.	Royston, Kingsfield Road, Watford.
1921.	BURDETT, G. W. S.	7, Albert Place, Peterborough.
1918.	BURFORD, E. H.	167, Walm Lane, Cricklewood, N.W.2.
* 1920.	BURKETT, G. T. W.	Parkfield, Dudley Park Road, Acocks Green, Birmingham.
1920.	BURROWS, C. H. C.	The Vicarage, St. Nicholas Street, Coventry.
1918.	BUSWELL, F. J.	20, Milcote Road, Bearwood, Birmingham.
1915.	BUTLER, F. B.	164, Albany Road, Coventry.
1911.	BUTLER, L. G. F.	7, Kent Road, Bishopston, Bristol.
1921.	CAPLIN, E. S.	18. Howard Road, Walthamstow, E.17.
1909.	CARTWRIGHT, D.	c/o Straits Motor Garage, Orchard Road, Singapore, S.S.
* 1918.	CHAPPELL, C. A.	14, The Avenue, Brondesbury, N.W.
* 1919.	CHATTERTON, E.	11B, Polygon Buildings, St. Pancras, N.W.1.
1920.	§CLARE, E., Jr.	Fernside, Seymour Road, Hampton Wick.
1919.	CLARK, F. I.	213, Edward Street, Nuneaton.
1920.	CLARKE, F. G. E.	"The Elms," Marlbrook, Bromsgrove, Worcs.
1920.	CLEVERLY, H. L.	Bramcote, Hampton-in-Arden, Warwickshire.
1920.	§COLE, P. D.	28, College Road, Alum Rock, Birmingham.
1919.	COLLINS, A. E. L.	Kingston, Radford, Coventry.
1920.	§COOPER, L. A.	Y.M.C.A. Association Buildings, Dale End, Birmingham.
1920.	CORBETT, Geo.	54, Kyotts Lake Road, Sparkbrook, Birmingham.
1920.	CORNFORD, C. C.	Austin Engineering College, Bromsgrove, Worcs.
1915.	§COX, A.	
1913.	COZENS, P. H.	15, South Avenue, Rochester.
1910.	CROSBY, A. D.	St. John's, Woking Road, Guildford.
* 1917.	CROSS, F. P.	48, Peagrave Road, Coventry.
1920.	§CUMMING, H. B.	30, Blythswood Drive, Glasgow, N.
1920.	CURE, H.	41, Farman Road, Earlsdon, Coventry.
1920.	CURTIS, E.	103, St. Peter's Road, Earley, Reading.

§ Probationary Graduate.

Date of Election.		
1911.	DANIEL, J. P.	Bryn Dovey, 37, Cranbrook Park, Ilford.
* 1908.	DAVIES, Capt. O.	65, Frances Road, Windsor.
1920.	§DAVIES, W. S.	c/o Mrs. Davies, "Belle Crest," Upper Nyack, Hudson River, New York, U.S.A.
* 1919.	DAWTREY, L. H.	101, Hastings Road, Stoke, Coventry.
1920.	DAY, G. H.	74, Spencer Avenue, Coventry.
1921.	DAY, R. W.	"The Holt," Forest Road, Loughborough.
1920.	§DEAKIN, W. A.	Colesdale, Albert Road, Stechford, Birmingham.
1921.	§DEEMAN, F.	Noyapara, Itakola P.O., Assam-Bengal Railway, South Sylhet, India.
1914.	§DE FRAINE, A. F.	44, Bowron's Avenue, Wembley.
1917.	§DERWENT, A. L.	1, Ansdell Avenue, Sandy Lane, Chorlton-cum-Hardy, Manchester.
1920.	§DIXON, G. H.	43, Ranelagh Road, Blakenhall, Wolverhampton.
1914.	DOBSON, F. E.	26, Little Green Lanes, Erdington, Birmingham.
1920.	DORMER, C. M.	40, Amberley Road, Bush Hill Park, Enfield, N.
1921.	DOUTHWAITE, S. M.	Technical College, Loughborough.
1921.	DUNNETT, D.	30, Broomhill Road, Aberdeen.
1915.	DYKE, C. H.	Selby, Robinhood Lane, Hall Green, Birmingham.
1920.	§EASON, J. E.	108, Grove Road, Walthamstow, E.17.
1919.	EDEN-EADON, Lieut. A. T.	Ruthwell House, Gainsborough.
1921.	EDWARDS, C. A.	15, Bloomfield Road, Moseley, Birmingham.
1920.	§EDWARDS, W. E.	19, Golden Hillock Road, Small Heath, Birmingham.
1920.	§EKERS, S. E.	80, Edmund Road, Saltley, Birmingham.
1918.	§ELLIOTT, E. T.	c/o Egertons, Northgate Street, Ipswich.
1917.	ELLIS, J. G. E.	
1920.	ELSBY, L. F.	"Ashleigh," Bebington, Cheshire.
1920.	EMERSON, C. F.	58, Holdenhurst Avenue, N. Finchley, N. 12.
1918.	§ENGLAND, B.	Belle Isle, Wakefield.
1920.	§EVANS, T. H.	Bryn-y-Ffynon, Leeswood, nr. Mold.
* 1920.	§EVERY, E. L.	241, Selly Oak Road, King's Norton, near Birmingham.
1910.	FAULKNER, H.	Fieldgate Close, Kenilworth.
1914.	FAULKNER, Lt. W. D.	65, Hunton Road, Erdington, Birmingham.
1918.	FEARNLEY, R. A.	182, Psalter Lane, Sheffield.
1920.	§FENN, F.	St. Elmo, 27, Chantrey Road, Moseley, Birmingham.
1913.	FISHER, C. J.	44, Churchill Avenue, Coventry.
1910.	FISHER, T. MORRIS	The Grange, Kislingbury, Northampton.
1915.	FLATMAN, L.	Villa Constance, Wimereux, France.
1917.	FLEMING, E. W.	55, Herne Hill, S.E. 24.

§ Probationary Graduate.

Date of Election.		
* 1919.	§Florance, T. R.	Oakfield, Warwick Road, Olton, Warwickshire.
* 1920.	§Ford, V. A.	29, Edmund Road, Saltley, Birmingham.
1919.	French, E. J.	55, Park Road, Coventry.
1920.	§Fryer, H. A. B.	
1921.	Fulton, S. M.	15, Bolton Road, Chiswick, W.4.
1920.	Gadsden, J. E.	c/o "Hillington," 12, Ravenswood Road, Croydon.
1921.	Gammon, D. L.	"Hopefield," St. Catherine's, Guildford.
1921.	Gavin, W. L.	29, Town Lane, Idle, Bradford.
1919.	§Gibbard, Pt. T.	Longai T.E., Chaudkhria, Sylhet, India.
1911.	Gidden, Lce.-Corpl. L. Sapsford.	53, Moresby Road, E.5.
1921.	§Gigli, R.	155, Archway Road, Highgate, N.6.
1921.	§Gilliat, E. V.	"Lindum," Claremont Gardens, Monkseaton, Northumberland.
1921.	Goffey, P. H.	Technical College, Loughborough.
1917.	§Golder, H. C.	Homeleigh, Rectory Road, South Farnborough.
* 1920.	Gosling, C. A.	60, Greenhill Road, Moseley, Birmingham.
1920.	§Gould, H.	Austin Engineering College, Bromsgrove, Worcs.
1917.	Gould, H. G.	39, Kensington Road, Earlsdon.
1914.	Grace, V. R. P.	7, Birkbeck Mansions, Birkbeck Road, Hornsey, N.8.
1914.	Graham, C. E.	70, Elm Grove Road, Barnes, S.W.
1921.	§Graham, G. H.	Culverden Garage, 3a, Standen Street, Tunbridge Wells.
1920.	§Gray, J. R.	48, Woodstock Road, Chiswick, W.4.
1921.	Greaves, A. I.	Central Garage, Hill Street, Grahamstown, South Africa.
1920.	§Greenham, C.	65, Hatherley Road, Walthamstow, E.17.
1914.	Gregory, R.	Merridale, Cranmore Lane, Aldershot.
1921.	§Griggs, J. W.	7, Parkhurst Road, Walthamstow, E.17.
* 1919.	Hacking, J. T.	School House, Kenilworth.
1921.	Hackney, W.	Technical College, Loughborough.
1914.	Halliwell, H. A.	Hill House Residential Hotel, Handsworth Wood, Birmingham.
1921.	§Hallpike, A. W.	58, Well Street, South Hackney, E.9.
1918.	§Halsall, C. R.	80, North Side, Wandsworth Common, S.W.
1917.	Hamilton, J. R.	179, Humber Avenue, Coventry.
1921.	Hands, W. E.	25, Gedling Grove, Nottingham.
1920.	Hanman, J. L.	c/o Centrifugal Castings, Ltd., Kirkstyle Road, Kilmarnock.
1921.	Hardy, G. Dudley	"Forest View," Normanton-on-Soar, nr. Loughborough.
1920.	Hardy, P. H. C.	2, Douglas House, Maida Hill West, W.2.
1919.	Harries, E. C.	55, Park Road, Coventry.
1920.	§Harris, L. B.	9, Princes Square, Bayswater, W.2.
1920.	Hawkins, A. H.	98, Ellesmere Road, Alum Rock, Birmingham.

§ Probationary Graduate.

Date of Election.		
1917.	HAY, J. M.	
1920.	HAYNES, L. A.	Sunnydene, Montem Lane, Slough, Bucks.
* 1919.	HAYWARD, A. J.	137, Richmond Street, Coventry.
1913.	HAZELL, J. A.	Technical Staff Mess, Royal Aircraft Establishment, South Farnborough, Hants.
* 1920.	HEARD, J. F.	83, Merridale Road, Wolverhampton.
1921.	HENN, S. T.	10, Eckstein Road, Clapham Junction, S.W.11.
1921.	HERBERT, E. C.	10, The Quadrant, Coventry.
1911.	§HIRSCHFELD, J. A.	3, Heathview Road, Thornton Heath, Surrey.
1920.	HISCOX, H. W.	7, Cecil Avenue, Wembley, Middlesex.
1919.	§HOLMES, H. R.	77, Spencer Avenue, Earlsdon, Coventry.
* 1909.	HOAR, H. T.	"Devona," Canterbury Road, South Farnborough, Hants.
1921.	§HOWLAND, I.	Pension, 2, rue Bernard Dessuad, Génève, Suisse.
1921.	HUGHES, D. A.	Crofton, Oxton, Cheshire.
* 1913.	§HUGHES, V.	Mars Hill Hotel, Maywood, Ind., U.S.A.
1919.	HUMBLE, A. W.	81, Northfield Road, Stoke, Coventry.
1909.	JACKSON, B. W.	Bridge House, Pilton, Barnstaple, N. Devon.
* 1915.	JACKSON, H. B.	Elton, near Peterborough, Hunts.
1920.	JAEGER, J. C.	23, College Road, Alum Rock, Birmingham.
1921.	JAMES, R. V.	Wesley House, Stafford.
1920.	JAY, R.	3, Queensbury Place, The Green, Richmond.
1919.	JOHNSON, E. H.	The Laurels, Longford, Coventry.
1920.	§JONES, D. W. C.	130, Northfield Road, King's Norton, Birmingham.
1920.	JONES, F. A.	"Mansfield," Binley Road, Coventry.
1911.	JONES, H. DREW	Laurel Villa, Queen's Road, Peckham, S.E.15.
1919.	§JONES, H. F.	1, Leicester Terrace, Northampton.
1920.	JONES, R. A. WILSON	55, Berkeley Road, Earlsdon, Coventry.
1920.	KEMPSTER, C. H.	9, Cromwell Road, Montpelier, Bristol.
1916.	KENT, R.	2, Kelross Road, Highbury, N.
1920.	KIDD, R. H.	Marlborough House, Newbury.
1916.	§KING, C. S.	19, Sutton Court, Chiswick, W.4.
1920.	§KING, D. W.	4, Lonsdale Road, Wolverhampton.
1920.	§KNEE, E. B.	Little Cot, Coombe-in-Teignhead, near Teignmouth, S. Devon.
1921.	§KNIGHT, F. W.	20, Curzon Street, Loughborough, Leicestershire.
1920.	LAMMING, J. G.	c/o Mr. Jones, Turls Hill Road, Sedgley, Staffs.
1920.	LANE, A. S.	14, Mill Street, Ludlow.

§ Probationary Graduate.

Date of Election.		
1917.	§Leak, V. B.	9, Springfield Road, Brighton.
1918.	Leckie, R. V.	
1921.	§Le Maitre, H. E.	Engineering College, Loughborough.
1920.	Lidsey, A. J.	c/o Lonsdale House, Charnwood Street, Derby.
1920.	Lines, D. A.	5, Station Road, Cookham, Berks.
1920.	Linford, (Miss) E. J.	42, Beacon Road, Loughborough.
1920.	Lissaman, G. S.	School House, Slip End, Luton, Beds.
1908.	Lister, W. W.	67, Herne Hill, S.E.24.
1921.	§Littlewood, L. A.	"Ormesby," Stanley Road, Hendon, N.W.9.
1920.	Lovesey, A. C.	Sarum Lodge, 13, Clyde Park, Redland, Bristol.
1909.	§Lucas, Lt.-Col. F. W.	26, Stanhope Gardens, Queen's Gate, S.W.7.
1919.	McCarthy, H. C.	Jesus College, Cambridge.
1920.	§McCormack, D.	172, Northfield Road, King's Norton, Birmingham.
1919.	McDowall, L.	635, Smithdown Road, Wavertree, Liverpool.
1921.	§Macdonald, A. J. W.	Technical College, Loughborough.
1915.	Macklin, D. J.	54, Maldon Road, Acton, W.3.
1919.	Magson, A.	108, Gulson Road, Coventry.
1920.	§Mahomed, J.	72, Longridge Road, Earls Court, S.W.5.
1920.	§Male, C. D.	Emscote, Belwell Lane, Four Oaks, Warwickshire.
1920.	§Maltby, W. L.	"Kinver," Starley Road, Coventry.
1919.	§Mansell, B. W.	273, Harnall Lane E., Coventry.
* 1913.	Marden, G. E.	14, Wentworth Place, Newcastle-on-Tyne.
1917.	Marks, A. S. C.	158, Upper Bridge Road, Chelmsford.
1914.	Marsh, W. W.	Redmarley, South Farnborough, Hants.
* 1920.	Marshall, A. O.	258, Whippendell Road, Watford.
1909.	Marshall, V. M. C.	
* 1920.	§Martineau, G. M.	10, Queen Victoria Road, Coventry.
1919.	Masser, A. H.	33, High Street, Watford, Herts.
* 1909.	Maxwell, R. M. S.	Boldrewood, Guildford Road, S. Farnborough.
1920.	§Meads, G.	"Athlone," Stechford, Birmingham.
1920.	§Miller, K. B.	Temple Bar, Willenhall, Staffs.
1919.	Mitchell, A. W.	17, Temple Street, St. George's Road, S.E.11.
1921.	§Molyneux, A. L.	79, Holyhead Road, Coventry.
1917.	Morgan, P. D.	"Cambria," Devonshire Road, Hornchurch, Essex.
* 1908.	Morley, E. G.	12, Castle Street, Hereford.
1915.	§Morris, C. V.	Coldbath, King's Heath, Birmingham.
1920.	Mountfort, R.	11, Coundon Street, Coventry.
1920.	§Muir, W. G.	The Cairns, Ralston, Paisley.
1920.	Munro, D. W.	23, Gladstone Road, Sparkbrook, Birmingham.
* 1912.	Naish, E. P.	21, Northdown Avenue, Cliftonville, Margate.
1917.	Napier, R. C.	c/o Mrs. Nicholson, 402, Byres Road, Glasgow.

§ Probationary Graduate.

Date of Election.		
* 1917.	NEWEY, J.	Ivy Cottage, St. Nicholas Street, Coventry.
1920.	NORMAN, H. N.	Trinity College, Cambridge.
1920.	OAKES, H. I.	40, Queen's Drive, Walton, Liverpool.
1921.	OLIVER, E. A. C.	Tudor House, Loughborough.
1919.	PARISOT, E. LA V.	3, Beverley Road, Turnham Green, Chiswick, W.4.
1909.	PARKINSON, B.	"Glen Nevis," 45, Marshall Avenue, Bridlington.
1921.	PARRITT, L. J.	14, Stanhope Gardens, Highgate, N.6.
* 1916.	§PARSONS, A. F.	Uplands, Keynsham, near Bristol.
1920.	PASSMORE, C. W.	Clent House, 170, Lea Road, Wolverhampton.
1921.	§PEACH, W. G.	West Street, Alford, Lincs.
1918.	§PEACOCK, C. D.	155, Franciscan Road, Tooting Common, S.W.17.
1915.	§PEARCE, R. G.	39A, St. Patrick's Road, Coventry.
1920.	§PEARSON, J. L.	The Homestead, Four Oaks, Sutton Coldfield.
1920.	PELLOW, L.	Austin Engineering College, Bromsgrove, Worcs.
1920.	§PENWARDEN, F. A.	"Ivythorpe," Eachway, Rubery.
1921.	PHILLIPS, E.	32, Dairy House Road, Derby.
1920.	§PHILPOT, S. R.	29, Newcombe Road, Coventry.
1920.	§PICKERING, O. C.	Mansfield Villa, Binley Road, Coventry.
1920.	PITTS, G. Y.	54, Huskisson Street, Liverpool.
1920.	PLATT-HIGGINS, E. B.	5, Regent Street, Coventry.
1916.	POBJOY, D. R.	14, Hurlingham Road, Ashley Hill, Bristol.
1920.	POTTER, E. K.	39, Evelyn Road, E.17.
1921.	POTTER, W. G.	80, Edmund Road, Saltley, Birmingham.
* 1920.	PRESTAGE, Jr., W. H.	20, Phipson Road, Moseley, Birmingham.
1921.	PRICE, R. S.	c/o A. Owen, Lang Street, Dursley, Glos.
1918.	§PULLINGER, C. P.	Galloway Engineering Co., Tongland, Kirkcudbright.
1920.	RAY, M.	16, Parsi Bagan, Amherst St. P.O., Calcutta, India.
1919.	§REYNOLDS, JAS.	12, Ellys Road, Coventry.
1909.	REYNOLDS, R. J.	13, Westminster Road, Coventry.
1917.	§RHYS, W. D.	Adare Street, Bridgend, Glam.
1920.	RICHARDSON, H. F.	19, Highfield Road, Saltley, Birmingham.
1909.	RICHARDSON, J. A.	
1920.	§RIDING, C. H.	Fairholme, Royal Road, Sutton Coldfield.
1919.	ROBOTHAM, C.	7, Cannon Hill Road, Birmingham.
1919.	ROCHFORD, L. H.	63, Loveday Road, Ealing, W.13.
1916.	§ROFE, H.	Westcote, Dumpton Park Drive, Ramsgate.
1920.	§ROGERS, E. W.	Eversley, St. Agnes Road, Moseley, Birmingham.
* 1917.	§ROMYN, A. W.	Leopolds Private Hotel, 185—7, High Street, Oxford Road, Rusholme, Manchester.

§ Probationary Graduate.

Date of Election.		
1921.	§Ross, J. W. E.	"Silverdale," 132, Babington Road, Streatham, S.W.16.
1914.	Royle, F.	347, Abbey Hay Lane, Gorton, nr. Manchester.
1908.	Rush, H. H.	19, Melville Road, Coventry.
1918.	Rushbrook, S. R.	c/o Albion Motor Car Co., Ltd., Scotstoun, Glasgow.
1920.	§Russell, G.	New Buildings, Jeffcock Road, Wolverhampton.
1920.	§Salmon, E. N.	Austin Engineering College, Bromsgrove, Worcs.
1920.	Sankey, Capt. H. B.	Finchfield House, Wolverhampton.
1909.	Scott, T. L.	Gadsden, Hayes, Kent.
1920.	Seale, R.	57, Northumberland Road, Coventry.
1915.	§Seaton, D. G.	
1921.	Senior, F.	869, Thornton Road, Thornton, Bradford.
1919.	Shellard, S. H.	72, Stanley Road, Earlsdon, Coventry.
1921.	Shepherd, G. W.	Stainlands, Ferrybridge, Yorks.
1920.	§Shutt, W. K.	Austin Engineering College, Bromsgrove, Worcs.
1917.	§Simpson, T. G.	95, Clarendon Road, Putney.
1920.	Skinner, T. V.	51, Benton Road, Sparkhill, Birmingham.
1917.	Smith, G. H.	Halifax Motor Co., Ltd., 73, Northgate, Halifax.
1914.	Smith, H.	497, Belchers Lane, Little Bromwich, Birmingham.
1916.	Smith, J.	69, Warwick Road, Sparkhill, Birmingham.
1909.	Smith, P. W.	63, Sandbourne Road, Alum Rock, Birmingham.
1921.	§Snow, W. E.	Loughborough College Union, Technical College, Loughborough.
1920.	Southern, T. E.	37, Ninian Road, Roath Park, Cardiff.
1921.	Spackman, N.	"Craigside," Bath Road, Worcester.
1920.	§Sparkes, A. E.	74, Aubrey Road, Small Heath, Birmingham.
1919.	§Starling, W. M.	218, Richmond Road, Kingston, Surrey.
* 1919.	§Sterland, L.	126, Broomfield Road, Coventry.
1921.	§Stubings, C. A.	63, Tower Street, Dudley, Worcs.
1911.	Sullivan, D.	13, Anglesea Street, Cork.
1919.	Swift, R. A.	6, Clara Street, Stoke, Coventry.
* 1914.	Sykes, A. A.	29, White Street, Coventry.
1921.	Taylor, A. L.	"The Limes," Newport Pagnell.
1921.	Taylor, G.	12, Stratford Street, Dewsbury Road, Leeds.
1920.	§Tearne, C. E.	1, Claremont Crescent, Weston-super-Mare.
1920.	§Thomas, C. L.	Mansfield Villa, Binley Road, Coventry.
1921.	§Thomas, E. A.	Wyddrington, Church Rd., Edgbaston, Birmingham.
1921.	Thomas, L. C.	95, Queen Mary's Road, Foleshill, Coventry.

§ Probationary Graduate.

Date of Election.		
1914.	THOMPSON, C.	160, Mary Vale Road, Bourneville, Birmingham.
1921.	§THORP, A. S.	5, Station Parade, Beaconsfield.
1913.	TILSLEY, E. F.	3, Bedford Road, West Ealing, W.
1918.	TOMSON, R. E.	"Hillcrest," Station Road, Balsall Common, nr. Coventry.
1919.	TOWLER, C. O.	15, Rosebery Road, Chelmsford.
	TRAVERS, (Miss) D. A. ...	Capel House, Guildford, Surrey.
1908.	TURNER, H.	Merlebank, Weybridge.
1921.	§TURNER, R. F.	32, Bloomfield Road, Moseley, Birmingham.
1909.	TYLER, B. W.	
1920.	§TYMONS, J. H.	18, Hillaries Road, Erdington, Birmingham.
1920.	VARIAN, T.	171, Kyverdale Road, Stoke Newington, N.16.
1920.	§VAUX, H. O.	2, Middleboro' Terrace, Radford Road, Coventry.
1919.	VERRY, A.	27, Union Street, Coventry.
1920.	VINSEN, H. L.	197, Park Street, Luton, Beds.
1921.	VIVIAN-THOMAS, H. C. ...	Engineering College, Loughborough.
1916.	§WADDINGTON, E. S.	P.O. Box 5, Grahamstown, S. Africa.
1919.	WAIGHT, F. C.	61, Week Street, Maidstone, Kent.
1913.	WAKE, R. R.	J. Sankey & Sons, Ltd., Hadley Castle Works, Wellington, Salop.
1921.	WALDECK, T. E.	Technical College, Loughborough.
1912.	WARD, T. B.	41, Hustler Street, Bradford.
* 1919.	WARDALL, W. H.	2, West Villas, Ledger's Road, Slough.
1909.	WATERSTON, R.	c/o Andersons, Ltd., Engineers, Christchurch, New Zealand.
* 1919.	WATSON, L. F.	9, Kempslade Street, Deptford, S.E.8.
*g 1915.	WEDMORE, C. P.	178, Albert Road, Stechford, Birmingham.
1920.	WELBY, T. D.	16, Staplehall Road, Northfield, Birmingham.
1921.	WELCH, E. C. D.	"Newlyn," Holmfield Road, Leicester.
1914.	WELDHEN, T. W.	35, Elgin Avenue, Maida Vale, W.9.
1909.	WELLS, S.	Twixtwo, Waverley Road, Kenilworth.
1920.	§WHARTON, J.	231, Stanstead Road, Forest Hill, S.E.23.
1914.	WHITCOMB, T.	Campion, Harlow, Essex.
1920.	WHITEHEAD, R. M.	61, Ashenhurst, Huddersfield.
1920.	§WHITFIELD, G. H.	257, Somerville Road, Small Heath, Birmingham.
* 1920.	WHITING, T. H. B.	3, Wendover Road, Harlesden, N.W.10.
1921.	§WILKINSON, B. I.	11, Greenbank Crescent, Darlington.
1919.	WILLIAMS, Basil	The Croft, Ellington Road, Tapplow, Bucks.
1921.	§WILLIAMS, D. S. D.	Rosslyn Lodge, Lyndhurst Road, Hampstead, N.W.3.
1913.	WILLIAMS, H. K.	Railway Hotel, Church End, Finchley, N.
1921.	§WILLIS, H.	13, Denbigh Street, Small Heath, Birmingham.
1917.	WILLISON, R. G. E.	

§ Probationary Graduate.

Date of Election.		
1920.	WILLOUGHBY, E. P.	University Club, Edgbaston, Birmingham.
1909.	WILSON, L.	
1916.	WINDSOR, J. E.	Bridge Street, Walton-on-Thames.
1920.	§WOOD, G. N.	Harboro' Banks, Kingswood, Hockley Heath, Birmingham.
1920.	WOOD, J. H. Y.	Stratford Road, Shirley, Warwickshire.
1920.	WOODALL, A. S.	21, Peel Road, Wembley.
1911.	WOOLER, E.	3004, Essex Road, Cleveland Heights, Ohio, U.S.A.
1909.	WRENCH, E. G.	16, Coverdale Road, Millhouses, Sheffield.
1921.	§WRIGHT, A. J.	3, The Pavement, Burnham-on-Crouch.
1920.	§WROUGHTON, T. H.	57, Westgate, Mansfield, Notts.
1913.	§YOUNG, G. C.	
1920.	YULE, Jr., Thos.	6, Park Street, Bootle, Liverpool.
1912.	ZEIHER, A.	The Elk Hotel, Veemmerer, Wyoming, U.S.A.

4. *Associates.*

1919.	ALLEN, A. D.	"Clonard," View Road, Mount Eden, Auckland, New Zealand.
1920.	ARTHUR, Major E. J.	Wolseley Motors, Ltd., Sandhurst Bridge Road, Chaupatty, Bombay.
1920.	ASPEY, Jr., Urban	Geo. Aspey & Son, Ltd., Neville Street, Leeds.
1917.	ASTON, Capt. W. G.	Queen Anne's Mansions, St. James' Park, S.W.1.
1912.	BACON, Sir Hickman B., Bart.	Thonock, Gainsborough.
1920.	BENNETT, H. A.	Bennetts (Nottingham), Ltd., 28, Shakespeare Street, Nottingham.
1914.	BENNETT, P. F., O.B.E. ...	Lucas Electrical Co., Great King Street, Birmingham.
1919.	BEST, C. E.	Mercury Cars, Ltd., 24, Budge Row, Cannon Street, E.C.4.
1919.	BILBE, H. S.	93–95, High Street, Crawley, Sussex.
1910.	BRANSTON, C. A.	20, Trinity Street, Cambridge.
1920.	BRISTOW, F. G.	The Commercial Motor Users Association, 50, Pall Mall, S.W.1.
1920.	BROOKER, J. W. G.	Price's Company, Ltd., Battersea, S.W.11.
1917.	BROWNJOHN, W. H.	148, Station Road, Hendon, N.W.4.
l 1911.	BUIST, H. Massac	9, Gardnor Mansions, Church Row, Hampstead.
l 1914.	BURBERRY, H. Stone	Ingoldsby, Chelston, Torquay.
1919.	BURGOYNE, Lt.-Col. A. H.	Finchers House, Amersham, Bucks.

§ **Probationary Graduate.**

Date of Election.		
1907.	CARPMAEL, Jr., A.	24, Southampton Buildings, Chancery Lane, W.C.2.
1920.	CARTER, W.	Triumph Steel Works, Sheffield.
1920.	CHAPMAN, M.	Vickers, Ltd., 247, West George Street, Glasgow.
1912.	COATS, E. H.	Coats' Machine Tool Co., Ltd., 14, Palmer Street, S.W.1.
1908. G. 1909. A.	COLVER, E. de W. S., D.Met.	Calverthorpe Manor, Stubbington, Hants.
1913.	COOKE, Stenson	Fanum House, Whitcomb Street, W.C.2.
1919.	DODSON, P. L.	21, Ravenscourt Park, W.6.
1913.	FILER, F. E.	Glyn House, Ewell, Surrey.
1907.	GAWLEY, G. J.	Kent House, Fleet, Hants.
1910.	GLUCKSTEIN, S. M.	7, Belvedere Drive, Wimbledon, S.W.19.
1919.	GOOLD, R. L.	5, Corporation Street, Birmingham.
1912.	GOWER, F. W.	Birmingham Aluminium Castings (1903) Co., Ltd., Cambridge Street Works, Birmingham.
1920.	GRUNDY, E. E. L.	24, Victoria Grove, Kensington, W.8.
1918.	HAMILTON, O.	Midland Automobile Engineering Co., Old Stratford, Stony Stratford.
1907.	HOWELL, Lt.-Col. G. Ll. Hinds	c/o "J" Branch, Cox & Co., 16, Charing Cross, S.W.1.
1912.	HUDSON, F. R.	Cliff House, Budleigh Salterton, Devon.
1920.	JESSON, G. A. T.	Endsley, Swinton, Lancashire.
1920.	JOHNSON, W. H.	14, Panton Street, S.W.1.
1918.	JOYCE, Hy.	Anderton Mills, Sydenham Road, Birmingham.
1913.	KER, J. Inglis	"The King's Highway," 37, Piccadilly, W.1.
1919.	KILBURN, B. E. Dunbar...	Chancery Lane Station Chambers, 31, High Holborn, W.C.
1913.	KNOLLES, E. Miles	Thornton Engineering Co., Humboldt Street, Bradford.
1910.	LANCHESTER, Frank	95, New Bond Street, W.1.
1911.	LAZARUS, W.	193, Regent Street, W.1.
1912.	LEDGER, S. A.	The Hoyt Metal Co., Ltd., Deodar Road, Putney, S.W.15.
1920.	LEES, S.	51, Chesterton Road, Cambridge.
1911.	LEONARD, W. J.	Hope Chemical Works, Hackney Wick, N.E.
1909.	MCADAMS, C.	Yarm Lane, Stockton-on-Tees.
1920.	MCLEOD, W. W.	The Municipal Technical Institute, Belfast.
1919.	MOTH, J. C.	Dalkeith, Boreham Wood, Herts.

Date of Election.		
*1911.	NICOL, Sir THOMAS, K.B.E.	28, Park Lane, W.1.
1920.	O'RIORDAN, G. F.	Municipal Technical College, Park Street, Hull.
1914.	PASKELL, E. C.	31, Colmore Row, Birmingham.
1917.	PLATTS, H. W.	Messrs. Andrew Handyside & Co., Britannia Ironworks, Derby.
1918.	POLLOCK, T.	Park Vale, Wylde Green, Birmingham.
1914.	PROSSER, H.	98, Hope Street, Glasgow.
1920.	PYE, D. R.	Trinity College, Cambridge.
1914.	ROSSINI, J. L.	Portland Place, East Bay, Dunoon, N.B.
1921.	SANDERS, J. M.	"Woodside House," Canford Park, Westbury, Bristol.
1913.	SANKEY, G. H.	Finchfield House, Wolverhampton.
1917.	SHARP, G. L.	21, Springfield Gardens, Upper Clapton, N.E.
1919.	SHÖLL, W. S.	30, Oxford Gardens, Ladbroke Grove, W.10.
1914.	SIDDELEY, J. D.	Armstrong-Siddeley Motors, Ltd., Park Side, Coventry.
1918.	SKERRETT, H. N.	24, Temple Row, Birmingham.
1921.	SKERRETT, W. S.	24, Temple Row, Birmingham.
1914.	SMITH, R. J.	163, West George Street, Glasgow.
1914.	SNELL, Capt. E. SCOTT ...	1, St. John's Road, Wimbledon, S.W.19.
1918.	STOCKS, J. W.	Components, Ltd., Bournbrook, Birmingham.
1920.	STRACHAN, W. J.	Robert Gordon's Technical College, Aberdeen.
1915.	STRAIN, G. H.	20, Tower Road, Dartford, Kent.
1918.	THOMAS, H. C.	24, Townley Road, Dulwich, S.E.
1911.	THOMSON, OSCAR	Geo. Thompson & Co., Ltd., 7, Billiter Sq., E.C.
1917.	TURNER, L.	c/o The Technical Institute, Coventry.
1919.	UNDERDOWN, H. C.	Commercial Cars, Ltd., Luton.
1913.	WATKINS, A. H.	Stephen Watkins, Son & Groves, Metropolitan Chambers, Lichfield Street, Wolverhampton.
1911.	WEBB, CHAS. J.	Bentley Lodge, Rosehill Road, Wandsworth, S.W.18.
1913.	WHITCOMB, H. J.	10, St. James' Street, S.W.1.
1919.	WILKES, F. A.	The Red Garage, Llandudno.
1916.	WILLCOX, F. A.	J. & E. Hall, Ltd., Dartford, Kent.
1920.	WILLIAMS, A. GORDON	Cambridge Court, Twickenham, Middx.
1919.	WILLIAMS, H. L.	Motor Traction, 20, Tudor Street, E.C.4.
1921.	WILLIAMSON, H. R.	The Technical Institute, Ponder's End, Middlesex.
1920.	WOODHOUSE, Major BRIERLEY.	84, Jermyn Street, S.W.1.

Affiliates.

Date of Election.		
1921.	AYERS-HUNT, E.	The Yorkshire Insurance Co., Ltd., Bank Buildings, Princes Street, E.C.2.
1921.	BRACKENBURY, Capt. K. F.	Anglo-American Oil Co., Ltd., 36, Queen Anne's Gate, S.W.1.
1921.	COATHUPE, B.	London Motor Garage Co., Ltd., 33—37, Wardour Street, W.1.
1921.	DARBY-LAWSON, A.	The Chloride Electrical Storage Co., 220, Shaftesbury Avenue, W.C.2.
1921.	FULLER, H. C.	General Motors, Ltd., Thurloe Place, S.W.7.
1921.	HAYTREAD, J. S.	G. W. K., Ltd., Cordwalles Works, Maidenhead.
1921.	JOUVE, A.	Société des Automobiles Brasier, 77, High Street, Marylebone, W.1.
1921.	LAZARRE, A. E.	Michelin Tyre Co., Ltd., 81, Fulham Road, Chelsea, S.W.3.
1921.	LOVELL, DUDLEY	General Motors, Ltd., Thurloe Place, S.W.7.
1921.	NICKINSON, H. D.	Thos. Tilling, Ltd., Bull Yard, High Street, Peckham, S.E.15.
1921.	PARSONS, J. A.	Palmer Tyres, Ltd., 119—123, Shaftesbury Avenue, W.C.2.
1921.	RAMSDEN, Hy.	Ramsden's (Halifax), Ltd., Station Works, Halifax.
1921.	STONE, H. H.	Tozer, Kemsley & Millbourne, Ltd., 84, Fenchurch Street, E.C.3.
1921.	THOMAS, J.	J. Thomas & Co. (Cardiff), Ltd., 1—2, Wellington Street, Cardiff.
1921.	WATERLOW, C. H.	G. W. Waterlow & Sons, 152, High Street, Scunthorpe, Lincs.
1921.	WEIGHT, H. E.	Jeff Patent Tube, Ltd., 210, Strand, W.C.2.

The Institution of Automobile Engineers.

TOPOGRAPHICAL LIST.

ENGLAND.

Accrington—
Barlow, F. H. G.

Aldershot—
Cooper, Lt. H. J. A.M.
Foster, T. M. A.M.
Gregory, R. G.
Hubbard, Capt. R. K. M.
O'Mahony, Major C. S. M.

Alford—
Peach, W. G. G.

Altrincham—
Browning, B. E. A.M.
Meldrum, H. J. A.M.
Price, F. C. A.M.

Amersham—
Burgoyne, Lt.-Col. A. H. A.

Andover—
Hayles, R. B. A.M.

Ashton-on-Mersey—
Baker, J. A. A.M.

Ashton-under-Lyne—
Whitelegg, J. F. A.M.

Aylesbury—
Baume, H. M.
Whyte, R. A.M.

Barnet—
Charles, H. N. A.M.
Marvin, E. G. A.M.

Barnstaple—
Jackson, B. W. G.

Basingstoke—
Endacott, N. M.
Fowler, H. T. M.
Niblett, Lt.-Col. H. N. M.

Beaconsfield—
Thorp, A. S. G.

Bebington—
Elsby, L. F. G.

Belvedere—
Reed, J. S. A.M.

Beverley—
Armstrong, F. G. G. A.M.

Bideford—
Stent, C. E. M.

Birmingham—
Abbott, E. D. G.
Abraham, A. M.
Anderson, D. M. A.M.
Aitchison, Dr. L. M.
Austin, Sir Herbert M.
Aveling, T. C. M.
Barlow, B. W. G.
Bartle, H. G.
Bartlett, J. C. G.
Baylis, W. E. A.M.
Beauchamp, W. H. M.
Bedford, J. A.M.
Benbow, H. G.
Bennett, P. F. A.
Berry, J. F. G.
Bird, A. W. M.
Blake, E. H. O. A.M.
Boucher, H. W. A.M.
Brocklehurst, J. W. G.
Bromhead, Capt. E. G. A.M.
Brook, R. V. C. M.
Burkett, G. T. W. G.
Burney, C. S. A.M.
Burton, Wm. A.M.

Birmingham—*contd.*

Buswell, F. J.	G.
Butterfield, W. H.	M.
Cave, H. A. S.	A.M.
Chalmers, T. E. B.	A.M.
Clarke, F. G. E.	G.
Clarke, J.	M.
Cleverley, H. L.	G.
Cole, P. D.	G.
Cooper, J. E.	M.
Cooper, L. A.	G.
Corbett, Geo.	G.
Cramp, S. G.	A.M.
Crowdy, A. E.	M.
Davey, A. G.	A.M.
Davidge, A. W.	A.M.
Deakin, W.	A.M.
Deakin, W. A.	G.
Dobson, F. E.	G.
Dougill, A. W.	M.
Duckett, W.	M.
Duffy, J.	A.M.
Dyke, C. H.	G.
Edwards, C. A.	G.
Edwards, W. E.	G.
Ekers, S. E.	G.
Evans, G. E.	M.
Every, E. L.	G.
Faulkner, Lt. W. D.	G.
Fenn, A. P.	A.M.
Fenn, F.	G.
Ford, V. A.	G.
Goold, R. L.	A.
Gosling, C. A.	G.
Gower, F. W.	A.
Gunn, W.	A.M.
Halliwell, H. A.	G.
Hawkins, A. H.	G.
Heather, D. S.	A.M.
Higham, W. H. M.	A.M.
Hillhouse, J. P.	M.
Holmes, H.	M.
Hughan, W. J.	A.M.
Ireland, H. W. F.	M.
Jaeger, J. C.	G.
Jones, D. W. C.	G.
Jones, O. T.	A.M.
Jordan, Major C. W.	M.
Joyce, Hy.	A.
Joyce, P. J.	A.M.
Kenyon, J.	A.M.
Kesterton, H. M.	A.M.
King, C. F. L.	A.M.
Kipling, H. S.	M.
Lamming, J. G.	G.
Lamplugh, H. A.	A.M.
Lanchester, G. H.	M.
Lechmere, W. F.	A.M.
Lee, A. C.	M.
Lee, F. H.	A.M.
Leeding, E. J.	M.

Birmingham—*contd.*

Luyks, E. S.	A.M.
Male, C. D.	G.
Marshall, C. E.	M.
McCormack, A.	M.
McCormack, D.	G.
Meads, G.	G.
Morcom, R. K.	M.
Morgan, R. T.	A.M.
Morris, C. V.	G.
Munro, D. W.	G.
Muskett, H. E.	A.M.
Norton, J. L.	M.
Page, W. H.	A.M.
Palmer, A. E.	M.
Paskell, E. C.	A.
Peare, W. F.	M.
Pearsall, R. H.	M.
Player, G. R.	A.M.
Pollock, T.	A.
Potter, W. G.	G.
Pratt, J. H.	A.M.
Prestage, Jr., W. H.	G.
Prosser, S. T. E.	A.M.
Ralls, G. E. D.	M.
Reeve, E.	M.
Remington, A. A.	M.
Richards, R. W.	A.M.
Richardson, H. F.	G.
Robotham, C.	G.
Rodway, G. H.	A.M.
Rogers, E. W.	G.
Selz, R.	M.
Sherratt, L.	A.M.
Shire, L. G. Wyndham	A.M.
Shurrock, W. B.	A.M.
Skerrett, H. N.	A.
Skerrett, W. G.	A.
Skinner, T. V.	G.
Smith, H.	G.
Smith, J.	G.
Smith, P. W.	G.
Snarry, J. W.	A.M.
Sparkes, A. E.	G.
Speck, T. R.	A.M.
Spencer, O. R.	A.M.
Stocks, J. W.	A.
Sweeney, J. G.	M.
Thomas, A. P.	A.M.
Thomas, E. A.	G.
Thomas, W. J.	A.M.
Thompson, C.	G.
Tredgold, C. F. H.	M.
Turner, R. F.	G.
Tymons, J. H.	G.
Watkins, M.	M.
Wedmore, C. Pole	G.
Welby, T. D.	G.
Whitfield, G. H.	G.
Williams, H.	A.M.
Willis, H.	G.

Birmingham—*contd.*
Willoughby, E. P. G.
Willson, H. C. M.
Wood, F. A.M.
Wood, G. N. G.
Woollard, F. G. M.
Woollen, Hy. A.M.

Blackpool—
Davies, Chas. M.

Boreham Wood—
Moth, J. C. A.

Botesdale—
Reeve, L. W. C. A.M.

Bournemouth—
Bolton, D. C. A.M.
Newey, G. E. A.M.

Boxmoor—
Wilson, W. L. A.M.

Bradfield—
Summers, L. F. A.M.

Bradford—
Farnell, M. A.M.
Gavin, W. L. G.
Knolles, E. Miles A.
Scott, A. A. M.
Senior, F. G.
Ward, T. B. G.
Wood, H. O. A.M.
Young, J. A.M.

Bray—
Edmonds, R. W. M.
Lamplough, F. M.

Brentford—
Bull, C. W. A.M.

Brentwood—
Pearson, G. H. A.M.

Bridgford—
Crosher, C. R. A.M.

Bridlington—
Parkinson, B. G.

Brierley Hill—
Hadley, A. A. A.M.

Brighouse—
Bond, F. W. A.M.

Brighton—
Castle, Geo. M.
Leak, V. B. G.
Wilkins, H. J. A.M.

Bristol—
Barlow, A. E. A.M.
Butler, L. G. F. G.
Fedden, A. H. R. M.
Hayes, H. G. A.M.
Johnson, A. E. M.
Kempster, C. H. G.
Lovesey, A. C. G.
Macdonald, A. J. W. G.
Morgan, W. M.
Parsons, A. F. G.
Pizzey, A. E. R. A.M.
Pobjoy, D. R. G.
Richardson, H. A.M.
Sanders, J. M. A.

Bromley—
Judge, Lt. A. W. A.M.

Bromsgrove—
Clark, F. G. E. G.
Cornford, C. C. G.
Pellow, L. G.
Salmon, E. N. G.
Shutt, W. K. G.

Bromwich (West)—
James, C. V. Aff.

Buckhurst Hill—
Buckley, J. F. A.M.

Bulford—
Blamey, Lt.-Col. E. H. M.

Burnham-on-Crouch—
Joy, Basil H. M.
Wright, A. J. G.

Burnley—
Harling, T. F. A.M.
Whitaker, H. A.M.

Burton-on-Trent—
Baguley, E. C. M.
Warwick, G. M.

Bury St. Edmunds—
McCall, A. K. A.M.

Bushey—
Wheeler, Major Chas. M.

Byfleet—
Dougill, E. M.

Cambridge—
Branston, C. A. A.
Dykes, F. J. M.
Lampard-Vachell, B. E. A.M.
Lees, S. A.

Cambridge—*contd.*
McCarthy, H. C. G.
Norman, H. N. G.
Pye, D. R. A.
Walford, J. B. M.

Carshalton—
Paul, F. E. A.M.

Chatham—
Golding, H. A. A.M.

Chelmsford—
Burton, J. M.
Clarkson, T. M.
Marks, A. S. C. G.
Morison, W. J. M.
Towler, C. O. G.

Chepstow—
Nolan, J. A.M.

Chertsey—
Carroll, H. E. A.M.

Chesterfield—
Clark, Geo. M.

Chichester—
Carolin, C. A.M.
Hardy, W. G. A.M.

Chipping Norton—
Robinson, P. S. A.M.

Cirencester—
Fry, Capt. H. D. A.M.

Clacton-on-Sea—
Bryan, L. C. A.M.

Cleckheaton—
Bentley, J. G.

Colchester—
Lambert, H. G. R. A.M.
Podd, J. K. A.M.
Sennitt, F. J. A.M.

Cookham—
Lines, D. A. G.

Corbridge-on-Tyne—
Reed, J. A.M.

Coventry—
Allard, F. W. G.
Austin, A. C. A.M.
Aveline, R. N. G.
Baldwin, E. G.
Ballard, C. G.
Barker, H. G. G.
Barry, R. C. G.

Coventry—*contd.*
Becker, Chas. A.M.
Berriman, A. E. M.
Betteridge, C. D. G.
Birch, Capt. E. W. A.M.
Bolton, W. G.
Boote, E. M. M.
Brooke, G. V. G.
Brown, W. Ferrier M.
Budge, J. M.
Bullock, W. E. M.
Burrows, C. H. C. G.
Butler, F. B. G.
Caudwell, E. A.M.
Clease, A. G. D. A.M.
Collins, A. E. L. G.
Craig, Alex. M.
Cross, F. P. G.
Cure, H. G.
Dalley, P. H. A.M.
Daniell, J. A. A.M.
Dawson, A. J. M.
Dawtrey, L. H. G.
Day, G. H. G.
Ellis, Wm. M.
Faulkner, H. G.
Fisher, C. J. G.
French, E. J. G.
Funck, G. A.M.
Gould, H. G.
Griffiths, L. A.M.
Halliwell, F. W. A.M.
Hamilton, J. R. G.
Harries, E. C. G.
Hayward, A. J. G.
Henson, G. H. M.
Herbert, E. C. G.
Holmes, H. R. G.
Humble, A. W. G.
Johnson, E. H. G.
Jones, F. A. G.
Jones, H. M.
Jones, R. A. G.
Lyth, C. J. A.M.
Magson, A. G.
Maltby, W. L. G.
Manning, P. J. A.M.
Mansell, B. W. G.
Martin, Percy M.
Martineau, G. M. G.
Milligan, J. L. M.
Molineux, A. L. G.
Mountfort, R. G.
Newey, J. G.
Newton, R. V. A.M.
Niblett, A. H. M.
Norris, Hy. A.M.
Parson, H. W. A.M.
Pearce, R. G. G.
Philpot, S. R. G.
Pickering, O. C. G.

Coventry—*contd.*
Platt-Higgins, E. B. G.
Poole, S. C. M.
Poppe, P. A. M.
Powell, E. M. A.M.
Pugh, J. V. M.
Reynolds, J. G.
Reynolds, R. J. G.
Ridley, Capt. C. M.
Riley, V. M.
Rush, H. H. G.
Seale, R. G.
Shellard, S. H. G.
Shilson, Major B. W. M.
Shorter, L. J. M.
Siddeley, J. D. A.
Simpson, C. M. A.M.
Soanes, E. G. A.M.
Stanley, G. E. A.M.
Starley, J. K. M.
Sterland, L. G.
Sturch, H. A. A.M.
Swift, R. A. G.
Sykes, A. A. G.
Teage, Capt. H. D. A.M.
Thomas, C. L. G.
Thomas, L. C. G.
Tilt, A. A.M.
Tomson, R. E. G.
Trickett, J. H. M.
Troughton, S. H. A.M.
Turner, H. A.M.
Turner, L. A.
Vaux, H. O. G.
Verry, A. G.
Walker, F. C. A.M.
Waring-Brown, R. A.M.
Warren, H. W. H. A.M.
Young, A. P. M.

Cowley—
Rowse, A. A. M.

Crawley—
Bilbe, H. S. A.

Crayford—
Clark, F. A. A.M.

Crewe—
Lowe, J. A.M.

Crowthorne—
Bassett, C. G.

Croydon—
Ashby, F. C. A.M.
Borland, D. D. G.
Gadsden, J. E. G.
Gaydon, H. A. A.M.
Marner, S. S. A.M.
Prescott, G. A.M.

Darlington—
Hill, S. F. A.M.
Wilkinson, B. J. G.

Dartford—
Benbow, W. E. G.
Heath, A. R. N. M.
Hodsden, G. C. M.
Strain, G. H. A.
Wilcox, F. A. A.

Datchet—
Kemp, A. F. A.M.

Derby—
Bailey, R. W. Harvey M.
Bolton, R. G.
Bradshaw, G. G. A.M.
Day, B. I. M.
Edmond, E. W. A.M.
Evernden, H. I. F. A.M.
Fowler, Sir Henry M.
Lidsey, A. J. G.
Phillips, E. G.
Platts, H. W. A.
Pottinger, S. F. A.M.
Royce, F. H. M.
Staite, E. H. A.M.
Woodford, H. A.M.

Devonport—
Pottinger, A. A.M.

Doncaster—
Burdett, H. S. A.M.
Jackson, E. A. A.M.
Popplewell, F. B. A.M.

Dorking—
Russell, A. A. A.M.

Droitwich—
Bunn, G. M.

Dudley—
Bishop, W. H. A.M.
Stubings, C. A. G.

Dursley—
Price, R. S. G.

Ealing—
Ince, T. A. A.M.

Eastbourne—
Barber, P. A. J. A.M.
Mansfield, A. W. A.M.
Smith, C. A.M.

Edenbridge—
Ellis, H. C. A.M.

Edgware—
Hill, W. B. A.M.

Elstree—
Stephenson, C. H. A.M.

Enfield—
Dormer, C. M. G.
Dormer, W. F. A.M.

Erith—
King, H. G. M.
Steinheil, G. M.

Esher—
Burchall, H. A.M.
Gillett, Thos. M.

Ewell—
Filer, F. E. A.

Ewhurst—
Harrington, L. A. A.M.

Exeter—
Milton, W. H. A.M.

Falmouth—
Lewis, A. E. A.M.
Taylor, S. L. M.

Farnborough (Hants)—
Bargman, J. F. A.M.
Golder, H. C. G.
Gowring, E. C. A.M.
Hazell, J. A. G.
Haydon, F. G. A.M.
Hoar, H. T. G.
Marsh, W. W. G.
Maxwell, R. M. S. G.
Pitt, A. G. M.
Robinson, E. M.
Salmon, P. M.
Stevenson, A. Creery M.

Farnborough (Kent)—
Appleyard, E. A.M.

Farnham—
Gravenor, J. A.M.

Farningham—
Wilson, Major W. G. Hon. M.

Ferndown—
Tamplin, A. T. A.M.

Ferrybridge—
Shepherd, G. W. G.

Fleet—
Gawley, G. J. A.

Fleetwood—
Titterton, W. A.M.

Formby—
Coole, Thos. M.

Gainsborough—
Bacon, Sir Hickman B. A.
Eden-Eadon, Lt. A. T. G.

Gerrards Cross—
Roscoe, Capt. E. C. A.M.

Gloucester—
Reid, W. J. A.M.

Godalming—
Buttemer, R. W. M.
Kirkaldy, Major A. H. M.

Goodmayes—
Charge, H. R. A.M.

Guildford—
Charles, C. R. M.
Clifford, A. C. M.
Crosby, A. D. G.
Gammon, D. L. G.
Gray, A. A. A.M.
Jacobs, G. H. A.M.
Murray, Geo. M.
Travers, (Miss) D. A. G.
Williams, H. L. A.M.

Halifax—
Holroyd, J. M. M.
Ramsden, H. Aff.
Smith, Edgar A.M.
Smith, G. H. G.
Smith, S. A.M.

Hampton—
Clare, E. M.
Clare, Junr., E. G.
Searle, Col. F. M.

Hampton-in-Arden—
Bradley, T. G. G.

Hanham—
Fincken, Geo. A.M.

Harlow—
Whitcomb, T. G.

Harrogate—
Mackay, W. M. A.M.

Harrow—
Saunders, S. C. A.M.
Ward, C. W. A.M.

Hayes—
Scott, T. L. G.

Hayling Island—
Ingram, J. A.M.

Hereford—
Morley, E. G. G.

High Wycombe—
Stevenson, D. A.M.

Hitchin—
Sharman, P. A. M.

Hornchurch—
Morgan, P. D. G.

Horsham—
Purves, Wm. W. A.M.

Hove—
Ainsworth, A. G. A.M.
Bechtell, G. H. A.M.
Burton, P. G. M.
Lester, H. G. A.M.
Pigott, C. O. A.M.

Huddersfield—
Applegate, H. F. A.M.
Bostock, F. J. M.
Bramley-Moore, Capt. S. M.
Clayton, R. F. M.
Marchant, H. G. M.
Whitehead, R. M. G.
Wilson, A. D. A.M.
Young, W. M.

Hull—
Nicholson, J. A.M.
Nicholson, T. M. A.M.
O'Riordan, G. F. A.
Parish, C. A.M.
Simpson, A. T. M.

Icklesham—
Goddard, G. A.M.

Ilford—
Daniel, J. P. G.

Ilfracombe—
Brock, F. G. A.M.

Ilkley—
Hoyland, S. A.M.

Ingatestone—
Sherrin, G. C. A.M.

Ipswich—
Elliott, E. T. G.
Rickards, J. G. A.M.

Isleworth—
Fairbrother, Lt. E. A.M.

Kenilworth—
Coulson, R. A.M.
Hacking, J. T. G.
Napier, J. S. M.
Prior, D. L. A.M.
Smith-Clarke, Capt. G. T. M.
Wells, S. G.

Kenley—
Smith, W. T. M.

Kew Gardens—
Blake, F. C. M.
Fisher, H. R. A.M.
Osmand, Lt. G. A.M.
Payne, Lt. L. T. A.M.

Kidderminster—
Firth, E. L. A.M.

Kingsbury—
Bennett, F. S. M.

King's Lynn—
Gill, J. W. H. M.

Kingston-on-Thames—
Hounsfield, L. H. M.
Starling, W. M. G.
Turner, E. A. A.M.

Kingswear—
Carey, F. A.M.

Knighton—
Evans, Major C. B. M.

Lancing—
Ricardo, H. R. M.

Leamington—
Abell, C. F. A.M.
Brown, C. G. G.
Chatterton, R. B. A.M.
Cox, C. H. A.M.
Holroyd, V. M.
Maudslay, R. W. M.
Pendrell, A. G. A.M.
Robertson, Capt. H. J. A.M.
Wild, Mark M.

Leeds—
Appleyard, J. E. A.M.
Aspey, Junr., U. A.
Cowell, F. R. A.M.
Hodgson, Herbert M.
Lower, W. P. A.M.
Sellars, C. A. A.M.
Taylor, G. G.
Williams, J. Lloyd A.M.
Winn, R. M.

Leicester—
Briggs, H. G.
Ionides, A. G. M.
Welch, E. C. D. G.

Leiston—
Serve, A. J. A.M.

Letchworth—
Brown, F. J. M.
Hooydonk, J. van M.

Leyland—
Naylor, J. T. A.M.
Plant, A. E. A.M.
Thomas, J. G. P. M.
Windsor, A. W. M.

Lincoln—
Armstrong, J. G.
Barker, A. A.M.
Bretherton, F. J. A.M.
Gartside, G. S. A.M.
Gilbert, G. A.M.
Mawer, S. M.
Nelson, H. A.M.
Smith, G. A. A.M.
Wans, O. M.
Wealleans, H. A.M.

Liphook—
Murray, Dr. Blackwood M.

Liscard—
Brayshaw, H. A. A.M.

Liverpool—
Atherton, J. H. A.M.
Atkinson, R. A.M.
Baragwanath, E. G. G.
Birchall, C. A.M.
Fairrie, G. A.M.
Jolly, J. C. A.M.
McDowall, L. G.
Myers, E. R. A.M.
Oakes, H. I. G.
Paterson, T. W. A.M.
Pitts, G. Y. G.
Pollitt, Junr., J. A.M.
Priestley, P. M.
Reece, J. Graham M.
Royle, R. S. A.M.
Scholes, G. E. A.M.
Yule, T. G.

London—
Abbott, J. D. A.M.
Abrahams, W. H. A.M.
Adam, M. A. M.
Adam, W. F. M.
Addis, J. A.M.
Allday, E. H. A.M.
Armstrong, K. P. F. G.
Arnold, T. J. A.M.
Arnold, W. R. H. A.M.
Arrow, P. A.M.
Arter, A. M. M.
Arter, Erland A.M.
Aston, W. G. A.
Atkinson, R. M. M.
Attree, J. R. G.
Ayers-Hunt, E. Aff.

London—*contd.*
Bagnall-Wild, Brig.-Gen. R. K. M.
Baillie, G. H. M.
Bale, Major F. H. M.
Bales, F. G. A.M.
Ball, J. H. A.M.
Bamber, H. W. M.
Bamford, R. A.M.
Barker, W. J. A.M.
Barrett, Capt. N. A.M.
Barton, A. D. A.M.
Bass, E. L. G.
Beaumont, Major E. G. E. M.
Beaumont, W. Worby M.
Beere, W. J. E. A.M.
Bell, J. M.
Bell, R. M.
Bennet, J. W. A.M.
Bennett, A. E. A.M.
Benny, H. B. A.M.
Best, C. E. A.
Betterton, W. A.M.
Bickell, S. F. G.
Bircham, F. R. S. M.
Birmingham, A. J. G.
Bishop, P. A.M.
Blackford, H. O. A.M.
Blanch, T. A. A.M.
Boden, H. G.
Bohringer, A. A.M.
Bonallack, W. M.
Booth, J. F. G.
Bown, C. H. M.
Boyd, R. H. A.M.
Brackenbury, Capt. K. F. Aff.
Bradley, A. P. A.M.
Bradley, R. P. A.M.
Brander, Lt.-Col. M. S. M.
Brent, L. G.
Bretton, H. J. A.M.
Briggs, Engr. Commr. W. M.
Bristow, F. G. A.
Bristow, Lt.-Col. W. A. M.
Brooker, J. W. G. A.
Brown, G. W. A. M.
Brown, Lt. M. H. A.M.
Browne, Lt.-Col. T. B. M.
Brownjohn, W. H. A.
Burford, E. H. G.
Burford, H. G. M.
Burgess, F. T. M.
Burgoine, Capt. A. C. M.
Burleigh, W. H. A.M.
Burnside, G. M. M.
Burt, E. J. M.
Burt, S. A.M.
Buvyer, G. A.M.
Caillatt, J. M.
Callendar, Prof. H. L. Hon.M.
Cannell, W. M.

London—*contd.*

Caplin, E. S.	G.
Carpenter, T. D.	A.M.
Carpmael, A.	A.
Castello, W. E.	A.M.
Catherwood, W. A.	A.M.
Chalmers, A.	A.M.
Chapman, A. W.	A.M.
Chappell, C. A.	G.
Chater-Lea, Wm.	M.
Chatterton, E.	G.
Cheeseman, A. B. E.	M.
Chorlton, A. E. L.	M.
Clark, Major A. G.	M.
Clark, David	M.
Clerk, Sir Dugald	M.
Coathupe, B.	Aff.
Coats, E. H.	A.
Cochrane, Lt.-Col. T. H.	M.
Cockerill, J. F.	A.M.
Collier, H. A.	A.M.
Collins, A. J.	A.M.
Collins, W. F.	A.M.
Conroy, T. M.	M.
Cook, Stenson	A.
Coombes, Geo.	M.
Cosway, Major L. H. Baskerville	M.
Courtney, T.	M.
Coventry, F. C. A.	M.
Critchley, Capt. J. S.	M.
Cromie, W. H.	A.M.
Crompton, Claud	M.
Crompton, Col. R. E.	M.
Cruickshank, G. M.	A.M.
Cundy, F. G.	A.M.
Currin, S. A.	A.M.
Darby-Lawson, A.	Aff.
Davey, A. T. H.	A.M.
Davidson, Lt.-Col. A. E.	M.
Davies, Capt. E. K.	A.M.
Davies, E. O.	A.M.
Davis, Capt. S. H.	A.M.
Deakin, A. W.	A.M.
De la Garde, L. A.	A.M.
Dermigny, H.	A.M.
De Silva, Louis	A.M.
Dodsdon, P. L.	A.
Donkin, H. J.	M.
Dorling, J. A.	A.M.
Dormer, C. M.	G.
Doyle, D.	M.
Drayson, W. F.	M.
Drover, Capt. D.	A.M.
Eason, J. E.	G.
Eastaugh, G.	A.M.
Elliott, E. S.	A.M.
Emerson, C. F.	G.
Emerson, K. B.	A.M.
Emtage, E. L. M.	A.M.
Erade, C. J.	M.

London—*contd.*

Esse, C. E.	M.
Evans, A. F.	M.
Feeny, V. F.	M.
Fisher, W. L.	M.
Fleming, E. W.	G.
Flood, C. J. M.	A.M.
Forrest, H. R.	A.M.
Forster, G. E.	A.M.
Fuller, H. C.	Aff.
Fulton, S. M.	G.
Garbutt, H.	A.M.
Garrard, C. R.	M.
Gibbons, N. B.	M.
Gidden, L. Sapsford	G.
Gigli, R.	G.
Gillet, E. H. J. C.	M.
Girling, A. H.	A.M.
Glaser, H.	M.
Glass, W.	A.M.
Gluckstein, S. M.	A.
Gnapp, G. C.	A.M.
Gould, A. E. B.	A.M.
Grace, V. R. P.	G.
Graham, C. E.	G.
Gray, J. R.	G.
Green, F. M.	M.
Greenham, C.	G.
Greening, S. M.	A.M.
Gregory, H. H.	M.
Griggs, J. W.	G.
Grone, A. A.	A.M.
Grundy, E. E. L.	A.
Guildford, C. H.	A.M.
Hacking, C. W.	M.
Hadfield, Sir Robert	M.
Hall, H. Scott	M.
Halliday, Higley	M.
Hallpike, A. W.	G.
Halsall, C. F.	M.
Halsall, C. R.	G.
Hamilton, D. W.	M.
Hands, L. N.	A.M.
Hardy, P. H. C.	G.
Harris, L. B.	G.
Harrison, J.	A.M.
Harrison, R. N.	A.M.
Harrison, T. Crespin	A.M.
Harvey-Tomeo, W.	A.M.
Hawes, S. H.	A.M.
Hawkes, W. D.	A.M.
Hayter, G. W.	M.
Hedgcock, F. W.	A.M.
Hele-Shaw, Dr. H. S.	M.
Hemens, H. A.	A.M.
Henn, S. T.	G.
Herbert, A. G. A.	A.M.
Hesse, Major J. H. B.	M.
Hickson, S. C. S.	M.
Higgs, G. G.	A.M.
Hingston, W. H.	M.

London—*contd.*

Hiscocks, S. W.	M.
Hoare, Capt. H. G.	A.M.
Holden, Brig-Gen. Sir Capel	M.
Holloway, S. C.	M.
Holt-White, Major F. R.	A.M.
Hope, Capt. A. R.	A.M.
House, C. E. G.	A.M.
Howell, Lt.-Col. G. Ll. Hinds	A.
Hudlass, F. W.	M.
Hunter, A.	A.M.
Hutchings, R. R.	A.M.
Hutchinson, Col. T. M.	M.
Iden, W. J.	M.
Ireland, A. J.	A.M.
Jackson, W.	A.M.
Jacques, A. V.	A.M.
James, W. G.	M.
Johnson, E. W.	A.M.
Johnson, H.	A.M.
Johnson, W. H.	A.
Jones, D. A.	A.M.
Jones, H. Drew	G.
Jones, Lieut. T. P.	A.M.
Jouve, A.	Aff.
Joy, B. C.	M.
Junner, G. M.	A.M.
Keep, T. B.	A.M.
Kemp, E.	A.M.
Kennedy, D.	M.
Kennedy, S. S.	M.
Kennington, W. O.	M.
Kent, R.	G.
Ker, J. Inglis	A.
Kilburn, B. E. D.	A.
King, A.	A.M.
King, C. S.	G.
Kingstone, H. G.	M.
Knaggs, M. J.	A.M.
Knowles, G. J. F.	M.
Lancaster, E. H.	M.
Lanchester, F. W.	M.
Lanchester, Frank	A.
Lander, C.	A.M.
Lawrence, Max R.	M.
Lazarre, A. E.	Aff.
Lazarus, W.	A.
Ledger, S. A.	A.
Leechman, G. D.	M.
Legros, L. A.	M.
Leitner, H.	M.
Leonard, W. J.	A.
Lewin, Major H. W.	M.
Liddell, C. B.	A.M.
Lister, W. W.	G.
Littlewood, L. A.	G.
Longstaffe, B. H.	A.M.
Love, L. A.	A.M.
Lovell, D.	Aff.
Low, Dr. A. M.	M.

London—*contd.*

Low, E. H.	A.M.
Lucas, Lt.-Col. F. W.	G.
Macdonald, W. H.	A.M.
Macfarlane, W. M.	M.
Macklin, D. J.	G.
Mackrow, G. H.	A.M.
Maclean, J. B.	M.
Mahomed, J.	G.
Maidens, Lt. J. R.	A.M.
Manville, E.	M.
Markham, Lt.-Col. R. G.	M.
Marples, S. A.	M.
Marden, A.	M.
Martineau, F. L.	M.
Mathias, J. S.	A.M.
McConnell, H. P.	A.M.
McGillivray, J. A.	A.M.
Meyer, T. B.	A.M.
Mills, Major H. C.	A.M.
Mitchell, A. W.	G.
Mitchell, J. B.	A.M.
Morgan, B. H.	M.
Morris, G. L.	M.
Mossay, P. A.	A.M.
Mosses, A.	M.
Muir, R. S.	A.M.
Nash, Capt. A. Frazer	M.
Nash, Capt. E. C. W.	A.M.
Needham, S. O.	A.M.
Nevill, Lt.-Col. S. S.	M.
New, R. H.	A.M.
New, V. G.	M.
Newton, F.	M.
Nickinson, H. D.	Aff.
Nicol, Sir Thos.	A.
Noel-Storr, C. E.	A.M.
North, O. D.	M.
O'Gorman, Lt.-Col. Mervyn	M.
Opperman, Carl	M.
Ormandy, Dr. W. R.	M.
Osler, J. B.	M.
Owen, Major A. D.	M.
Owen, W. J.	A.M.
Padgen, W. J.	A.M.
Page, N. V.	A.M.
Pagis, A.	A.M.
Parisot, E. la V.	G.
Parnacott, A. E.	M.
Parnell, F. G.	A.M.
Parritt, L. J.	G.
Parsons, J. A.	Aff.
Pate, Geo., Jr.	M.
Paterson, B.	A.M.
Payne, G. S.	A.M.
Peacock, C. D.	G.
Peel, W. E.	A.M.
Pettit, A. R.	A.M.
Philpott, Major H. P.	M.
Pickard, Major J. A. A.	M.
Pickering, G. F.	A.M.

London—*contd.*

Pitt, H. W.	M.
Pitt, P. S.	M.
Plaister, W. E.	M.
Pollard, G.	M.
Ponsford, G. L.	M.
Pope, C. R. Egerton	M.
Potter, E. K.	G.
Prance, H. Waymouth	M.
Prestwich, J. A.	M.
Pugh, Rhys	A.M.
Putland, E. E.	A.M.
Rainer, E. A.	M.
Rainforth, W. F.	M.
Rallings, E. H.	M.
Rawlings, F.	A.M.
Rawlings, H. S.	M.
Rayner, F.	A.M.
Rendall, W. R. W.	A.M.
Renwick, W. S.	A.M.
Richardson, D. H. G.	A.M.
Rivett, A.	M.
Robbins, S. T.	A.M.
Roberts, W. St. Clare	A.M.
Robinson, M. Scott	M.
Rochford, L. H.	G.
Ronca, J. F.	A.M.
Rose, Lt.-Col. E. A.	M.
Ross, Capt. A. A.	M.
Ross, J. W. E.	G.
Rowell, H. S.	M.
Rowledge, A. J.	M.
Rush, C. H. E.	M.
Rushton, G.	M.
Savage, W. P.	A.M.
Scholte, Lt.-Col. L. F.	A.M.
Scott, J. Morse	A.M.
Scott, S. B.	A.M.
Scott, W. C. H.	A.M.
Sharp, Prof. A.	M.
Sharp, G. L.	A.
Sharp, R. H.	A.M.
Shave, G. J.	M.
Shaw, F.	A.M.
Shaw, G. Livingstone	A.M.
Simms, F. R.	M.
Simpson, E.	M.
Simpson, T. G.	G.
Smith, Lt.-Col. D. J.	M.
Smith, F. G.	A.M.
Smith, P. W.	M.
Smith, R. R.	M.
Snell, Capt. E. Scott	A.
Snowden-Smith, Lt.-Col. R. T.	M.
Splitter, B.	A.M.
Spooner, Prof. H. J.	M.
Stafford, E.	A.M.
Stewart, C. J.	A.M.
Stokes, E.	A.M.
Stone, H. H.	Aff.

London—*contd.*

Strachan, J. M.	A.M.
Straker, S.	M.
Strickland, F.	M.
Sumner, W. H.	M.
Swinton, A. A. Campbell	M.
Symonds, Major J. C.	A.M.
Tankard, Hy.	A.M.
Tapp, E. T.	A.M.
Taylor, G. Campbell	M.
Tennant, W. J.	M.
Thomas, H. C.	A.
Thomas, H. Kerr	M.
Thomas, W. P.	M.
Thompson, E. F.	M.
Thomson, A. F. W.	A.M.
Thomson, K. J.	M.
Thomson, O.	A.
Thomson, R. St. Clare	A.M.
Thonger, Lt. J. R.	A.M.
Thurston, Lt. A. P.	M.
Tilling, R. M.	M.
Tilsley, E. F.	G.
Tilston, E.	M.
Tolley, Capt. R. G.	M.
Tookey, W. A.	M.
Travers, J. L.	A.M.
Tulloch, Capt. T. G.	M.
Turrell, C. McR.	M.
Varian, E.	G.
Vincent, E. T.	A.M.
Wade, F. R.	M.
Walling, S.	A.M.
Walthull, Capt. H. D.	M.
Ware, E. B.	M.
Warner, L. W.	A.M.
Waterhouse, L. M.	M.
Watson, G. W.	M.
Watson, L. F.	G.
Watson, O. W. J.	M.
Webb, C. J.	A.
Weldehen, T. W.	G.
Wellingham, H. J.	A.M.
Wells, W. H.	M.
Wharton, J.	G.
Whitcomb, H. J.	A.
White, T. E. B.	A.M.
White, H. T.	M.
Whiting, T. H. B.	G.
Wilder, Lt.-Col. H. C.	M.
Wilding, J. A.	M.
Wilkins, Capt. G. B.	A.M.
Wilkinson, G. S.	M.
Williams, D. S. D.	G.
Williams, H. K.	G.
Williams, H. L.	A.
Willis, H.	A.M.
Wilson, W. B.	A.M.
Wood, T.	A.M.
Wood, W. J.	A.M.
Woodcock, Major R. R.	A.M.

London—*contd.*
Woodhouse, Major B. A.
Woollen, T. H. M.
Workman, W. H. A.M.
Wright, H. E. Aff.
Young, G. M. M.

Longtown—
Lindsay, R. J. A.M.

Loughborough—
Bower, G. S. A.M.
Bright, J. E. A.M.
Day, R. W. G.
Goffey, P. H. G.
Hackney, W. G.
Hardy, G. D. G.
Johnson, W. P. A.M.
Le Maitre, H. E. G.
Linford, (Miss) E. J. G.
Oliver, E. A. C. G.
Russell, F. A.M.
Snow, W. E. G.
Vivian-Thomas, H. C. G.
Waldeck, T. E. G.

Lowesmoor—
Barker, P. W. A.M.

Lowestoft—
Davison, E. G. A.M.
James, J. M. M.
Talbot, E. M.

Ludlow—
Lane, A. S. G.

Luton—
Attree, F. G.
Hancock, A. J. M.
Kidner, P. C. M.
King, E. A.M.
Lissman, G. S. G.
Marriott, E. J. A.M.
Moyes, E. H. A.M.
Rossiter, A. M.
Thomas, E. M.
Underdown, H. C. A.
Vinsen, H. L. G.

Lytham—
Draper, H. A.M.

Maidenhead—
Evans, C. W. A.M.
Grice, A. G. M.
Haytread, J. S. Aff.
Keiller, C. M. M.
Wilton, H. L. A.M.

Maidstone—
Barnwell, E. A. M.
Smith, P. Frost M.
Waight, F. C. G.

Malling—
Hutt, A. C. A.M.

Manchester—
Arnold, W. A.M.
Bell, P. A. G. M.
Derwent, A. L. G.
Elliott, P. L. D. A.M.
Galbraith, T. A.M.
Gawkrodger, W. A.M.
Haworth, J. W. M.
Hutton, J. M. A.M.
Jagger, J. G. A.M.
Johnson, N. W. A.M.
Kenyon, A. A.M.
Leadbetter, T. A. N. A.M.
Middlemiss, E. T. M.
Phillips, W. E. M.
Reed, Hy. A.M.
Reynold, T. A.M.
Roberts, H. W. A.M.
Romyn, A. W. G.
Roy, R. C. A.M.
Royle, F. G.
Saunders, W. H. A.M.
Stafford, C. E. A.M.
Tate, W. H. A.M.
Webb, W. L. M.
Wyke, C. J. A.M.

Mansfield—
Evinson, C. M. A.M.
Kent, S. T. A.M.
Wroughton, T. H. G.

Manuden—
Burls, G. A. M.

Margate—
Brown, L. W. G.
Naish, E. P. G.

Market-Harboro—
Lonsdale, H. M. A.M.

Marple—
Wells, R. G. A.M.

Middlesbrough—
Pearse, H. A.M.
Stead, Dr. J. E. M.

Molesey—
Smith, Major W. H. M.

Monkseaton—
Gilliatt, E. V. G.

New Brighton—
Berry, J. H. G.
Braimbridge, H. G.
Robinson, Capt. G. H. A.M.

New Malden—
Noble, A. M.

Newark-on-Trent—
Littler, L. M.

Newbury—
Chillingworth, Percy A.M.
Kidd, R. H. G.
Marshall, C. F. B. M.

Newcastle-on-Tyne—
Cummings, H. T. A.M.
Hodgson, Major Duncan M.
Hutchinson, T. A.M.
MacFarlane, D. A.M.
Marden, G. E. G.
Towns, H. L. A.M.

Newport Pagnell—
Taylor, A. L. G.

North Shields—
Clark, V. I. R. A.M.

Northampton—
Dickens, W. E. B. A.M.
Dover, H. W. M.
Fisher, T. M. G.
Grose, W. T. A.M.
Jones, H. F. G.
Mobbs, H. A.M.

Nottingham—
Baird, R. G. G.
Bennett, H. A. A.
Brough, G. A.M.
Comery, Wm. M.
Hands, W. E. G.
Ross, R. H. A.M.

Nuneaton—
Clark, F. I. G.
Mathers, J. H. A.M.
Sweeting, C. P. A.M.

Oldham—
Cross, J. A.M.

Olton—
Florance, T. R. G.

Oxford—
Giblett, Major R. H. A.M.
Landstad, H. M.

Oxton—
Hughes, D. A. G.

Penrith—
Fleming, S. A.M.
Taylor, Geo. A.M.

Peterborough—
Booth, J. H. M.
Bowness, E. A.M.
Burdett, G. W. S. G.
Dewhurst, Frank A.M.
Jackson, H. B. G.
Pointon, H. E. M.

Plymouth—
Andrew, J. H. A.M.
Hebblethwaite, A. C. A.M.
Humm, R. A.M.

Ponders End—
Hellon, K. A. A.M.
Williamson, H. R. A.

Portsmouth—
Taylor, H. J. A.M.

Preston—
Breakell, F. B. A.M.

Radlett—
Greenland, Major H. M.

Ramsgate—
Knight, F. W. G.
Rofe, H. G.

Rayleigh—
Copsey, W. A. B. M.

Reading—
Adams, C. H. A.M.
Curtis, E. G.
Kennedy, Lt.-Col. D. S. M.
Sully, R. J. M.
Woollan, S. W. M.

Redditch—
Guise, H. T. M.
Smith, F. W. M.
Smith, R. W. M.

Redhill—
Whyte, S. M.

Reigate—
Woodall, J. C. A.M.

Richmond—
Crook, W. A.M.
Jay, R. G.
Lambert, Major A. W. A.M.
Leckie, R. V. G.
Norwood, C. R. M.

Rochester—
Cozens, P. H. G.
Jones, P. Muir A.M.

Rock Ferry—
Okill, J. M.

Rotherham—
Smitheman, A. H. A.M.

Rothley—
Calvert Harrison, Capt. J. A. A.M.

Rubery—
Penwarden, F. A. G.

Rugby—
Hemmings, H. F. M.
Hopps, Bernard M.
Saxelby, F. A. A.M.

Runcorn—
Allen, J. R. G.

Salcombe—
Hooper, P. N. M.

Salford—
Kersey, A. T. J. M.

Salterton—
Hudson, F. R. A.

Sanderstead—
Sholl, W. S. A.

Scunthorpe—
Waterlow, C. H. Aff.

Sedgley—
Lamming, J. G. G.

Selby—
Birrell, E. A.M.

Sheen—
Cadett, J. M.
Smallwood, H. F. A.M.

Sheffield—
Bayliss, A. A.M.
Best, F. A. A.M.
Brearley, Hy. M.
Carter, W. A.
Clerke, Major A. B. M.
Crawley, R. C. A.M.
Dawson, W. J. M.
Dewhurst, F. A.M.
Dickenson, J. H. S. M.
Fearnley, A. R. M.
Fearnley, R. A. G.
Flather, David M.
Flather, G. D. M.
Hatfield, E. W. M.
Hatfield, Dr. W. H. M.
Henderson, L. A.M.
Huntley, Lt. G. E. A.M.
Jackson, C. A.M.
Meldrum, C. A.M.
Milward, W. F. A.M.
Perkins, C. B. M.
Pitt, J. D. M.
Potts, J. L. A.M.
Wrench, E. G. G.

Shepperton—
Cotsworth, G. W. A.M.

Shipley—
Baxter, C. A.M.

Shirley—
Wood, J. H. V. G.

Slough—
Cleaver, Lt. C. F. M.
Haynes, L. A. G.
Humphrey, Barnard M.
Mitchell, R. J. M.
Wardall, W. H. G.

Southampton—
Hutchinson, H. W. M.

South Bank—
Parker, J. B. A.M.

Southport—
Grinham, E. A.M.
Hopkins, W. H. M.
Mallins, C. W. M.
Parr, H. A.M.

St. Albans—
Hutchinson, F. W. H. M.
Hyland, Capt. C. A.M.

St. Anne's-on-Sea—
Aked, Wm. A.M.

St. Ives—
Parker, Lt. R. G. A.M.

St. Margaret's-on-Thames—
Hayward, W. G. C. A.M.
Hey, Geo. M.

Stafford—
James, R. V. G.
Outram, W. C. A.M.

Stanmore—
Wood, E. B. M.

Stockport—
Barnett, E. P. A.M.
Constantine, W. W. M.
Galbraith, Capt. J. A. M.
Jones, J. H. A.M.
Owen, R. S. M.
Thornburn, G. M.
Turtle, Chas. A.M.
Witard, S. L. A.M.

Stockton-on-Tees—
McAdams, C. A.

Stony Stratford—
Hamilton, O. A.

Stubbington—
Colver, E. de W. S. A.

Sunderland—
Green, Engr.-Commr. E. C. M.
Howell, J. C. M.

Surbiton—
Barnes, F. W. M.
Dommett, Capt. W. E. A.M.
Heaton, C. B. A.M.

Sutton Coldfield—
Benest, (Miss) de H. A.M.
Nasmith, J. M.
Pearson, J. L. G.
Riding, C. H. G.
Stubbs, A. M.

Swinton—
Jesson, G. A. T. A.

Taplow—
Williams, Basil G.

Teddington—
Gray, W. H. A.M.
Hyde, J. H. M.
Rosenhain, Dr. W. M.

Teignmouth—
Knee, E. B. G.

Thames Ditton—
Shoebottom, W. H. A.M.

Thornton Heath—
Grace, Lt. C. H. A.M.
Hirschfield, J. A. G.
Landmark, J. S. A.M.
Turner, A. B. A.M.

Tonbridge—
Hall, H. E. M.

Torquay—
Burberry, H. Stone A.

Tunbridge Wells—
Graham, G. H. G.
Hasler, H. N. A.M.
Pritchard, W. O. M.
Stirling, John M.
Thorpe, T. H. A.M.

Twickenham—
Brown, A. G.
Buckingham, W. R. A.M.
Lord, A. O. A.M.
Pentony, R. M.
Williams, A. Gordon A.

Tynemouth—
Henderson, T. A.M.

Uxbridge—
Fox, R. S. A.M.

Wakefield—
England, B. G.

Wallasey—
Jackson, G. G. A.M.

Wallington—
Gallagher, Capt. H. N. A.M.

Walsall—
Spruce, E. U. A.M.

Walton-on-Thames—
Windsor, J. E. G.

Warlingham—
Loughborough, T. W. A.M.

Watford—
Browne, H. E. G.
Marshall, A. O. G.
Masser, A. H. G.

Wednesbury—
Smallman, H. S. M.

Wellington (Salop)—
Wake, R. R. G.

Wembley—
Begbie, S. D. M.
Clench, E. C. M.
De Fraine, A. F. G.
Hiscox, H. W. G.
Mills, Lt.-Col. G. P. M.
Runnells-Moss, N. A. A.M.
Wickham, W. E. A.M.
Woodall, A. S. G.

Westcliff-on-Sea—
Hudson, A. A.M.

Westgate-on-Sea—
Satterford, H. T. M.

Weston Park—
Clare, L. J. A.M.
Stagg, W. S. A.M.

Weston-super-Mare—
Tearne, C. E. G.

Weybridge—
Costigan, J. C. M.
Martin, H. P. A.M.
Turner, H. G.

Weymouth—
Spivey, G. A.M.

Whyteleafe—
Pike, Lt. F. E. A.M.

Wigan—
MacDonagh, F. M. A.M.
Timberlake, H. H. A.M.
Walker, J. S. A. A.M.
Williamson, W. D. M.

Willenhall—
Miller, K. B. G.

Winchcombe—
New, C. T. A.M.

Windsor—
Davies, Capt. O. G.

Woking—
Woollen, Hy. A.M.

Wolverhampton—
Acres, F. A. S. M.
Alstead, T. A.M.
Booth, A. G. A.M.
Bowden, S. H. G.
Bowkett, N. W. A.M.
Buckney, E. J. D. M.
Coatalen, L. M.
Cozens, N. A.M.
Currie, J. A.M.
Dixon, G. H. G.
Dudley, J. A. A.M.
Eugen, C. M. van A.M.
Ford, W. V. A.M.
Greenwood, J. E. M.
Hart, E. T. A.M.
Heard, J. F. G.
Irving, Capt. J. S. M.
Jones, A. E. A.M.
Kay, C. B. M.
King, D. W. G.
Lisle, R. M.

Wolverhampton—*contd.*
Marston, Chas. M.
Mitchell, A. P. A.M.
Passmore, G. W. G.
Pountney, F. M.
Russell, G. G.
Ryan, D. V. A.M.
Sankey, G. A.
Sankey, Capt. H. B. G.
Stevens, A. J. M.
Stevens, G. A.M.
Stevens, H. A.M.
Stevens, J. A.M.
Thomas, A. A.M.
Watkins, A. H. A.

Wooburn Green—
Salter, F. E. A.M.

Woodford Green—
Grant, Lt. R. J. A.M.
Lomax, S. A.M.
Savage, G. H. A.M.

Worcester—
Spackman, N. G.

Worksop—
Hunter, A. R. A.M.
Wardell, T. M.

Worthing—
Evans, A. T. M.
Gambell, H. C. A.M.
Reilly, F. M. A.M.

Wraysbury—
Thomas, R. G. A.M.

York—
Darnell, H. A.M.
Gibbs, J. E. M.
Hunter, M. C. A.M.

IRELAND.

Belfast—
Ferguson, Hy. A.M.
McLeod, W. W. A.

Cork—
Sullivan, D. G.

Dublin—
Hallett, A. A.M.
Kelly, W. F. A.M.
Pile, W. D. A.M.
Reeves, J. H. A.M.

SCOTLAND.

Aberdeen—
Asher, R. S. A.M.
Dunnett, D. G.
Mitchell, G. M.
Smith, P. W. M.
Strachan, W. J. A.

Arbroath—
Young, W. F. C. A.M.

Bothwell—
Morton, T. M. Gray M.

Coatbridge—
Elvidge, Hy. M.
Benson, Carl T. F. M.

Dalmuir—
Richards, G. T. M.
Richardson, J. M.

Dumfries—
Drummond, D. M. M.
Eastwood, Geo. A.M.
Pullinger, T. C. M.

Dunoon—
Rossini, J. L. A.

Edinburgh—
Bell, L. M.
Chalmers, R. P. A.M.
McCallum, D. A. A.M.
Spence, W. L. A.M.
Steuart, A. A.M.
Thomson, W. J. M.

Girvan—
Jack, R. W. A.M.

Glasgow—
Bertram, W. R. A.M.
Bonn, C. R. H. A.M.
Boyd, J. M. G.
Carlaw, D. M.
Carlaw, R. H. A.M.
Carter, W. G. A.M.
Chapman, M. A.
Cormack, Prof. J. D. M.
Crowther, C. A.M.
Cumming, H. B. G.
Cutbush, G. H. A.M.
Daniels, W. A.M.
Don, G. A.M.
Donaldson, A. M.
Dunsmuir, A. G. A.M.

Glasgow—*contd.*
Fulton, N. O. M.
Gray, Gordon A.M.
Hannah, W. J. A.M.
Henderson, J. F. M.
Holloway, R. A.M.
Keachie, D. M.
King, W. R. A.M.
MacDonald, G. M.
Maclean, M. A.M.
Martin, J. A. A.M.
Martin, T. A.M.
May, N. A. A.M.
McCance, Dr. A. M.
McCaw, G. E. M.
McIntosh, D. C. A.M.
Millar, Jackson A.M.
Mills, J. W. M.
Muirhead, D. P. A.M.
Napier, R. C. G.
Niven, A. M. A.M.
Ovenstone, J. K. A.M.
Packer, C. A. A.M.
Parkes, J. D. A.M.
Prosser, H. A.
Rankin, J. A.M.
Rushbrook, S. R. G.
Shaylow, S. E. A.M.
Smith, R. J. A.
Sommers, T. B. M.
Sumner, F. A.M.
Tait, J. J. A.M.
Toby, A. I. A.M.
Watt, J. M.
Webster, T. L. M.

Gourock—
McMillan, A. L. A.M.

Hawick—
Russell, J. A.M.

Inverness—
Donald, R. M.
Mackenzie, James A.M.

Johnstone—
Husband, J. A.M.

Kilmarnock—
Hanman, J. L. G.

Kirkcudbright—
Neale, F. A.M.
Pullinger, C. P. G.
Pullinger, (Miss) D. A.M.
Twells, J. A.M.

Scotland—*continued.*

Leith—
Cook, A. E. A.M.

Newport (Fife)—
Lamb, J. C. A.M.

Paisley—
Muir, W. G. G.
Shaw, B. J. A. M.

Perth—
Ferguson, P. A.M.
Shaw, R. S. A.M.

Sandilands—
Dewar, P. A.M.

Stirling—
Alston, C. H. T. M.

Stranraer—
Adams, J. A.M.

Wishaw—
Buchanan, W. M. A.M.
Costley, J. P. A.M.

WALES.

Bridgend—
Rhys, W. D. G.

Cardiff—
Deverson, F. C. A.M.
Gaccon, F. A.M.
Marshall, Major John A.M.
Southern, T. E. G.
Thomas, J. Aff.

Garnant—
Beanland, H. A.M.

Llandudno—
Browne, G. W. M.
Deacon, A. E. A.M.
Wilkes, F. A. A.

Mold—
Evans, T. H. G.

Monmouth—
Webb, Geo. A.M.

Newport—
Williams, P. A. A.M.

Prestatyn—
Buckley, F. M.

Swansea—
Bennett, A. B. G.
Davies, W. D. A.M.
Thomas, F. A. A.M.

Foreign and Colonial.

AFRICA.

Cape Province—

Cape Town.
Coningsby, H. R. A.M.

Grahamstown.
Greaves, A. I. G.
Waddington, E. S. G.

Egypt—

Alexandria.
Donohue, Col. W. E. M.
Samson, A. O. A.M.

Cairo.
Montague, Capt. G. M.
Nesbitt, Major E. A. M.

Egypt—*contd.*

Khartoum.
Rhodes, J. A.M.

Rhodesia—

Salisbury.
Chatfield, W. T. A.M.

Transvaal—

Pretoria.
McFarlane, D. A.M.

Johannesburg.
Anderson, D. G.

Uganda—
Hill, Capt. R. B. A.M.

AMERICA.

NORTH AMERICA.

British North America—

Toronto.
Johnson, F. E. H. A.M.
Mortimer, J. B. A.M.

United States—

Akron.
Hargraves, A. A.M.

Ardmon.
Bachman, B. B. M.

Avenel.
Myers, C. T. M.

Beloit.
Creager, F. L. A.M.

Bethlehem.
Clausen, H. C. A.M.

Bridgport.
Ainsworth, Lt. B. W. M.

Buffalo.
Brewer, Capt. R. W. A. M.

Cambridge.
White, P. A.M.

Cleveland.
Jehle, F. A.M.
Newsome, H. H. M.

NORTH AMERICA—*contd.*

United States—*contd.*

Cleveland—contd.
Pomeroy, L. H. M.
Strickland, W. R. M.
Wooler, E. G.
Younger, J. M.

Dayton.
Heron, S. D. A.M.
Hunt, J. H. M.

Detroit.
Alden, H. W. M.
Bull, A. A. A.M.
Coyne, J. H. M.
Hunt, O. E. M.
Kellar, W. S. M.
Utz, J. G. M.
Vincent, J. G. M.

Elyria.
Edwards, H. J. M.

Fairfield.
Riker, A. L. M.

Hartford.
McMurtry, A. L. M.

Indianapolis.
Marmon, Howard Hon. M.
White, D. O. McC. M.

America—*continued.*

NORTH AMERICA.—*contd.*

United States—*contd.*

Lansing.
Jack, R. K. A.M.

Long Island City.
Clayden, A. L. M.

Maywood.
Hughes, V. G.

New York City.
Church, H. D. M.
Davies, W. S. G.
Dunham, G. W. M.
Jannay, R. M.
Landau, D. M.
Masury, A. F. M.

Paterson.
Mead, G. J. A.M.

Peoria.
Gotshall, R. W. A.M.

Pittsburgh.
Zwally, E. L. A.M.

Racine.
Batenburg, P. J. F. M.

Springfield.
Burton, R. B. A.M.

NORTH AMERICA.—*contd.*

United States—*contd.*

Toledo.
Boor, A. A.M.

Watervliet.
Armstrong, P. A. E. A.M.

Wilkes-Barre.
Laycock, A. M. M.

Wilkinsburg.
Dicksee, C. B. M.
Lessells, J. M. A.M.

Wyoming.
Zeiher, A. G.

SOUTH AMERICA.

Brazil.
Hardwicke, J. A. W. A.M.

Argentine.
Lawrence, R. M. A.M.

Chili.
Carey, C. M. A.M.
Travers, J. L. A.M.

ASIA.

Burma—
Carty, S. W. M.

Ceylon—
Gray, J. M.

China—

Peking.
Wright, T. H. M.

Shanghai.
Surman, E. J. A.M.

India—

Bombay.
Arthur, E. J. A.
Borthwick, T. A. M.
Hyland, A. A.M.
Smith, Lt. E. Seton A.M.
Stewart, P. A. N. M.
Williams, J. A.M.

Calcutta.
Chambers, H. J. A.M.
Earp, Lt. A. C. M.

India—*contd.*

Calcutta—contd.
Jackson, P. L. A.M.
Porter, S. H. A.M.
Ray, M. G.

Dehra Dun.
Groves, Lt. R. A.M.

Johdpur.
Forbes, V. A. A.M.

Karachi.
Wyse, M. N. A.M.

Kathiawar.
Mirza, P. D. A.M.

Madras.
Green, H. S. A.M.

Mysore.
Morgan, F. J. A.M.
Simonelli, L. A.M.

Nagpur.
Green, A. A.M.

Asia—*continued.*

India—*contd.*

Poona.
Brearley, J. W. A.M.

Presy.
Cornfield, M. A.M.

Rangoon.
Holliday, H. O. A.M.
Marris, R. W. A.M.

India—*contd.*

Sylhet.
Deeman, F. G.
Gibbard, T. G.

Straits Settlements—

Singapore.
Cartwright, D. G.

AUSTRALASIA.

New South Wales—

Rockdale.
Taylor, B. G. A.M.

Sydney.
Broadsmith, H. E. A.M.
Dobson, C. O. A.M.
Hamilton, Major J. A.M.
Instone, A. C. M.

Victoria—

Melbourne.
Boult, E. F. A.M.
Burnside, F. M. M.
Hamilton-Grapes, G. A.M.

West Australia—

Perth.
Shave, P. A.M.

New Zealand—

Auckland.
Allen, Capt. A. D. A.

Christchurch.
Waterston, R. G.

Wellington.
Baxter, L. E. A.M.
Drake, L. S. A.M.
Jackson, E. W. A.M.
* Johnson, T. N. A.M.
Ross, A. H. A.M.

EUROPE.

France—

Cannes.
Napier, M. S. M.

Paris.
Bollack, L. G.
Parsons, N. W. A.M.
Perrot, Henri M.
Petard, Rene M.
Sinstadt, J. A.M.
Waterlow, C. B. A.M.

Rouen.
Craig, Capt. A. E. S. M.
Partridge, Major E. H. M.

Suresnes.
Stevens, H. C. M. M.

Wimereux.
Flatman, L. G.

Germany—

Coblentz.
Boyd, J. H. M.

Essen-Ruhr.
Grenfell, E. M.

Greece—

Athens.
Zannos, J. C. A.M.

Italy—

Milan.
Durlacher, C. A.M.

Sweden—

Barkarby.
Johnson, Major C. H. A.M.

Switzerland—

Geneva.
Howland, J. G.

The Institution of Automobile Engineers.

Memorandum of Association.

1. The name of the Institution is "THE INSTITUTION OF AUTOMOBILE ENGINEERS."

2. The Registered Office of the Institution will be situate in England.

3. The objects for which the Institution is established are :—

(A) To promote the science and practice of engineering as applied to the construction of automobiles, all forms of self-propelled and mechanically-propelled vehicles, motors, and to every kind of mechanical locomotion on land, on or in water, or in air ; and to initiate and carry through any scheme or to organise any movement likely to be useful to the members of the Institution and to the community at large in relation thereto.

(B) To hold meetings of the Institution for reading and discussing communications bearing upon engineering as applied to the matters enumerated in paragraph A, or the applications thereof or upon subjects relating thereto.

(C) To enable engineers to correspond, and to facilitate the interchange of ideas respecting improvements in the various branches of the practice of engineering as applied to mechanical locomotion, and the publication and communication of information on such subjects to the members.

(D) To establish scholarships, organise lectures, hold examinations, to grant premiums and prizes for papers and essays, and by any other similar means to enlarge the knowledge and improve the practice of engineering as applied to mechanical locomotion.

(E) Subject to the provisions of the 21st Section of the Companies Act, 1862, to purchase, take, lease, exchange, hire, or otherwise acquire any real and personal property and funds, and any rights or privileges necessary or convenient for the purposes of the Institution. To construct, alter, and maintain any buildings required for the purposes of the Institution.

(F) To sell, improve, manage, develop, lease, mortgage, dispose of, turn to account, or otherwise deal with all or any part of the property of the Institution.

(G) To invest the moneys of the Institution not immediately required upon such securities or otherwise in such manner as may from time to time be determined.

(H) To borrow any moneys required for the purposes of the Institution upon such terms and in such securities as may be determined.

(I) To establish, undertake, superintend, administer, and contribute to any charitable or benevolent fund from which may be made donations or advances to persons at any time employed by the Institution, or who are or may have been engaged in engineering work and are not members of the Institution.

(J) To establish, subsidise, promote, co-operate with, receive into union, become a member of, act as or appoint trustees, agents, or delegates for control, manage, superintend, lend monetary assistance to or otherwise assist any associations and institutions incorporated or not incorporated with objects altogether or in part similar to those of the Institution, and which may prohibit the payment of dividend or profit to its members at least to as great an extent as such payment is prohibited to the members of the Institution.

(K) To establish, form, and maintain a library and collection of models, designs, drawings, and other articles of interest in connection with the development, and improvement of mechanical locomotion.

(L) To encourage the settlement of disputes by arbitration, and to act as or nominate arbitrators and umpires on such terms and in such cases as may seem expedient.

(M) To undertake and execute any trusts which may seem to the Institution conducive to any of its objects.

(N) To do all such other lawful things as are incidental or conducive to the attainment of the above objects, or any of them. Provided that the Institution shall not support with its funds, or endeavour to impose on or procure the observance by its members or others of any regulation or restriction which if an object of the Institution would make it a Trade Union. Provided also, that in case the Institution shall take or hold any property subject to the jurisdiction of the Charity Commissioners for England and Wales, the Institution shall not sell, mortgage, charge, or lease the same without such consent as may be required by law, and as regards any such property the Managers or Trustees of the Institution shall be chargeable for such property as may come into their hands, and shall be answerable and accountable for their own acts, receipts, neglects, and defaults, and for the due administration of such property in the same manner and to the same extent as they would as such Managers or Trustees have been if no incorporation had been effected, and the incorporation of the Institution shall not diminish or impair any control or authority exerciseable by the Chancery Division or the Charity Commissioners over such Managers or Trustees, but they shall, as regards any such property, be subject jointly and separately to such control or authority as if the Institution were not incorporated. In case the Institution shall take or hold any property which may be subject to any trusts, the Institution shall only deal with the same in such manner as allowed by law having regard to such trusts.

4. The income and property of the Institution, from whatever source derived, shall be applied solely towards the promotion of the objects of the Institution as set forth in this Memorandum of Association, and no portion thereof shall be paid or transferred, directly or indirectly, by way of dividend, bonus, or otherwise howsoever, by way of profit to the members of the Institution, or to any of them. Provided that nothing herein contained shall

prevent the payment in good faith of remuneration to any officers or servants of the Institution, or to any member of the Institution, or other person, in return for any services rendered to the Institution, or prevent the giving of privileges to any member of the Institution, whether a member of the Council of Management or other Governing Body or not, in attending the meetings of the Institution, or prevent the borrowing of money (under such powers as the Institution and the Council thereof may possess) from or giving security to any member of the Institution, whether a member of the Council of Management or other Governing Body or not, at a rate of interest not greater than five per cent. per annum. Provided further, that no member of the Council of Management or Governing Body of the Institution shall be appointed to any salaried office of the Institution, or any office of the Institution paid by fees, and that no remuneration shall be given to any member of such Council or Governing Body except repayment of out-of-pocket expenses and interest on money lent, or rent for premises demised to the Institution. Provided further, that this provision shall not apply to any payment to any Railway, Gas, Electric Lighting, Water, Cable, or Telephone Company, of which a member of the Council of Management or Governing Body may be a member, or any other Company in which such member shall not hold more than one-hundredth part of the capital, and such member shall not be bound to account for any share of profits he may receive in respect of such payment.

5. The fourth paragraph of this Memorandum is a condition on which a licence is granted by the Board of Trade to the Institution in pursuance of Section 23 of the Companies Act, 1867.

6. If any member of the Institution pays or receives any dividend, bonus, or other profit in contravention of the terms of the fourth paragraph of this Memorandum, his liability shall be unlimited.

7. Every member of the Institution undertakes to contribute to the assets of the Institution, in the event of the same being wound up during the time that he is a member, or within one year afterwards, for payment of the debts and liabilities of the Institution, contracted before the time at which he ceases to be a member, and of the cost, charges, and expenses of winding up the same, and for the adjustment of the rights of the contributories amongst themselves, such amount as may be required, not exceeding one pound,

or, in case of his liability becoming unlimited, such other amount as may be required, in pursuance of the last preceding paragraph of this Memorandum.

8. If upon the winding up or dissolution of the Institution there remains, after the satisfaction of all its debts and liabilities, any property whatsoever, the same shall not be paid to or distributed among the persons included in the membership of the Institution, but so far as effect can be given to the following provision, shall be given or transferred to some other institution or institutions having objects similar to the objects of the Institution, to be determined by the persons included in the membership of the Institution at or before the time of dissolution, or in default thereof by such Judge of the High Court of Justice as may have or acquire jurisdiction in the matter, and so far as effect cannot be given to such provision, then to some charitable object.

9. True accounts shall be kept of the sums of money received and expended by the Institution, and the matters in respect of which such receipts and expenditure takes place, and of the assets, credits, and liabilities of the Institution, in books of accounts which shall be kept at the Registered Office of the Institution, or at such other place or places as the Institution think fit, and subject to any reasonable restrictions as to the time and manner of inspecting the same that may be imposed in accordance with the regulations of the Institution for the time being shall be open to the inspection of members. Once at least in every year the accounts shall be examined, and the correctness of the statement and balance-sheet ascertained by one or more properly qualified Auditor and Auditors.

We, the several persons whose names and addresses are subscribed, are desirous of being formed into an Association in pursuance of this Memorandum of Association :—

NAMES, ADDRESSES, AND DESCRIPTIONS OF SUBSCRIBERS.

ROOKES EVELYN BELL CROMPTON,
Lt.-Colonel, R.E. and E.E.,
Thriplands, Kensington Court, W., London.

DUGALD CLERK,
Consulting Engineer,
18, Southampton Buildings, Chancery Lane, W.C., London.

NAMES, ADDRESSES, AND DESCRIPTIONS OF SUBSCRIBERS.

HENRY SELBY HELE-SHAW,
Engineer,
64, Victoria Street, Westminster, S.W.

WILLIAM WORBY BEAUMONT,
Consulting Engineer,
222, Strand, London.

MERVYN O'GORMAN,
Consulting Engineer,
82, Victoria Street, Westminster, S.W.

FULWAR CECIL ASHTON COVENTRY,
Mechanical Engineer,
Upwell House, Langley Road, Slough.

TOM BOUSQUET BROWNE,
Engineer,
Westcroft Works, King Street, Hammersmith, London, W.

Dated the 6th day of July, 1907.

Witness to the above signatures,

FREDERICK GEORGE BRISTOW,
1, Albemarle Street, Piccadilly, London, W.,
Assistant Secretary.

The Institution of Automobile Engineers.

Articles of Association

1. In these regulations, unless there is something in the subject or context inconsistent therewith—

"The Institution" means the above-named Institution.

"The Council" means the members for the time being of the Council hereby constituted.

"In writing" means written or printed, or partly written or partly printed.

Words importing the singular number only include the plural number, and *vice versâ*.

Words importing persons include Corporations.

2. The subscribers to the Memorandum of Association of the Institution, and such other persons as shall be admitted to membership, in accordance with these regulations, and none others, shall be members of the Institution, and shall be entered in the register of the Institution accordingly.

CONSTITUTION.

3. For the purposes of Registration the number of persons included in the membership of the Institution is unlimited.

MEMBERSHIP.

4. The Institution shall consist of Honorary Members, Ordinary Members, Associate Members, Graduates, Associates and Affiliates, and all of whom are included in the term "membership" and members.

5. No Honorary Member, Associate Member, Graduate, Associate or Affiliate shall, by reason of being legally a Member of the Institution, within the meaning of the Companies Acts, 1862 to 1900, be entitled to any privileges other than those which by these Articles attach to the specific class of Members of the Institution to which he belongs, and wherever the term "Member" is hereinafter used without qualification, it shall be taken to mean Ordinary Member,

and exclude Honorary Members, Associate Members, Graduates, Associates, and Affiliates.

6. The Institution may admit such other persons as may be hereafter qualified and elected in that behalf as Honorary Members, Ordinary Members, Associate Members, Graduates, Associates and Affiliates respectively, but such persons shall sign such form of application as may from time to time be authorised by the Council.

7. The rights and privileges of every Honorary Member, Ordinary Member, Associate Member, Graduate, Associate and Affiliate shall be personal to himself, and shall not be transferable or transmissible by his own act, or by operation of law.

ABBREVIATED TITLES.

8. The authorised abbreviations indicating the class in the Institution to which any Honorary Member, Ordinary Member, Associate Member, or Associate belongs, shall be as follows:—For an Honorary Member, Hon. M.I.A.E.; for a Member, M.I.A.E.; for an Associate Member, A.M.I.A.E.; for an Associate, A.I.A.E.

Such abbreviations shall not be used by any Member who has resigned or been removed from the Institution, nor may they at any time be used upon any shop front, facia or sign. If used upon door or wall plates or similar places, the abbreviations shall not exceed $1\frac{1}{2}$ ins. in height.

CERTIFICATES.

9. Subject to such regulations as the Council may from time to time prescribe, the Council may issue to any Ordinary Member or Associate Member a certificate showing the class to which he belongs. Every such certificate shall be according to the form which may from time to time be approved by the Council, and shall remain the property of, and shall on demand be returned to, the Institution.

QUALIFICATIONS FOR MEMBERSHIP.

10. Candidates for admission as Ordinary Members must be persons not under twenty-five years of age, who, having occupied during a sufficient period a responsible position in connection with the practice of Engineering as applied to mechanical locomotion, or otherwise proved their thorough knowledge of the theory and practice of Automobile Engineering, may be considered by the Council qualified for election.

11. Candidates for admission as Associate Members must be

persons not under twenty-three years of age, who shall have been trained as Engineers, and who shall be able to satisfy the Council that they have subsequently been employed in the practice and science of Engineering as applied to mechanical locomotion for at least two years, and shall be actually engaged in the work of such Engineering at the time of their application for election, and are considered by the Council to be qualified for election. They may be transferred at the discretion of the Council to the class of Members.

12. Graduates shall be persons of any age who, at the time of election, are being trained as pupils to an Automobile Engineer, or are studying Engineering as applied to mechanical locomotion, or who otherwise satisfy the Council that there are special circumstances which, in the opinion of the Council, entitle them to admission.

13. Candidates for admission as Associates must be persons not under twenty-five years of age, who, by reason of their scientific attainments or their position in the Engineering industry as applied to mechanical locomotion, may be considered eligible by the Council, or persons not under the said age and not in the industry, who from the interest they take in mechanical locomotion may be considered eligible by the Council.

13A. An Affiliated Firm shall be a firm, company or individual engaged in the manufacture or sale of self-propelled vehicles or of parts or materials to be incorporated therein, or who shall be otherwise interested in the objects of the Institution.

An Affiliated Firm shall be entitled to nominate one member of their staff as an Affiliate for each payment of four guineas per annum to the funds of the Institution, provided always that such Affiliate shall be approved by the Council of the Institution.

An Affiliate shall be entitled to attend all Meetings in London and the provinces, the Summer Visit and all Visits to Works, to receive the Proceedings and all other publications, to use the Library and to all other privileges of membership except that of voting and the use after his name of the authorised abbreviations of the Institution as set out in Article 8.

An Affiliate shall not be required to pay an entrance fee.

14. The Council shall have the power to elect as Honorary Members persons who, by reason of their past services to Automobile Engineering, or by other eminent qualifications, are, in their opinion, eligible for that position.

ELECTION OF MEMBERS, &c.

15. Elections shall take place as often as may be considered desirable, and they shall be made by the Council.

16. An application for admission to the Institution shall be made according to rules laid down by the Council, and on forms to be approved by the Council; such rules or forms to be varied at the discretion of the Council.

17. The Secretary shall forward to each candidate whose application has been considered and approved by the Council an announcement of his election on such form as may be approved by the Council. On the return of this form, duly signed, the candidate shall be considered duly elected, but he will not be admitted to the Institution unless within two months of his election he shall have paid his first annual subscription. Failure to comply with this Article will nullify the election of the candidate.

CONTRIBUTIONS TO THE FUNDS.

18. Each Ordinary Member shall pay an annual subscription of three and a half guineas or such other sum as the Council may from time to time determine.

19. Each Associate Member shall pay an annual subscription of two and a half guineas or such other sum as the Council may from time to time determine.

20. Each Graduate shall pay an annual subscription of one guinea or such other sum as the Council may from time to time determine.

21. Each Associate shall pay an annual subscription of two and a half guineas or such other sum as the Council may from time to time determine.

21A. The subscriptions payable by Ordinary Members, Associate Members, Graduates and Associates of the Institution residing abroad shall be the sums provided for by Articles 18, 19, 20 and 21, according to the class of membership, or such other sum as the Council may from time to time determine.*

22. All subscriptions shall be payable in advance on the 1st January in each year, and the first subscription shall date from the 1st day of January in the year of their election.

23. In the case of Ordinary Members, Associate Members, Graduates, Associates or Affiliates elected in the last three months

* At a meeting held on 8th December, 1920, it was resolved that the subscriptions of Members residing abroad should, until further notice, be 10*s.* 6*d.* less than the above amounts, in each grade of membership.

of any year, the first subscription shall cover the year of election and the succeeding year.

24. Any Ordinary Member, Associate Member, or Associate, whose subscription is not then in arrear, may compound for the then current year and all future years by the payment of twenty-five guineas or such other amount as may be approved by the Council.*

25. Every new Ordinary Member, Associate Member, Graduate, and Associate may be required to pay an entrance fee on admission to the Institution, the amount of which shall be determined by the Council from time to time, but no payment other than the increased annual subscription shall be due on transfer from one class to another.

26. Every one admitted into the Institution, whether as Ordinary Member, Associate Member, Graduate, Associate or Affiliate, shall be considered as belonging thereto, and as such, liable to the payment of his annual subscriptions and other payments, until his name shall have been removed by the Institution from its Register.

27. No Ordinary Member, Associate Member, Graduate, Associate or Affiliate, whose contribution is six months in arrear shall be entitled to attend or take part in the meetings of the Institution, nor to receive the Institution printed papers, nor shall any such Ordinary Member, Associate Member, or Associate be entitled to vote. Any Ordinary Member, Associate Member, Graduate, Associate or Affiliate, whose contribution is two years in arrear, shall be deemed to have forfeited his claim to all privileges of the Institution, and his name may be removed from the register by order of the Council; but he shall nevertheless continue liable to pay the arrears of subscription due at the time of his name being so removed, provided always that this Article shall not be construed to compel the Council to remove any name, if they be satisfied that the same should be retained.

28. The Council may at their discretion reduce or remit the annual subscription or the arrears of the annual subscription of any Ordinary Member, Associate Member, Graduate, or Associate.

TRANSFERS.

29. An Associate Member or Graduate desirous of being transferred to a higher class shall forward to the Secretary a recommendation according to the rules laid down by the Council.

* By resolution of Council, January 12th, 1921, the Life Composition Fee was raised to £42.

Such recommendation shall be laid before the Council at their next meeting for consideration, and, if approved, the Secretary shall forward to the applicant an announcement of his transfer on such form as may be approved by the Council.

RESIGNATION AND EXPULSION.

30. Any Ordinary Member, Associate Member, Graduate, Associate or Affiliate, notifying in writing to the Secretary that he is desirous of withdrawing from the Institution shall, after the payment of all arrears (if any) then due by him, cease to be included in the membership of the Institution, and his name shall be removed forthwith by the Institution from its register.

31. If any Ordinary Member, Associate Member, Graduate, Associate or Affiliate shall leave his subscription in arrear for six months, and shall fail to pay such arrears within three months after a written application has been sent to him by the Secretary, his name may be struck off the register by the Council at any time afterwards, and he shall thereupon cease to have any rights as an ordinary Member, Associate Member, Graduate, Associate or Affiliate, but he shall nevertheless continue liable to pay the arrears of subscription due at the time of the above-mentioned application. Provided always that this regulation shall not be construed to compel the Council to remove any name, if they be satisfied that the same should be retained.

32. The Council may, by a majority of the whole of the Councillors, refuse to continue to receive the subscriptions of any Ordinary Member, Associate Member, Graduate, Associate or Affiliate, and may remove his name from the register without assigning any reason for so doing, and he shall thereupon cease to be an Ordinary Member, Associate Member, Graduate, Associate or Affiliate of the Institution, as the case may be.

COUNCIL.

33. The Council of the Institution shall be chosen from Ordinary Members only, and shall consist of thirty-six Members, including the President, the past Presidents as hereinafter mentioned, not more than four Vice-Presidents, and the Chairman and one Member of the Committee of each of the Local Centres. The number of Vice-Presidents shall be determined by the Council before their election. The President shall be elected for one year, and shall not be eligible for more than one re-election until after

an interval. The retiring President in any year shall remain a Member of the Council until the next succeeding election of Councillors after his retirement from the Chair, and thereafter for a period of two years, after which time he shall be eligible for re-election to the Council. Half the Councillors shall retire at each Annual General Meeting, but shall be eligible for re-election. Unless the Council unanimously agree otherwise, the retiring Councillors shall be chosen from those who have been longest in office, and in the case of equal seniority, by ballot. Occasional vacancies upon the Council may be filled by the Council.

33A. The Council of the Institution shall have power from time to time, if, in their opinion, it is desirable to do so, to co-opt as additional members of the Council not more than three persons, who shall be nominated by any other Society, Corporation or Body which shall have kindred interests with or further or assist the objects of the Institution. Such additional members shall hold office for such period and on such terms as may be agreed upon by the Council and the nominating body.

34. Candidates for election as Councillors shall be nominated in writing at or before the General Meeting preceding the Annual General Meeting, when the Council shall present a list of their retiring Members who are eligible, and willing, to be re-elected. Any Ordinary Member or Associate Member shall be entitled to nominate candidates. The names, business and private addresses, occupations, and honorary titles of the candidates, together with the other Institutions to which they belong, shall be printed on a Ballot List which shall be forwarded to the Ordinary Members and Associate Members at least seven days previous to the Ballot being taken. The names of those elected to fill the offices of President and Vice-President and the names of the Chairmen and of the Members of the Committees of the Local Centres elected by those Committees to represent them on the Council shall not be subject to Ballot, but the names, addresses, titles, &c. of those so elected, together with the names of the non-retiring Councillors, shall be sent to the Members with the Ballot List. An Ordinary Member or Associate Member voting *against* a candidate shall strike out the name in accordance with the instructions on the Ballot Paper as may be determined by the Council from time to time. Only those votes shall be effective which are recorded on lists reaching the Secretary by the specified date.

35. Candidates for the offices of President and Vice-President shall be nominated in writing by a Member of Council before the Council Meeting prior to that at which the election of President and Vice-Presidents takes place. They shall be elected by the Council at a meeting held prior to the date of the Annual General Meeting, and shall hold office for a period of one year from the first Ordinary General Meeting of the Session following their election.

36. The First Council of the Institution shall consist of such as shall consent to act of the undermentioned President and Councillors, who shall, save for resignation, hold office until the Annual General Meeting held in 1908. The Council so constituted may co-opt additional Councillors, provided that the number of the Council does not exceed that prescribed. Vacancies occurring between the one Annual General Meeting and the next may be filled by the Council.

Name.	Address.	Description.
Rookes Evelyn Bell Crompton (*President*).	Thriplands, Kensington Court.	Engineer.
Herbert Austin	"Denehurst," Barnt Green	Automobile Engineer.
Ernest Edw. Baguley....	Birmingham Small Arms Co., Birmingham.	Manager, Motor Department.
Willm. Worby Beaumont.	Outer Temple, 222, Strand, W.C.	Consulting Engineer.
Tom Bousquet Browne ..	Westcroft Works, King St., Hammersmith, W.	Engineer.
Dugald Clerk	19, Southampton Buildings, Chancery Lane, W.C.	Consulting Engineer.
Fulwar Cecil Ashton Coventry.	Motor Department, Great Western Railway, Slough.	Mechanical Engineer.
Alexander Craig	2, Grosvenor Rd., Coventry	Engineer.
Jas. Sidney Critchley ..	45, Great Marlborough St., Regent Street, W.	Consulting Engineer.
Robert Henry Fowler ..	Steam Plough Works, Leeds	Engineer.
Chas. Riley Garrard....	12, St. Charles Square, North Kensington, W.	Engineer.
Dr. Hy. Selby Hele-Shaw, F.R.S.	64, Victoria Street, Westminster, S.W.	Engineer.
Alick Sargeant Hill....	Boston Lodge, Coventry ..	Manufacturer.
Henry Lea	38, Bennett's Hill, Birmingham.	Consulting Engineer.
Geo. Douglas Leechman..	1, Crown Office Row, Temple, E.C.	Barrister-at-Law.

Name.	Address.	Description.
Ed. Woodward Lewis....	Willenhall House, near Coventry.	Consulting Engineer.
Fred. Wm. Lanchester ..	53, Hagley Road, Birmingham.	Consulting Engineer.
Charles Marston	Ofcot, Compton, Wolverhampton.	Manufacturer.
Percy Martin	Elmfield, Coventry.....	Mechanical Engineer.
Francis Leigh Martineau	Farnham, Surrey	Engineer.
Alfrd. Douglas Mackenzie	109, Victoria Street, Westminster, S.W.	Consulting Engineer.
John Stewart Napier....	Underwood, Paisley	Engineer.
Mervyn O'Gorman	82, Victoria Street, Westminster, S.W.	Consulting Engineer.
Thomas Charles Willis Pullinger.	32, Cranmer Street, Nottingham.	Works Manager.
Victor Riley	City Works, Coventry	Works Manager.
Archibald Sharp.....	231, Strand, W.C.....	Consulting Engineer.
David Joseph Smith	58, Compton Street, E.C...	Automobile Engineer.
Alex. Edwin Tucker	Norwich Union Chambers, Birmingham.	Consulting Chemist.
Charles Wheeler	Stores Department, G.P.O., Bedford Street, W.C.	Automobile Engineer.
Thomas Hy. Woollen ...	Clement-Talbot, Ltd.. Ladbroke Grove, North Kensington, W.	Engineer.

PROCEEDINGS, POWERS, AND DUTIES OF THE COUNCIL.

37. The Council shall direct and manage the property and affairs of the Institution, and may regulate their own procedure, except that not less than five Councillors shall form a quorum. The Council may exercise all such powers of the Institution as are not by the Companies Acts, 1862 to 1900, or any statutory modification thereof for the time being in force required, or by the Articles required or expressly directed to be exercised, or expressly declared to be exercisable by the Institution in General Meeting, or with the sanction of a resolution in General Meeting, subject nevertheless to any regulations of the Articles to the provisions of the said Acts, and to such regulations being not inconsistent with the

aforesaid regulations and provisions as may be prescribed by the Institution in General Meeting, but no regulation made by the Institution in General Meeting shall invalidate any prior act of the Council which would have been valid if such regulation had not been made. Provided that the Council may, with but not without the Authority of a resolution of the Ordinary Members, Associate Members, and Associates in General Meeting, borrow moneys for the purposes of the Institution on the security of the property of the Institution or other available security, or otherwise, at their discretion.

38. The Council shall have power to consider and decide all matters not provided for by the Articles.

39. The Council shall (in accordance with the provisions of the Memorandum of Association) at all times cause to be kept, in appropriate books, proper and sufficient accounts of the Capital, Funds, Receipts, and Expenditure of the Institution. The financial year of the Institution shall end on the 31st December in each year; and a statement of the funds of the Institution and of the receipts and expenditure during such financial year shall be made, under the direction of the Council each year, and after having been verified and signed by the Auditors and approved by the Council, shall be laid before the Annual General Meeting of the year following.

40. The Council may appoint Committees chosen from their own body, and Committees for special purposes consisting of members of the Council and Ordinary Members, Associate Members, Associates and Affiliates of the Institution and others, with such powers as the Council may prescribe. In the absence of the President the Council or Committee shall elect a chairman from among those present.

41. The President shall, *ex-officio*, be a Member of all Committees of Council.

42. The Council, when they may consider it expedient to propose the enactment of any new Article, or the alteration or repeal of any existing one, shall summon the necessary Special General Meetings of Ordinary Members and Associate Members to decide the same; and the Council are at all times bound to summon such a Special General Meeting on a requisition, in writing, of any twenty Ordinary Members, Associate Members, or Associates, specifying

the particular new Article, or the alteration of an existing one, which they recommend.

43. It shall be the duty of the Council to adopt all due means for the advancement of the Institution; to provide for properly conducting its business in all cases of emergency; and to arrange for the publication, in such a way as they may deem desirable, of the papers read at Meetings of the Institution, and discussions hereon, and of such documents as may be calculated to advance Automobile Engineering knowledge.

44. No act done by the Council, whether *ultra vires* of the Council or not, which shall have received the express or implied sanction of the Ordinary Members and Associate Members and Associates in General Meeting, shall be afterwards impeached by any person included in the membership of the Institution on any ground whatever, but shall be deemed to be an act of the Institution, provided that though *ultra vires* of the Council it be within the powers of the Institution.

APPOINTMENT AND DUTIES OF OFFICERS.

45. The Treasurer of the Institution shall be appointed as and when a vacancy occurs by the Council, and shall be removable by the Council upon one month's notice from any day, but in the case of serious negligence or misconduct, may be dismissed without notice. The Treasurer shall give one month's notice in the event of his wishing to resign.

46. The Treasurer shall hold and be responsible for the uninvested funds of the Institution; shall keep all the accounts necessary and proper for the purposes of the Institution; shall from time to time submit financial statements at the request of the Council; and shall pay all moneys into a bank approved by the Council upon receipt.

47. The Secretary of the Institution shall be appointed by the Council.

48. It shall be the duty of the Secretary in person or by deputy, under the direction of the Council, to conduct the correspondence of the Institution; to attend all meetings of the Institution, and of the Council; to take minutes of the proceedings of such meetings; to superintend the publication of such papers as the Council may direct; to have charge of the Library; and to conduct the collec-

tion of all subscriptions. He shall also engage and be responsible for all persons employed under him. He shall conduct the ordinary business of the Institution in accordance with these Articles and the directions of the President and Council.

49. The Auditors shall be elected annually by the Annual General Meeting, and shall be eligible for re-election on the expiration of their year of office. The provisions of the Companies Act, 1900, as to audit and auditors shall apply and be observed by the Institution.

GENERAL MEETINGS.

50. The General Meetings shall consist of the Ordinary Meetings, the Annual General Meeting, and of Special Meetings as hereinafter defined.

51. The Annual General Meeting shall take place in London in one of the first four months of every year. The Ordinary Meetings shall take place at such times and places as the Council shall determine.

52. A Special Meeting may be convened at any time by the Council, and shall be convened by them whenever a requisition signed by twenty Ordinary Members or Associate Members of the Institution, specifying the object of the Meeting, is left with the Secretary. If for fourteen days after the delivery of such requisition a Meeting be not convened in accordance therewith, the requisitionists or any twenty Ordinary Members or Associate Members of the Institution may convene a Special Meeting in accordance with the requisition. All Special Meetings shall be held in London.

53. The quorum of the Annual General Meeting or of a Special Meeting shall be five. In the event of the quorum not being formed within half an hour of the time announced for the commencement of the Meeting, the Council shall be empowered to deal with the business of the Meeting.

54. Seven clear days' notice of every Meeting, specifying generally the nature of any special business to be transacted at any Meeting, shall be given to every person on the Register of the Institution, except as provided by Article 69, and no other special business shall be transacted at such Meeting; but the non-receipt of such notice shall not invalidate the proceedings of such Meeting. No notice of the business to be transacted (other than such ballot

lists as may be requisite in case of elections) shall be required in the absence of special business.

55. Special business shall include all business for transaction at a Special Meeting, and all business for transaction at every other Meeting, with the exception of the reading and confirmation of the Minutes of the previous Meeting, the election of Members, Associate Members, Graduates, Associates and Affiliates, and the reading and discussion of communications and papers.

56. No one entered on the Register other than Ordinary Members, Associate Members, or Associates shall have the right to vote at any Meeting of the Institution. Each Ordinary Member, Associate Member, and Associate shall have one vote. Honorary Members, Graduates and Affiliates shall not have votes at Meetings of the Institution.

LOCAL SECTIONS.

57. The Council may, at their discretion, upon receipt of a request to that effect from a sufficient number of Honorary Members, Ordinary Members, Associate Members, Associates or Affiliates, resident in any district, create a Local Section of the Institution in such district, and the Council shall also have power to dissolve such Section at any time after it has been formed.

58. Each Local Section shall be constituted and its affairs shall be carried on in accordance with rules and regulations laid down from time to time by the Council.

59. The Council may, subject to ratification at a General Special Meeting convened for the purpose, arrange for the union, alliance, or incorporation with the Institution of any Society with kindred objects; and may also, if they think fit, remit or reduce the entrance fees of the Members of such Society at the time of union, alliance, or incorporation.

PROPERTY AND FUNDS.

60. The property and funds of the Institution, and generally all its personal estate and all its real estate (if any), may be sold or disposed of by or according to the order and direction of the Council, but only if and provided that it be sanctioned by a Special General Meeting of Ordinary Members, Associate Members, and Associates.

61. Any donation may be accepted by the Council and Treasurer in aid of the funds of the Institution.

62. All the moneys of the Institution in excess of such current balance in the hands of the Treasurer as the Council shall from time to time require the Treasurer to keep in hand to meet the current expenses of the Institution, shall be invested in any mode in which Trustees are or shall be by law, in absence of special direction, authorised to invest trust moneys under their control.

63. Every paper presented to the Institution, and accepted for reading or for publication in the "Proceedings," and the copyright thereof, shall be the property of the Institution, unless there shall have been some previous arrangement to the contrary. But the Council, in such cases as they may think fit, shall have power to release or surrender their rights in respect of any such paper or the copyright thereof.

64. Each Councillor shall be accountable in respect of his own acts only, and shall not be accountable for any acts done or authorised to which he shall not have expressly assented. No Councillor shall incur any personal liability in respect of any loss or damage incurred through any act, matter, or thing done, authorised, or suffered by him, being done in good faith for the benefit of the Institution, if believed by him to be within although in excess of his legal power.

COMMON SEAL.

65. The Council may provide a Common Seal of the Institution, and make rules for the safe custody of the same, and for the use thereof.

66. The Institution, acting by the Council, may exercise all the powers given by the Companies' Seals Act, 1864.

NOTICES.

67. A notice may be served by the Council or Secretary of the Institution upon any Honorary Member, Ordinary Member, Associate Member, Graduate, Associate or Affiliate, either personally or by sending it through the post in a prepaid letter addressed to such Honorary Member, Ordinary Member, Associate Member, Graduate, Associate or Affiliate, at his address, as registered in the books of the Institution.

68. Any notice, if served by post, shall be deemed to have been served at the time when the letter containing the same would be

delivered in the ordinary course of the post; and in proving such service it shall be sufficient to prove that the letter containing the notice was properly addressed and put into the Post Office.

69. No Honorary Member, Ordinary Member, Associate Member, Graduate, Associate or Affiliate, not having a registered address within the United Kingdom, shall be entitled to any notice; and all proceedings may be had and taken without notice to such Honorary Member, Ordinary Member, Associate Member, Graduate, Associate or Affiliate, in the same manner as if he had had due notice.

NAMES, ADDRESSES, AND DESCRIPTIONS OF SUBSCRIBERS.

ROOKES EVELYN BELL CROMPTON,
Engineer, Lt.-Colonel, R.E. and E.E.,
Thriplands, Kensington Court.

DUGALD CLERK,
Consulting Engineer,
18, Southampton Buildings, Chancery Lane, W.C., London.

HENRY SELBY HELE-SHAW,
Engineer,
64, Victoria Street, Westminster, S.W.

WILLIAM WORBY BEAUMONT,
Consulting Engineer,
222, Strand, London.

MERVYN O'GORMAN,
Consulting Engineer,
82, Victoria Street, Westminster, S.W.

FULWAR CECIL ASHTON COVENTRY,
Mechanical Engineer,
Upwell House, Langley Road, Slough.

TOM BOUSQUET BROWNE,
Engineer,
Westcroft Works, King Street, Hammersmith, London, W.

Dated the 6th day of July, 1907.

Witness to the above signatures,

FREDERICK GEORGE BRISTOW,
1, Albemarle Street, Piccadilly, London, W.
Assistant Secretary.

NOTE.—In accordance with a Resolution of Council dated December 14th, 1910, members of the American Society of Automobile Engineers are privileged to attend the Meetings and Visits, and to use the Library of the Institution without payment of any subscription.

[COPY.]

No. 94216.

Certificate of Incorporation.

I HEREBY CERTIFY, that the INCORPORATED INSTITUTION OF AUTOMOBILE ENGINEERS (the word "Limited" being omitted by License of the Board of Trade) is this day incorporated under the Companies' Acts, 1862 to 1900, and that the Company is Limited.

Given under my hand at London this seventeenth day of July, One Thousand Nine Hundred and Seven.

Fees and Deed Stamps, £21 12*s.* 6*d.*
Stamp Duty on Capital, £——.

(Signed)
H. F. BARTLETT,
Registrar of Joint Stock Companies.

[Copy]

Certificate of Change of Name.

I Hereby Certify that the INCORPORATED INSTITUTION OF AUTOMOBILE ENGINEERS (the word "Limited" being omitted by License of the Board of Trade) having with the sanction of a Special Resolution of the said Company and with the approval of the Board of Trade changed its name, is now called the

INSTITUTION OF AUTOMOBILE ENGINEERS

and I have entered such new name on the Register accordingly.

Given under my hand at London this fifteenth day of September, one thousand nine hundred and eleven.

(Signed)

H. BIRTLES,

Assistant Registrar of Joint Stock Companies.

INDEX TO PROCEEDINGS.

1906 TO 1921.

AUTHORS.

Author.	Subject of Paper.	Proceedings.		
		Session.	Vol.	Page.
Acres, F. A. S.	Inertia Torque in Crankshafts ..	1919–20	XIV.	447
Ainsworth, B. W.	The Training of Automobile Engineers.	1912–13	VII.	131
Aitchison, Leslie	Valve Failures and Valve Steels in Internal Combustion Engines.	1919–20	XIV.	31
Bailey, R. W. Harvey	The Front Driving of Steam and Petrol Vehicles.	1907–8	II.	121
Bagnall-Wild, Lt.-Col. R. K., and Birch, Lieut. E.W.	The Use and Abuse of Steel	1916–17	XI.	307
Bagnall-Wild, Lt.-Col. R. K.	Presidential Address	1917–18	XII.	4
Bagnall-Wild, Brig.-Gen. R. K.	Some Notes on the Metallurgy of Steel.	1917–18	XII.	489
Baillie, G. H.	Springs	1912–13	VII.	451
Ballantyne, Horatio	Some Chemical Notes on Petrol..	1908–9	III.	293
Barrett, G. F.	Causes of Failure in Ball Bearings	1911–12	VI.	195
Berriman, A. E.	An Analysis of Test Records of some Petrol Engines.	1918–19	XIII.	83
Bircham, F. R. S.	Some Points in the Design and Use of Small Internal Combustion-Engined Boats.	1908–9	III.	159
Blount, Bertram	The Composition of Commercial Petrols.	1908–9	III.	301
Bone, Prof. W. A.	The Combustion of Hydrocarbons	1906–7	I.	7
Bramley-Moore, Capt. S.	Suggested Lines of Development in Transmission.	1920–21	XV.	261
Brown, Capt. G. W. A.	The Lubrication of Motor Cars..	1918–19	XIII.	299
Browne, T. B.	Some Points affecting the Design of Petrol Motor Vans.	1909–10	IV.	167
Browne, T. B.	The London Motor Omnibus Service.	1912–13	VII.	556
Browne, T. B.	The Progress of Automobilism ..	1912–13	VII.	3
Burchall, H.	The Training of Automobile Engineers.	1912–13	VII.	151
Burchall, H.	Retail Transport	1914–15	IX.	43

Author.	Subject of Paper.	Proceedings.		
		Session.	Vol.	Page.
Burls, G. A.	Proposal for a Maximum Power Rating Formula.	1910–11	V.	186
Callendar, Prof. H. L.	The Effect of Size on the Thermal Efficiency of Motors.	1906–7	I.	223
Campbell-Taylor, G..	The Training of Automobile Engineers.	1912–13	VII.	131
Caudwell, E.	The Engine of the Sidecar Motor-cycle.	1918-19	XIII.	345
Chorlton, A. E. L. ..	An Agricultural Power Unit....	1917–18	XII.	71
Clark, A. Graham ..	Aeronautical Engines	1912–13	VII.	63
Clarkson, Thos.....	Steam Omnibus.....	1912–13	VII.	577
Clarkson, Thos.....	The Possibilities of Further Economies in Road Transport.	1919-20	XIV.	5
Clarkson, Thos.	Wheels for Commercial Motors ..	1914-15	IX.	105
Clayden, A. Ludlow..	The Economics of Design	1913–14	VIII.	103
Clayden, A. Ludlow..	Retail Transport	1914–15	IX.	43
Clayden, A. Ludlow..	Electrical Equipment of a Car ..	1916-17	XI.	33
Clerk, Dugald	The Principles of Carburetting as determined by Exhaust Gas Analysis.	1907–8	II.	47
Clerk, Dugald	President's Address. Some Problems of the Motor Car.	1908–9	III.	3
Clerk, Dugald	Note on Report of the Horse-Power Formula Committee.	1910–11	V.	181
Coffin, Howard E. ..	American Tendencies in Motor Car Engineering.	1911–12	VI.	23
Coker, Prof. E. G....	Photo-Elasticity for Engineers ..	1917–18	XII.	31
Craig, A.	Valves and Valve Gearing	1909-10	IV.	269
Critchley, J. S.....	Résumé of Front Drive Motor Patents.	1907–8	II.	138
Critchley, J. S.....	Motors for Aerial Navigation ...	1909–10	IV.	23
Critchley, J. S.....	Presidential Address. The Fuel Question.	1913–14	VIII.	5
Crompton, Col. R. E..	President's Address. The Future of Automobilism.	1907–8	II.	3
Crompton, Col. R. E..	Notes on Wheel Diameters.....	1907–8	II.	319
Crompton, Col. R. E...	The Wheel and the Road.....	1912–13	VII.	403
Crompton, Col. R. E...	Notes on Screw Gauges	1916-17	XI.	173
rompton, Col. R. E..	Roads and Vehicle Maintenance..	1920-21	XV.	167

Author	Subject of Paper.	Proceedings.		
		Session.	Vol.	Page.
Day, Chas.	Diesel Engines	1918–19	XIII.	471
de Havilland, G.	Aeroplane Undercarriages	1911–12	VI.	289
Dickenson, J. H. S...	The Choice of Steel for Use in Automobile Construction.	1915–16	X.	3
Dickenson, J. H. S...	Some Notes on the Report of the Steel Research Committee.	1920–21	XV.	99
Duffield, E. N......	Car Design and Car Usage.....	1919–20	XIV.	121
Fenning, R. W.	Efficiency of a Two-Cycle Motor.	1910-11	V.	83
Fowler, Sir H......	Some Notes on Production	1920–21	XV.	6
Gibson, A. H......	The Air Cooling of Petrol Engines.	1919–20	XIV.	243 & 715
Gibson, W. H. H. ...	A Diagram of the Thermal Properties of Gas Mixtures.	1919–20	XIV.	657
Gibson, W. H. H. ...	A Diagram for the Calculation of Nozzles.	1919–20	XIV.	657
Gilchrist, Wm......	The Motor Car considered as a Carriage.	1906–7	I.	305
Goodman, Prof. J. ..	Some Points in the Design of Ball and Roller Bearings.	1913–14	VIII.	107
Groom, Prof. P.	The Artificial Seasoning of Wood	1917–18	XII.	125
Guest, J. J.	Grinding	1911–12	VI.	327
Hatfield, W. H.	The Most Suitable Steels for Automobile Parts.	1919–20	XIV.	503 & 731
Hatfield, W. H.	Further Notes on Automobile Steels.	1920-21	XV.	465
Heathcote, H. L.....	The Ball Bearing, in the Making, under Test and on Service.	1920–21	XV.	569
Heather, D. S......	A Survey of Current Motorcycle Design.	1918–19	XIII.	247
Heather, D. S......	Comparative Motorcycle Performances.	1920–21	XV.	839
Hele-Shaw, Dr. H. S.	Front Driving, with special reference to Electric and Hydraulic Transmission.	1907–8	II.	107
Hele-Shaw, Dr. H. S.	President's Address—Aerial Automobilism.	1909–10	IV.	3
Henderson, J. F.	Magneto Ignition	1914–15	IX.	443
Hill, A. S......	Chains for Power Transmission ..	1909–10	IV.	307
Hoblyn, J. B.	Commercial Steels and their Heat Treatment.	1917–18	XII.	373
Hodgson, J. Lawrence	Some Notes on the Fan Dynamometer.	1915–16	X.	301
Hodgson, J. Lawrence	Two Papers dealing with some Tests on Model Propellers.	1916–17	XI.	261

Author.	Subject of Paper.	Proceedings.		
		Session.	Vol.	Page.
Hopkinson, Prof. Bertram.	The Effect of Size and Speed upon the Performance of an Internal Combustion Engine.	1908–9	III.	211
Horse-Power Formulæ Committee of I.A.E.	The Rating of Petrol Engines ..	1908–9	III.	37
Ditto.	ditto.	1910-11	V.	168
Hounsfield, L. H. ..	Appendix to The Wheel and the Road.	1912–13	VII.	419
House, Chas. E. G. ..	The Training of Automobile Engineers.	1912–13	VII.	131
Kennedy, D. S.....	Heavy Motor Vehicles	1912–13	VII.	311
Lake, W. B.....	Steel Castings used in Automobile Construction.	1917–18	XII.	469
Lanchester, F. W. ..	The Horse Power of the Petrol Motor in its relation to Bore, Stroke and Weight.	1906–7	I.	155
Lanchester, F. W. ..	Some Problems peculiar to the Design of the Automobile.	1907–8	II.	187
Lanchester, F. W. ..	Tractive Effort and Acceleration of Automobile Vehicles on Land, Air and Water.	1909-10	IV.	123
Lanchester, F. W. ..	Presidential Address. Factors that have contributed to the Advance of Automobile Engineering.	1910-11	V.	3
Lanchester, F. W. ..	Worm-Gearing	1912–13	VII.	215
Lanchester, F. W. ..	Engine Balancing	1913–14	VIII.	195
Lanchester, F. W. ..	The Flying Machine—The Aerofoil in the Light of Theory and Experiment.	1914-15	IX.	171
Lanchester, F. W. ..	The Screw Propeller	1914–15	IX.	263
Lanchester, F. W. ..	The Cylinder Cooling of Internal Combustion Engines.	1915–16	X.	59
Lanchester, F. W. ..	Worm Gear and Worm Gear Mounting.	1916–17	XI.	79
Larard, C. E.	Castellated Shafts.....	1910-11	V.	133
Leechman, G. D.	Efficient Invention with special reference to Patents affected by the War.	1918–19	XIII.	207
Leechman, G. D.	Motor Bicycle Frame Design....	1920–21	XV.	219
Legros, L. A.	The Transmission of Power from the Engine to the Road Wheels in Motor Vehicles.	1908-9	III.	335
Legros, L. A.	The Use of Pressed Steel in Automobile Construction.	1910-11	V.	347

Author.	Subject of Paper.	Proceedings.		
		Session.	Vol.	Page.
Legros, L. A.	Presidential Address. The Influence of Detail on the Development of the Automobile.	1911–12	VI.	1
Legros, L. A.	Presidential Address	1916–17	XI.	3
Low, Dr. A. M.	Some Experiments and their Bearing upon the Design of Automobile Parts.	1913–14	VIII.	331
Macbeth, Colin.....	The Design and Manufacture of Pneumatic Motor Tyres.	1920–21	XV.	379
Mackenzie, Douglas..	The Effect of Motors on Roads ..	1907–8	II.	370
Mallock, A.	Wheel and Road	1910–11	V.	309
Markham, R. G. L...	Importance of Design in Motor Engines for Marine Work.	1911–12	VI.	417
Martin, Percy	Works Organisation.....	1906–7	I.	109
Martineau, F. L.	Accessibility and Cleanliness, and the best means of attaining them.	1906–7	I.	39
Martineau, F. L.....	Hydraulic Transmission	1916–17	XI.	223
Martineau, F. L.....	Nomography, with special reference to its Use in Engine Design.	1917–18	XII.	187
Mellanby, Dr. A. L. .	The Engineer of the Future	1917–18	XII.	445
Morcom, R. K......	Forced Lubrication	1909–10	IV.	135
Morgan, Prof. W. ..	Carburettor Action	1910–11	V.	37
Morgan, Prof. W. ..	Paraffin	1911–12	VI.	371
Morgan, Prof. W. ..	The Measurement of Horse-Power	1912–13	VII.	530
Morse, L. G. E.	The Mechanical and Thermal Efficiency of a Petrol Engine.	1908–9	III.	274
Napier, John S.	A System of Costing, Book-keeping and Recording, suitable for a Motor or other Engineering Factory.	1907–8	II.	261
O'Gorman, Mervyn..	The Weight of Motor Cars and Motor Car Parts.	1908–9	III.	107
O'Gorman, Mervyn ..	Aircraft and the Problems Relating thereto.	1910–11	V.	257
Opperman, C. T.	Electric Vehicles	1918–19	XIII.	395
Ormandy, Dr. W. R..	Some Experiments on Mixed Fuel, with especial reference to Alcohol Mixtures.	1913–14	VIII.	49
Ormandy, Dr. W. R..	Engine Lubricating Oils.....	1913–14	VIII.	371

Author.	Subject of Paper.	Proceedings.		
		Session.	Vol.	Page.
Ormandy, Dr. W. R..	Why Petrol must go Higher in Price.	1919–20	XIV.	647
Palmer, L. S.....	The Combustion of Naphthalene Solutions in Internal Combustion Engines.	1920–21	XV.	195
Philpott, Capt. H. P.	Some Experiments on Notched Bars.	1917–18	XII.	235
Pomeroy, L. H.	Engine Design for taking advantage of H.P. Rating Rules.	1911–12	VI.	73
Pomeroy, L. H.	Automobile Engineering and the War.	1914–15	IX.	5
Pomeroy, L. H.	Automobile Manufacturing and the War.	1914-15	IX.	397
Pomeroy, L. H.	Elements of High-Speed Automobile Engine Design.	1913–14	VIII.	407
Pomeroy, L. H.	Influence of Valve Area, Revolution Speed and Combustion Chamber Design upon Horse-Power and Thermal Efficiency.	1918–19	XIII.	163
Prior, D. L.	Epicyclic Gearing in Automobile Practice.	1920–21	XV.	887
Reeves, A. W.	The Two-Stroke Motor for Motor Vehicle Propulsion.	1913–14	VIII.	435
Reeves, A. W., and Kimber, Cecil.	Works Organisation.....	1916–17	XI.	371
Remington, A. A. ..	Presidential Address	1918–19	XIII.	1
Ricardo, H. R.....	Some Experiments on Supercharging in a High-speed Engine.	1920–21	XV.	717
Rosenhain, Walter ..	The Strength and Structure of Alloys.	1906–7	I.	71
Saunderson, H. P. ..	Motor Tractors	1917–18	XII.	435
Savage, E. A.	The Utilization of the data of the Automobile Industry through Bureaux of Information.	1917–18	XII.	451
Savage, G. H.	The Springing of Motorcycles....	1920–21	XV.	35
Schofield, H.	Test of a Daimler Sleeve-Valve Engine.	1912-13	VII.	35
Sharp, A.	Pneumatic Springs for Road Vehicles.	1909-10	IV.	87
Shilson, Major B. W.	From Engine to Axle	1915–16	X.	333
Smith, D. J.....	The Repair of Motor Vehicles ..	1911–12	VI.	133
Smith, D. J.	Producer Gas for Motor Vehicles.	1919–20	XIV.	169 & 709
Smith-Clarke, Capt. G. T.	Some Notes on Petrol Flowmeters and the Calibration of Carburettor Jets.	1918–19	XIII.	437
Spence, W. L.	A Suggested Design for the £100 Car.	1914-15	IX.	483
Stewart, J.	Producer Gas Practice.....	1920-21	XV.	813
Stubbs, A.	Drop Forging and the Automobile Industry.	1914–15	IX.	35

Author.	Subject of Paper.	Proceedings. Session.	Vol.	Page.
Taylor, W. J.	The Effect of Motors on Roads ..	1907–8	II.	343
Thomas, J. S. G., and Watson, Prof. W.	The Physical Properties of some Commercial Petrols.	1908–9	III.	429
Thomas, B. H.	The Electro-Deposition of Iron as applied to Motor Vehicle Repair Work.	1919–20	XIV.	603
Tilston, E.	Two-Cycle Engines for Motor-cycles.	1918–19	XIII.	517
Warren, H.	The Operation and Design of Sparking Plugs.	1917–18	XII.	513
Warren, H.	The Process of Ignition	1919–20	XIV.	305
Watson, Geo. W.....	Heavy Motor Vehicles.....	1912–13	VII.	311
Watson, Geo. W.....	Live Axles for Commercial Vehicles.	1915–16	X.	161
Watson, Geo.....	A Suggested Programme for Automobile Research.	1920–21	XV.	511
Watson, Prof. W. ..	The Thermal and Combustion Efficiency of a Four-Cylinder Petrol Motor.	1908–9	III.	387
Watson, Prof. W. ..	Efficiency of a Two-Cycle Motor.	1910–11	V.	83
Watson, Prof. W. ..	Test of a Daimler Sleeve-Valve Engine.	1912–13	VII.	35
Watson, Prof. W. ...	Benzol, Alcohol and Mixtures of these Liquids with Petrol as Fuel for Internal Combustion Engines.	1914–15	IX.	73
Watt, James.....	Differential Gears for Commercial Vehicles.	1919–20	XIV.	697
Weir, Jas.....	Some Post-War Tendencies in Engine Design.	1919–20	XIV.	687
Wheeler, Chas.	Calculating Depreciation of Commercial Automobiles.	1912–13	VII.	527
Williams, Lieut. W. E.	Some Causes of Failure, illustrating the necessity for care in the use of Steel for Automobile Construction.	1918–19	XIII.	33
Williamson, W. D. ..	Engines for Petrol Commercial Vehicles.	1915–16	X.	223
Wilson, J.....	Test of a Daimler Sleeve-Valve Engine.	1912–13	VII.	35
Wimperis, H. E.	Accelerometer Tests.....	1913–14	VIII.	281
Wood, E. B.	Carburettor Action	1910–11	V.	37
Wood, E. B.	Paraffin	1911–12	VI.	371
Wood, E. B.	The Measurement of Horse-Power	1912–13	VII.	530
Worsley, P. J.....	Gear Cutting for Motor Cars	1919–20	XIV.	401 & 723
Young, A. P.	The Process of Ignition	1919–20	XIV.	305

SUBJECTS.

Subject of Paper.	Author.	Proceedings.		
		Session.	Vol.	Page.
Accelerometer Tests	H. E. Wimperis	1913-14	VIII.	281
Accessibility and Cleanliness, and the best means of attaining them.	F. L. Martineau....	1906–7	I.	39
Aerial Automobilism.....	Dr. H. S. Hele-Shaw	1909–10	IV.	3
Aerial Navigation, Motors for....	J. S. Critchley.....	1909-10	IV.	23
Aerofoil, The.....	F. W. Lanchester ..	1914–19	IX.	171
Aeronautical Engines	A. Graham Clark ...	1912–13	VII.	63
Aeroplane Undercarriages	G. De Havilland....	1911-12	VI.	289
Agricultural Power Unit, An	A. E. L. Chorlton ..	1917-18	XII.	71
Air Cooling of Petrol Engines, The.	A. H. Gibson	1919-20	XIV.	243 & 715
Aircraft, Problems Relating to ..	M. O'Gorman	1910-11	V.	257
Alloys, The Strength and Structure of.	W. Rosenhain.....	1906-7	I.	71
American Tendencies in Motor Car Engineering.	Howard E. Coffin ...	1911–12	VI.	23
Automobile Engineering and the War.	L. H. Pomeroy	1914–15	IX.	5
Automobile Manufacturing and the War.	L. H. Pomeroy	1914–15	IX.	397
Balancing, Engine.....	F. W. Lanchester ..	1913–14	VIII.	195
Ball and Roller Bearings, Some Points in the Design of.	Prof. J. Goodman ...	1913–14	VIII.	107
Ball Bearing, The, in the Making, under Test and on Service.	H. L. Heathcote....	1920–21	XV.	569
Ball Bearings, Causes of Failure in	G. F. Barrett	1911–12	VI.	195
Benzol, Alcohol, and Mixtures of these Liquids with Petrol as Fuel for Internal Combustion Engines.	Prof. W. Watson ..	1914–15	IX.	73
Carburetting, Principles of, as determined by Exhaust Gas Analysis, The.	Dugald Clerk	1907–8	II.	47
Carburettor Action	Prof. W. Morgan and E. B. Wood.	1910–11	V.	37

Subject of Paper.	Author.	Proceedings. Session.	Vol.	Page.
Car Design and Car Usage	Edgar N. Duffield ..	1919–20	XIV.	121
Castellated Shafts	C. E. Larard	1910–11	V.	133
Chains for Power Transmission ..	A. S. Hill	1909-10	IV.	307
Commercial Motor Vehicles, Heavy	Geo. W. Watson and D. S. Kennedy.	1912–13	VII.	311
Commercial Vehicles, Differential Gears for.	James Watt.....	1919–20	XIV.	697
Commercial Vehicles, Petrol Engines for,	W. D. Williamson ..	1915–16	X.	223
Commercial Vehicles, From Engine to Axle.	Major B. W. Shilson	1915–16	X.	333
Commercial Vehicles, Live Axles for.	Geo. W. Watson	1915–16	X.	161
Commercial Vehicles, Some Points affecting the Design of.	T. B. Browne	1909–10	IV.	167
Cooling, Cylinder, of Internal Combustion Engines, The.	F. W. Lanchester ..	1915–16	X.	59
Costing, Book-keeping and Recording, suitable for a Motor or other Engineering Factory, A System of.	John S. Napier	1907–8	II.	261
Daimler Sleeve-Valve Engine, Test of a.	Prof. W. Watson, H. Schofield, and J. Wilson.	1912–13	VII.	35
Depreciation of Commercial Automobiles, Calculating.	Chas. Wheeler.....	1912–13	VII.	527
Design, Elements of High-Speed Automobile Engine,	L. H. Pomeroy	1913–14	VIII.	407
Design, Engine, for taking advantage of H. P. Rating Rules.	L. H. Pomeroy	1911–12	VI.	73
Design for the £100 Car, A Suggested.	W. L. Spence	1914–15	IX.	483
Design, Importance of, in Motor Engines for Marine Work, The.	R. G. L. Markham ..	1911–12	VI.	417
Design, The Economics of	A. Ludlow Clayden .	1913–14	VIII.	143
Diesel Engines	Chas. Day	1918–19	XIII.	471
Drop Forging and the Automobile Industry.	A. Stubbs	1914–15	IX.	357
Dynamometer Fan, Some Notes on the.	J. L. Hodgson	1915–16	X.	301
Effect of Size and Speed upon the performance of an Internal Combustion Engine.	Prof. B. Hopkinson..	1908–9	III.	211
Effect of Size on the Thermal Efficiency of Motors, The.	Prof. H. L. Callendar	1906–7	I.	223
Electric Vehicles	C. T. Opperman	1918–19	XIII.	395
Electrical Equipment of a Car ...	A. Ludlow Clayden..	1916–17	XI.	33

Subject of Paper.	Author.	Proceedings.		
		Session.	Vol.	Page.
Electro-Deposition of Iron as applied to Motor Vehicle Repair Work, The.	B. H. Thomas.....	1919-20	XIV.	603
Engineer of the Future, The	Dr. A. L. Mellanby .	1917-18	XII.	445
Epicyclic Gearing in Automobile Practice.	D. L. Prior	1919-20	XV.	887
Factors that have contributed to the Advance of Automobile Engineering.	F. W. Lanchester...	1910-11	V.	3
Flowmeters (Petrol) and the Calibration of Carburettor Jets, Some Notes on.	Capt. G. T. Smith-Clarke.	1918-19	XIII.	437
Front Drive Motor Patents, Résumé of.	J. S. Critchley.....	1907-8	II.	138
Front Driving of Steam and Petrol Vehicles, The.	R. W. Harvey Bailey	1907-8	II.	121
Front Driving, with special reference to Electric and Hydraulic Transmission.	Dr. H. S. Hele-Shaw .	1907-8	II.	107
Fuel Question, The.....	J. S. Critchley.....	1913-14	VIII.	5
Fuel, Mixed, Some Experiments on, with especial reference to Alcohol Mixtures.	Dr. W. R. Ormandy .	1913-14	VIII.	49
Future of Automobilism, The	Col. R. E. Crompton .	1907-8	II.	3
Gear Cutting for Motor Cars	P. J. Worsley.....	1919-20	XIV.	401 & 723
Grinding	J. J. Guest	1911-12	VI.	327
H.P. Formula Committee, Note on Report of.	Dugald Clerk	1910-11	V.	181
Horse Power of the Petrol Motor in its relation to Bore, Stroke and Weight, The.	F. W. Lanchester ..	1906-7	I.	155
Horse-Power, The Measurement of	Prof. W. Morgan and E. B. Wood.	1912-13	VII.	530
Hydraulic Transmission	F. L. Martineau	1916-17	XI.	223
Hydrocarbons, The Combustion of	Prof. W. A. Bone ...	1906-7	I.	7
Ignition, The Process of	A. P. Young and H. Warren.	1919-20	XIV.	305
Inertia Torque in Crankshafts ..	F. A. S. Acres.....	1919-20	XIV.	447
Influence of Detail in the Development of the Automobile, The.	L. C. Legros	1911-12	VI.	1
Influence of Valve Area, Revolution Speed and Combustion Chamber Design upon Horse-Power and Thermal Efficiency.	L. H. Pomeroy	1918-19	XIII.	163

Subject of Paper.	Author.	Proceedings.		
		Session.	Vol.	Page.
Invention, Efficient, with special reference to Patents affected by the War.	G. D. Leechman....	1918-19	XIII.	207
Lubricating Oils, Engine.....	Dr. W. R. Ormandy.	1913-14	VIII.	371
Lubrication, Forced.....	R. K. Morcom.....	1909-10	IV.	351
Lubrication of Motor Cars, The..	Capt. G. W. A. Brown.	1918-19	XIII.	299
Magneto Ignition	J. F. Henderson	1914-15	IX.	443
Mechanical and Thermal Efficiency of a Petrol Engine, The.	L. G. E. Morse	1908-9	III.	274
Motor Car considered as a Carriage, The.	Wm. Gilchrist.....	1906-7	I.	305
Motorcycle Design, Current, A Survey of.	D. S. Heather.....	1918-19	XIII.	247
Motor Cycle Frame Design	G. D. Leechman	1920-21	XV.	219
Motor Cycle Performances, Comparative.	D. S. Heather.....	1920-21	XV.	839
Motor Cycle Sidecar, The Engine of the.	E. Caudwell	1918-19	XIII.	345
Motor Cycles, The Springing of..	G. H. Savage	1920-21	XV.	35
Motor Cycles, Two-Cycle Engines for.	E. Tilston	1918-19	XIII.	517
Motor Tractors	H. P. Saunderson ..	1917-18	XII.	435
Naphthalene Solutions, The Combustion of.	L. S. Palmer.....	1920-21	XV.	195
Nomography, with special reference to its Use in Engine Design.	F. L. Martineau....	1917-18	XII.	187
Notched Bars, some Experiments on.	Capt. H. P. Philpott.	1917-18	XII.	235
Nozzles, A Diagram for the Calculation of.	W. H. H. Gibson ..	1919-20	XIV.	657
Omnibus Service, The London Motor.	T. B. Browne	1912-13	VII.	556
Omnibuses, Steam	Thos. Clarkson	1912-13	VII.	577
Paraffin	Prof. W. Morgan and E. B. Wood.	1911-12	VI.	37
Petrol, Some Chemical Notes on..	Horatio Ballantyne .	1908-9	III.	293
Petrols, Commercial, The Composition of.	Bertram Blount	1908-9	III.	301
Petrols, Commercial, The Physical Properties of some.	J. S. G. Thomas and Prof. W. Watson.	1908-9	III.	429
Photo-Elasticity for Engineers ..	Prof. E. G. Coker ..	1917-18	XII.	31

Subject of Paper.	Author.	Proceedings.		
		Session.	Vol.	Page.
Pneumatic Motor Tyres, The Design and Manufacture of.	Colin Macbeth.....	1920-21	XV.	379
Pneumatic Springs for Road Vehicles.	A. Sharp	1909-10	IV.	87
Presidential Address	L. A. Legros	1916-17	XI.	3
Presidential Address	Lt.-Col.R.K. Bagnall-Wild.	1917-18	XII.	4
Presidential Address	A. A. Remington ..	1918-19	XIII.	1
Pressed Steel, Use of, in Automobile Construction.	L. A. Legros	1910-11	V.	347
Producer Gas for Motor Vehicles .	D. J. Smith.....	1919-20	XIV.	169 & 709
Producer Gas Practice	J. Stewart	1920-21	XV.	813
Production, Some Notes on.....	Sir H. Fowler.....	1920-21	XV.	6
Progress of Automobilism	T. B. Browne	1912-13	VII.	3
Propellers, Model, Two Papers dealing with some Tests on.	J. Lawrence Hodgson	1916-17	XI.	261
Rating Formula, Proposal for a Maximum Power.	G. A. Burls	1910-11	V.	186
Rating of Petrol Engines, The ..	Horse Power Formulæ Committee of I.A.E.	1908-9	III.	37
Do. ..	Do.	1910-11	V.	168
Repair of Motor Vehicles, The ..	D. J. Smith	1911-12	VI.	133
Research, Automobile, A Suggested Programme for.	G. Watson	1920-21	XV.	511
Roads and Vehicle Maintenance..	Col. R. E. Crompton.	1920-21	XV.	167
Roads, The Effect of Motors on..	Douglas Mackenzie .	1907-8	II.	370
Roads, The Effect of Motors on ..	W. J. Taylor	1907-8	II.	343
Screw Gauges, Notes on	Col. R. E. Crompton..	1916-17	XI.	173
Screw Propeller, The	F. W. Lanchester ..	1914-15	IX.	263
Some Experiments and their Bearing upon the Design of Automobile Parts.	Dr. A. M. Low	1913-14	VIII.	331
Some Points in the Design and Use of Small Internal Combustion-Engined Boats.	F. R. S. Bircham....	1908-9	III.	159
Some Post-War Tendencies in Engine Design.	Jas. Weir.....	1919-20	XIV.	687
Some Problems of the Motor Car..	Dugald Clerk	1908-9	III.	3

Subject of Papar.	Authors.	Percentage.		
		Session.	Vol.	Page.
Some Problems peculiar to the Design of the Automobile.	F. W. Lanchester ..	1907-8	II.	187
Sparking Plugs, The Operation and Design of.	H. Warren	1917-18	XII.	513
Springs.....	G. H. Baillie	1912-13	VII.	451
Steel Castings used in Automobile Construction.	W. B. Lake.....	1917-18	XII.	469
Steel for Use in Automobile Construction, The Choice of.	J. H. S. Dickenson ..	1915-16	X.	3
Steel Research Committee, Some Notes on the Report of.	J. H. S. Dickenson..	1920-21	XV.	99
Steel, Some Causes of Failure, illustrating the necessity for care in the use of, for Automobile Construction.	Lieut. W. E. Williams.	1918-19	XIII.	33
Steel, Some Notes on the Metallurgy of.	Brig. Gen. R. K. Bagnall-Wild, C.M.G.	1917-18	XII.	489
Steel, The Use and Abuse of	Lt.-Col. R. K. Bagnall-Wild and Lieut. E. W. Birch.	1916-17	XI.	307
Steels, Automobile, Some further Notes on.	Dr. W. H. Hatfield..	1920-21	XV.	465
Steels, Commercial, and their Heat Treatment.	J. B. Hoblyn	1917-18	XII.	373
Steels for Automobile Parts, The most Suitable.	W. H. Hatfield	1919-20	XIV.	503 & 731
Supercharging in a High-speed Engine, Some Experiments on.	H. R. Ricardo.....	1920-21	XV.	717
Test Records of some Petrol Engines, An Analysis of.	A. E. Berriman	1918-19	XIII.	83
Thermal and Combustion Efficiency of a Four-Cylinder Petrol Motor, The.	Prof. W. Watson ...	1908-9	III.	387
Thermal Properties of Gas Mixtures, A Diagram of.	W. H. H. Gibson ..	1919-20	XIV.	657
Tractive Effort and Acceleration of Automobile Vehicles on Land, Air and Water.	F. W. Lanchester ..	1909-10	IV.	123
Training of Automobile Engineers, The.	B. W. Ainsworth, H. Burchell, C. E. G. House, G. Campbell Taylor.	1912-13	VII.	131

Subject of Paper.	Authors.	Proceedings.		
		Session.	Vol.	Page.
Transmission of Power from the Engine to the Road Wheels in Motor Vehicles, The.	L. A. Legros	1908–9	III.	335
Transmission, Suggested Lines of Development in.	Capt. S. Bramley-Moore.	1920–21	XV.	261
Transport, Retail	A. Ludlow Clayden & H. Burchall.	1914–15	IX.	43
Transport, Road, The Possibilities of further Economies in.	Thos. Clarkson	1919–20	XIV.	5
Two-Cycle Motor, The Efficiency of a.	Prof. W. Watson and R. W. Fenning.	1910–11	V.	83
Two-Stroke Motor for Motor Vehicle Propulsion, The.	A. W. Reeves	1913–14	VIII.	441
Utilization of the data of the Automobile Industry through Bureaux of Information, The.	E. A. Savage	1917–18	XII.	451
Valve Failures and Valve Steels in Internal Combustion Engines.	Leslie Aitchison	1919–20	XIV.	31
Valves and Valve Gearing	A. Craig	1909–10	IV.	269
Weight of Motor Cars and Motor Car Parts, The.	Mervyn O'Gorman ..	1908–9	III.	107
Wheel and Road.....	A. Mallock	1910–11	V.	309
Wheel and the Road, The	Col. R. E. Crompton .	1912–13	VII.	403
Wheel Diameters, Notes on.....	Col. R. E. Crompton .	1907–8	II.	391
Wheels for Commercial Motors ..	T. Clarkson	1914–15	IX.	105
Why Petrol must go Higher in Price.	W. R. Ormandy	1919–20	XIV.	647
Wood, The Artificial Seasoning of	Prof. P. Groom	1917–18	XII.	125
Works Organisation	Percy Martin	1906–7	I.	109
Works Organisation	A. W. Reeves and Cecil Kimber.	1916–17	XI.	371
Worm-Gearing	F. W. Lanchester ..	1912–13	VII.	215
Worm Gear and Worm Gear Mounting.	F. W. Lanchester ..	1916–17	XI.	79

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SUBJECT MATTER INDEX.

SUBJECT MATTER INDEX.

VOLS. XI. TO XV.

Acceleration due to Valve Cams, Nomograms for, XII. 201.
..... obtained with Hydraulic Transmission, XI. 254.
Accumulators for Electric Vehicles—early type, XIII. 396.
Adhesive Resistance, XII. 103.
Agricultural Tractors, Consumption of, per acre, XII. 87, 99.
..... Efficiency of Transmission, XII. 106.
..... Factors governing design of, XII. 73.
..... Particulars of, XII. 90.
..... Power required for, XII. 437.
..... Steering of, XII. 92.
..... Type of Engine for, XII. 87.
..... Uses to which put, XII. 73.
..... Weight of, on land, XII. 101.
..... Wheel arrangements of, XII. 92.
..... Working costs of, XII. 97.
Air Cooling, Advantages of, in Hot Climates, XIV. 295.
..... Cylinder Temperatures with various compression Ratios, XIV. 252.
..... Cylinder Temperatures with varying Revolutions, XIV. 251, 273.
..... Effect of Consumption on Cylinder Wall temperature, XIV. 259.
..... Effect of Fuel Composition on Heat transmitted to Cylinder Wall, XIV. 256.
..... Effect of reduction in Wind Speed on Cylinder temperature, XIV. 258.
..... Effect of Strength of Mixture on Heat transmitted to Cylinder Walls, XIV. 253, 717, 721.
..... Effect of Surface Roughness on, XIV. 285, 297.
..... Effect of Timing on Cylinder Wall temperature, XIV. 255.
..... Formula for Heat Loss, XIV. 244.
..... Heat Dissipation co-efficients of various materials, XIV. 243.
..... Heat transmitted to Cylinder Walls, Factors affecting, XIV. 246.
..... Laws of, XIV. 243, 285.
..... Maximum permissible temperature of Cylinder Head, XIV. 257.
..... Minimum Air Velocity for, XIV. 258.
..... Pitch of Fins, XIV. 245, 286.

Air Cooling, Power absorbed by Fan, XIV. 274, 284, 287, 288.
..... Proportions of Fins, XIV. 244, 245.
..... Surface required per B.H.P. for, XIV. 257, 259, 278, 286, 295.
AIR COOLING OF PETROL ENGINES, THE (Dr. A. H. Gibson), XIV. 243.
Air Hardening Steel, Heat-Treatment of, XI. 331; XIV. 519.
..... Precautions for design of Parts of, XI. 351, 366, 368.
Air-Resistance of Rotary Engines, XIII. 88.
Air and Water Cooled Engines, Weight of, compared, XIII. 94.
Alcohol, Alterations required in existing Engines for use with, XIV. 651.
..... Compression Pressure required for, XIV. 651.
..... Mixture of, suitable for use in existing Engines, XIV. 651.
..... Production of, from Muahua, XIV. 652.
Alloys, Action of, on Steel, XIV. 572, 590.
Alternating Stress Tests, Bibliography of Work done on, XV. 481.
Aluminium for Cylinders, Advantages of, XIV. 264.
..... Conductivity of, XIV. 261, 299.
..... for Motorcycles, XIII. 293.
..... "Growth" of, XIV. 261, 291, 294, 298.
..... Shrinkage allowance for, XIV. 262.
..... Strength of, XIV. 261, 299.
..... Suitable Alloys of, XIV. 260, 291, 574.
Annealing, Effect of, on Steel, XI. 320.
Anthracite, Composition of Gas from, XV. 815.
Arnold Test, Information given by, XIV. 568, 581, 596, 736.
Atwater-Kent System of Contact Breaker for Starting Gears, XI. 54.
"Autolew" Formula, XIV. 439, 444, 726.
Automobile Parts, Suggested materials for, XIV. 535, 559, 583, 585, 593, 599, 737, 742.
Auxiliary Spark Gap, Effect of Introduction of, on Magnetos, XIV. 327, 379, 390.
Axles, Materials for, XIV. 509, 594, 598, 600.

Backlash in Worm Gear, XI. 126, 158, 170.
Balance of Motorcycle Engine, XIII. 348, 384, 385, 520—537, 539, 541, 543, 554, 555.
BALL BEARING, The, IN THE MAKING, UNDER TEST AND ON SERVICE (H. L. Heathcote), XV. 569.
Ball Bearings, Cause of Failure in, XV. 616.
..... Clearance between Ball and Track, XV. 673, 685, 699.
..... Corrosion in, XV. 611, 664, 684.
..... "Creep" in, XV. 671, 678, 679, 680, 683, 684, 698.
..... Dimensions of Contact Surface in, XV. 599, 663, 668, 690, 692, 595.
..... Effect of fitting Cage on, XV. 647.
..... Effect on Heat of Varying Number and Radii of Balls, XV. 616.
..... End Play in, XV. 574, 672, 698.

Ball Bearings, Exclusion of Dirt from, XV. 667, 675, 679, 682, 694.
..... Fit of, XV. 570, 572, 587, 672, 690, 698.
..... Flexure in, XV. 594.
..... Force between Balls, XV. 644.
..... for Connecting Rods, XV. 639.
..... Form of Contact Surface in, XV. 593.
..... for Worm Gears, Radius on, XI. 130.
..... Friction and Heat at Contact Surfaces in Journal Bearings, XV. 602.
..... Friction and Heat at Contact Surfaces in Thrust Washer Bearings, XV. 622.
..... Friction between Balls, XV. 645.
..... Gauges for, XV. 650, 685.
..... Gyrostatic Action, XV. 636, 692.
..... Heat, Rate of Evolution of, in Journal and Thrust Bearings, XV. 625.
..... in Automobile Wheels, Stresses on, XV. 576.
..... Internal Stresses in, XV. 612.
..... Journal, Ability to withstand Side Thrust, XV. 587, 659, 663, 677, 687, 690.
..... Load Carrying Capacity of, XV. 634, 665, 682, 691.
..... as affected by Rotation of Centr or Outer Ring, XV. 689, 700.
..... Load on, when Cornering, XV. 583.
..... Load Stresses on Journal Bearings, XV. 629.
..... Thrust Bearings, XV. 628.
..... Lubrication of, XV. 690.
..... Manufacture of, XV. 647.
..... Balls for, XV. 652, 673, 680.
..... Method of Determining Overload on Balls, XV. 688, 691, 699.
..... Non-Slip Bands in, XV. 604.
..... Stresses due to Inertia of Revolving Parts, XV. 634.
..... Testing of, XV. 655, 657, 660, 667, 669, 693, 694.
..... Wear of Balls in, XV. 657.
Balls for Ball Bearings, Manufacture of, XV. 652, 673, 680.
Batteries, Chemical Actions in, XIII. 400, 424.
..... Exide and Edison compared, XIII. 399, 411, 414, 417, 420, 424, 428, 430.
..... Recuperation of, XIII. 400, 420.
Battery Service System, XIII. 405, 420.
Belt Transmission for Motorcycles, XIII. 261.
Bendix Pinion for Starting Gears, XI. 51, 52, 64, 66, 71, 73.
Benzol, Quantity produced, XIV. 14.
..... Effect of, on "Pinking," XIV. 256.
B.E.S.A. Standard Steels, XIV. 566.
Bevel Gear compared with Worm Gear, XI. 159.
Bijur System of Starting Gear, XI. 44.
Bonniksen Speedometer Used as Accelerometer, XV. 842.

Brass, Cold-Rolled, Wöhler Tests on, XV. 468, 489.
Bright-Drawing, Effect of, on Steel, XII. 397, 412, 416.
Brinell Test, Information given by, XV. 151.
Brinell Testing Machine, Use of, XI. 331.
"Burble" Point of Model Propellers, XI. 294, 295.

"Capacity Component," Definition of, XIV. 364.
Carbon, Influence of, on Valve Steels, XIV. 81, 111.
Carbon Steels, Safe Range of Stress in, XV. 475.
Carburation, Points necessary for, XIV. 306.
Carburising, Depth of, XIII. 48.
CAR DESIGN AND CAR USAGE (E. N. Duffield), XIV. 121.
Card Indexing, Doucet's System, XII. 168.
Carnot Cycle, XIII. 471.
Case-Hardening of Hollow Parts, XI. 321, 339, 359.
..... Nickel Steels, Percentage of Carbon in, XII. 388, 411.
..... Precautions necessary in, XII. 413.
Cast Iron for Cylinders, Suggested composition of, XIV. 527, 744.
Caterpillar Tractor for Agricultural work, XII. 97.
Causes of Failure of Material, XIII. 42.
Change-Speed Gear for Electric Vehicles, XIII. 397, 412, 414, 418, 420, 425, 430.
Change Wheels, Nomogram for obtaining sizes of, XII. 228.
Chassis, Materials for, XIV. 509.
Chromium, Effect of, on Steel, XII. 499.
..... Effect of, on Mechanical Properties of Steels of varying composition at high temperatures, XIV. 45.
..... Steel for Valves, Advantages and Disadvantages of, XIV. 62.
Chromium Steels, Treatment of, XI. 347.
Citroen Gear, XIV. 413.
Clutches for Motorcycles, XIII. 258, 283, 290.
Coal, Pulverised, Use of, in Internal Combustion Engines by Bourne, XIII. 496, 513.
.... Value of, as fuel under different conditions of use, XIV. 171.
Cobalt, Effect of, on Valve Steels, XIV. 81, 111.
Coke, Composition of Gas from, XV. 813.
Coke-Fired Lorry, Fuel cost of, XIV. 16.
Cold-Working, Effect of, on Test Results, XV. 500, 504.
Compression Pressure, Variation of, with Speed, XIV. 376, 389.
..... Ratio and Pressure, Nomogram for, XII. 204.
Conductivity of different Materials, XIII. 95.
Connecting Rods, Materials for, XIV. 509.
Connecticut System of Contact-Breaker for Starting Gears, XI. 60.
Consumption, Comparison of, between Motor Car and Motor Cycle, XV. 868.
..... Effect of size of Carburettor on, XIII. 196.
..... of Agricultural Tractor per acre, XII. 87, 99.
..... Improvement in, with increase of Speed, XIII. 187, 188.
..... on various Gears, XIII. 190.

Consumption at varying Loads, XIII. 186, 201.
Cost of Electric Traction, XIII. 402, 415, 432.
Cost, Comparative, of Electric and Horse Traction, XIII. 404, 431.
Cost per ton-mile of Coal and Petrol, XIV. 174, 175.
Costing System, XV. 16, 22.
Costs, Working, of Agricultural Tractor, XII. 97.
Crank Effort, XIII. 355.
Crank Pin Dimensions, Comparison of Car and Motorcycle Engine, XIII. 362.
Crankshaft, Cost of four-throw, XIII. 391.
..... Journals, Effect of Twisting of, XI. 310, 336, 350, 364.
..... Method of measuring frequency of torsional vibrations in, XIV. 497.
..... Six-throw, Design of, XI. 353.
..... Stiffness of, compared with straight shaft, XIV. 484.
..... Stress, Effect of balance weights on, XIV. 472, 484, 490.
Crankshafts, Materials for, XIV. 508.
Creep in Ball Bearings, XV. 671, 678, 679, 680, 683, 684, 698.
Critical Cooling velocity of Steel, XII. 497.
Crompton Adjustable Screw Gauge, XI. 189, 192, 204.
Cross Impact Tests, XIII. 39, 61, 69.
Cylinder Arrangements, Various possible, XIV. 447.
Cylinders, Materials for, XIV. 507, 527.

Daimler-Lanchester Standards for Worm Gear, XI. 118.
Delco Electric Starter, XI. 33.
Desaxé Engine, Advantages of, XIV. 493, 494.
..... Effect of, on Inertia Forces, XIV. 463, 485, 491.
..... Side Pressure on, XIV. 466.
..... Formula for obtaining Inertia in, XIV. 473.
Detonation, XV. 736, 738, 749.
DIESEL ENGINE, THE (Chas. Day), XIII. 471.
Diesel Engine, Advantages of, XIII. 475, 511.
..... Conditions of Cycle, XIII. 472, 493.
..... for which suitable, XIII. 482, 485.
..... Definition of, XIII. 486.
..... Distinction between, and Semi-Diesel Engine, XIII. 475.
..... for Automobile use, XIII. 476, 479, 508, 514.
..... for Locomotives, XIII. 479, 505.
..... Fuels suitable for, XIII. 479.
..... Weight of, XIII. 504, 510.
Diesel Patents, Early adoption of, XIII. 498.
Differential Gears Considered Unnecessary, XV. 286, 328, 341, 346.
Dilution of Labour, XI. 24.
Distortion in Valves, Best Steel for avoiding, XIV. 58.
Dolphin Engine, XIII. 540.
Double-Helical Gears, Advantages of, for Automobile Work, XV. 271, 312, 323, 332, 334, 339.

Draw-Bar Pull, XII. 111, 438.
Duralumin, Change of Properties in, with Time, XV. 489.
..... Wöhler Tests on, XV. 469, 489.
Dynamical Similarity Applied to Model Propellers, XI. 293, 298.
Dynamometer Road Tests, XII. 109.
Dynamo of Starting Gear, Speed of, XI. 49.

Edison Battery for Electric Vehicles, XIII. 399.
..... Guarantee for, XIII. 418.
Education of the Automobile Engineer, XI. 12.
..... Examination Test, XI. 15.
..... Teaching of Drawing, XI. 16.
Efficiencies of various Plant, XIII. 427.
Elastic Limit, Definition of, XV. 487.
np of Various Motor Cycle Engines, XV. 859.
ELECTRICAL EQUIPMENT OF A CAR, THE (by A. Ludlow Clayden), XI. 33.
Electric Starter, "Delco," XI. 33.
Electric Starter for Motor Cars, Origin of, XI. 33.
Electric Starting Systems, Single *v.* 2 Unit, XI. 34, 73, 75.
Electric Traction compared with Horse Traction, Cost of, XIII. 401, 404, 415, 431.
Electric Transmission, compared with Hydraulic, XI. 224.
ELECTRIC TRANSPORT VEHICLES (Carl Opperman), XIII. 395.
Electric Vehicles, Number of, in use in America, XIII. 409.
..... Sphere of Utility for, XIII. 408.
..... Road Resistance of, XIII. 410.
Electrodes, XII. 526.
Electro-Deposited Metal, Wearing qualities of, XIV. 605.
..... Heat-Treatment of, XIV. 638.
..... Parts for which suitable, XIV. 605, 606.
Electro-Deposition of Iron, Anodes for, XIV. 613, 637.
ELECTRO-DEPOSITION OF IRON, THE (B. H. Thomas), XIV. 603.
Electro-Deposition of Iron, Bath for, XIV. 612, 637.
..... Causes of Failure, XIV. 615.
..... Cleaning Work for, XIV. 610, 631, 635, 637, 640, 644.
..... Current Density for, XIV. 614, 627, 632, 633 637, 641, 644.
..... Origination of Plan, XIV. 622.
..... Precautions necessary, XIV. 623, 629, 640.
..... Rate of Deposit, XIV. 627.
..... Thickness of Deposit, XIV. 626.
..... Waxing Work for, XIV. 611.
Engineer, Automobile, Education of, XI. 12.
..... Examination Test for, XI. 15.
..... Teaching of Drawing to, XI. 16.
Engineers, Naval, Recognition of, by Admiralty, XI. 20.

Engineers, Position of, in the Service, XI. 23.
EPICYCLIC GEARING IN AUTOMOBILE PRACTICE (D. L. Prior), XV. 887.
Epicyclic Differential Gear, XV. 285, 904.
..... Gear-Box, Lanchester, XV. 314, 346, 908.
..... Gearing for Engine Starters, XV. 910.
..... Gears for Gear-Boxes, XV. 277, 314, 330, 339, 346, 906.
..... Ford, XV. 281, 907.
..... Rules for, XV. 317.
..... Train, Method of Calculating, XV. 282, 314, 888, 894.
Etching Steel, Re-agents for, XIII. 38.
Exhaust Gases, Effect of Admitting, to Cylinder, XV. 738, 743, 753, 758, 760.
Exide Battery for Electric Vehicles, XIII. 398.
..... Number of Discharges from, XIII. 421, 431.

FAILURE, SOME CAUSES OF, ILLUSTRATING THE NEED FOR CARE IN THE USE OF STEEL FOR AUTOMOBILE CONSTRUCTION (Lieut. W. E. Williams), XIII. 33.
Fan, Power absorbed by, XIV. 274, 284, 287, 288.
Farm Tractors, Factors governing design of engine for, XII. 87.
Farm Work, Universal Machine for, XII. 97, 99, 103, 104.
Fins for Air Cooling, Pitch of, XIV. 245, 286.
..... Proportions of, XIV. 244, 245.
F. J. Worm, XIV. 426.
Flanges for Starting Gears, Standardized, XI. 54.
FLOWMETERS, PETROL, AND THE CALIBRATION OF CARBURETTOR JETS, SOME NOTES ON (Captain G. Smith-Clarke), XIII. 437.
Flywheel, Size of, for engine for Agricultural Tractor, XII. 90.
Flywheels, Effect of Weight of, XIII. 387.
..... Inside, XIII. 362, 364, 377, 378.
Foremen, Duties of, XV. 15.
Föttinger Hydraulic Transmission, XI. 230.
Four-Cycle and Two-Cycle compared, XIII. 476, 501, 520.
"F. N." Four-Cylinder Motorcycle Engine, XIII. 373, 390.
Fractures, Appearance of, XIII. 36.
Frames for Motorcycles, XIII. 267.
Franklin Air Cooled Engine, XIV. 266, 290, 298.
Frémont Machine, Impact Speed of, XV. 131.
Frictional Losses in Motor, XV. 735.
Front Forks for Motorcycles, XIII. 269.
Fuels, B. Th. Us. per penny, XIV. 13, 14.
..... Boiling Points of, XIV. 309.
..... Calorific Value of, XIV. 13, 14.
..... Consumption of, in U. S. A., XIV. 14.
..... Notes on, XIV. 308.

Fuels, Price per gallon, XIV. 13, 14.
..... Spontaneous Ignition Temperature of, XV. 750.

Gas from Anthracite, Composition of, XV. 815.
..... Coke, Composition of, XV. 813.
Gas Tightness in Ignition Plugs, XII. 538.
Gas Traction, Report of Inter-Departmental Committee on, XV. 824.
Gas Velocity, Effect of, on Valve Temperature, XIV. 97.
..... Mean effective Pressure, XIII. 153.
Gas-Driven Tramway, Early use of, XIV. 24.
Gear-Box, Efficiency of, XI. 237.
..... N. P. L. Test of, XIII. 307.
..... for Motorcycles, XIII. 260, 289.
GEAR CUTTING FOR MOTOR CARS (P. J. Worsley), XIV. 401.
Gear Grinding Company's Machine, XIV. 422, 443, 727, 729.
Gear Teeth, Factors of Safety for, XIV. 726.
Gear Wheels, Steel most suitable for, XI. 327.
Gears, Bevel and Spiral compared, XIV. 703.
..... Number in use, XIV. 705.
..... Double-Helical, Advantages of, for Automobile Work, XV. 271, 312, 323, 332, 334, 339.
..... Erosion of, XV. 313, 334, 349, 364.
..... Formula for size of Teeth, XIV. 439, 444, 726, 729.
..... Heat-Treatment for, XIV. 422.
..... Steels for, XIV. 422, 439, 443, 510, 580, 595, 597, 727.
Gleason Gear-Hardening Machine, XIV. 422, 443, 727, 729.
..... Spiral Bevel Gears, XIV. 415.
Governor Design, Nomogram for, XII. 204.
Gudgeon Pin, Method of holding, XIII, 252, 364, 382, 384.
Gumming of Piston Rings, Cause of, XIII. 547.

Hair Cracks in Steel, XI. 312, 313, 344, 350, 354.
Hall Hydraulic Transmission, XI. 227, 231, 241, 249.
Heat Loss, at varying Revolution speeds, XIV. 273.
Heat-Treatment, Danger of Rough Surfaces in, XI. 310, 319.
..... Effect of variations in, XI. 308.
..... Notes on, XI. 328.
Heating and Cooling Curve for Steel, XII. 491.
Hele-Shaw Hydraulic Transmission, XI. 232, 237, 245, 257
Helical Gear-Box, Lanchester, XV. 312.
Helical Gears, Principle of, XIV. 413.
Helical Springs, Nomograms for design of, XII. 209.
Helicopters, Experiments on Model Propellers for, XI. 285, 291, 295, 302
Henderson Four-Cylinder Motorcycle Engine, XIII. 374.
Hindley Type of Worm Gear, Objections to adoption of, XI. 145.
Horse-Power for given Mean Effective Pressure, Nomogram for, XII. 215.
Horse Traction compared with Electric Traction, Cost of, XIII. 401, 404, 415, 431.

HYDRAULIC TRANSMISSION (by F. Leigh Martineau), XI. 223.
Hydraulic Transmission, Acceleration obtained with, XI. 254.
..... Comparison of, with electrical, XI. 224.
..... Cost of, XI. 240.
..... Föttinger, XI. 230.
..... Hall, XI. 227, 231, 241, 249.
..... Hele-Shaw, XI. 232, 245, 257.
..... Mechanical Efficiency of, XI. 237.
..... Janney, XI. 229, 231, 243, 248, 254.
..... Lentz, XI. 230.
..... Pier's Reciprocating Plunger Type, XI. 235.
..... Pittler, XI. 230, 250.
..... Prott, XI. 226.
..... for Steering Ships, XI. 252.
..... Temperature Increase Allowable in, XI. 256.
..... Trouble with Air in the Oil, XI. 256.
..... Variation in discharge of Pumps for, XI. 232.
..... Weight of, XI. 239.

Ignition Plugs, Air Cooled, XII. 544.
..... Bosch magnetic, XII. 514.
..... Requirements for, XII. 524.
..... Variation in design for different engines, XII. 522.
..... for working under Water, XIV. 387.
..... Amount of Petrol Vaporized per spark by, XIV. 373.
..... Effect of Electrode Material of, on voltage, XIV. 343.
..... High voltage in, on easy starting, XIV. 361.
..... Pressure of air on voltage, XIV. 351.
..... Temperature of air on voltage, XIV. 354, 390.
..... shape of Electrode on voltage, XIV. 344, 350, 380.
..... varying voltage on ignition, XIV. 368.
..... Factors affecting sparking voltage, XIV. 341.
..... Gas Tightness in, XII. 538.
..... Insulators for, XII. 531.
..... Materials for Insulation for, XIV. 360.
..... Mica for, XII. 533, 536.
..... Porcelain for, XII. 531.
..... Position of, XIII. 198.
..... Requirements to be met, XIV. 358, 362.
..... Spark Energy required to ignite Mixture, XIV. 368.
..... Spark Gap for, XII. 530.
..... Steatite for, XII. 532.
Ignition Temperature, Spontaneous, of different Oils, XIII. 480, 484, 487, 489, 507, 511, 514, XV. 750.
IGNITION, THE PROCESS OF (A. P. Young and H. Warren), XIV. 305.
Impact Test Piece, Circular, XI. 315, 341, 345, 347, 359, 365.
..... Used to check Analysis, XI. 323, 344, 351, 358.

Impact Test, Information given by, XIV. 568.
..... Speeds for, XII. 363, XV. 131.
Imports of Steel Castings, XII. 477.
" Impulse Starter," XIV. 374.
" Inductive Component," Definition of, XIV. 364.
INERTIA TORQUE IN CRANKSHAFTS (F. A. S. Acres), XIV. 447.
Inertia Torque, Definition of, XIV. 450.
..... Effect of Balance Weights on, XIV. 464.
..... Effect of Connecting Rod Length on, XIV. 461.
..... Stroke Length on, XIV. 464.
INFLUENCE OF VALVE AREA, REVOLUTION SPEED AND COMBUSTION CHAMBER DESIGN UPON HORSE-POWER AND THERMAL EFFICIENCY (L. H. Pomeroy), XIII. 163.
Ingots, Precautions to be taken in casting, XII. 500.
Inlet Pipes, Nomogram to obtain area of, XII. 218.
Inlet Valve, Masked, XV. 740.
Inside Flywheels, XIII. 362, 364, 377, 378.
Inspection, Value of, XII. 7.
..... Organisation of Staff for, XII. 10.
..... French System of, XII. 14.
Insulators for Ignition Plugs, XII. 531.
" Intrinsic Brittleness," XIV. 588.
INVENTION, EFFICIENT (Douglas Leechman), XIII. 207.
Inventions, Advice as to putting on Market, XIII. 223.
Inventors, Hints for, XIII. 226, 232, 234.
..... Need for Training of, XIII. 221.
Iron-Carbon equilibrium diagram, XII. 489.
Izod Machine, Impact Speed of, XV. 131.
Izod Test, Information given by, XIV. 589, 592, 734, 739, 741.

Janney Hydraulic Transmission, XI. 229, 231, 243, 248, 254.
Jets, Variation in flow through, XIII. 442.

Kennerson's Test on Worm Gears, XI. 83.
Kessler Engine, XV. 752, 759.
Keys, Materials for, XIV. 510.

Labour Bureau, Suggestions for, XI. 19.
..... Dilution of, XI. 24.
Labour "Turnover," XI. 405, XV. 12.
Laminated Gears. Experiments with, XIV. 432.
Lanchester Epicyclic Gear-Box, XV. 314, 346, 908.
..... Helical Gear-Box, XV. 312.
Lanchester Vibration Damper, XIV. 485, 490.
Land, Pressure on, XIII. 80, 118, 438.
Lentz Hydraulic Transmission, XI. 230.

Limit Gauging, Principles Involved in, XI. 188, 194.
Limit of Proportionality, XV. 487.
Literature, Classification of, XII. 151.
Locomotive, Weight of, XI. 4.
Lorry Engines, Speed of, XIV. 485.
Losses, frictional, in Motor, XV. 735.
Lubricating Oil, Tests of, XIII. 303, 335.
LUBRICATION OF MOTOR CARS, THE, (Captain G. W. A. Brown), XIII. 299.
Lubrication of Motorcycle Engines, XIII. 366.
..... Lanchester Device, XIII. 275.
Lucas Valveless Engine, XIII. 527, 539.

Machining, Ease of, Best Steel for, XIV. 59, 591.
Magneto and Battery Systems, Comparison of cost of running, XIV. 392.
..... working parts of, XIV. 313.
Magnetos, Auxiliary Spark Gap, Effect of Introduction of, XIV. 327, 379, 390.
..... Factors affecting voltage at which sparking occurs, XIV. 330.
..... "Impulse Ratio," Definition of, XIV. 331.
..... Variation of, with different metals, XIV. 378.
..... Method of obtaining artificial resistance, XIV. 381.
..... Recent Improvements in, XIV. 322.
..... Relationship between gap length and sparking voltage of, XIV. 333, 371.
..... Rotary Safety Spark Gap, Discharge voltage of, XIV. 327.
..... Spark Gap Distributor, Object of, XIV. 324.
..... Sparking voltage, Variation of, with Speed, XIV. 340.
..... Tests of, XIV. 318.
..... "Utility Test," XIV. 320.
Malleable Cast Iron, Strength of, XII. 404, 408; XIV. 526.
Manganese, Effect of, on Steel, XII. 499.
Mass Effect on Steel Tests, XV. 108, 121.
Materials for Axles, XIV. 509, 594, 598, 600.
..... Crankshafts, XIV. 508.
..... Cylinders, XIV. 507, 527.
..... Valves, XIII. 282.
..... Suggested for Automobile Parts, XIV. 535, 559, 583, 585, 593, 599, 737, 742.
Mechanical Tests, Description of various, XIV. 560—565.
Membership of the Institution of Automobile Engineers, XII. 4.
Mica for Ignition Plugs, XII. 533, 566.
"Miranda" S, Y, Speed of, XI 4.
Misfiring, Cause of, XIV. 361.
Monel Metal, XV. 521, 539, 553, 560, 562.
MOTOR BICYCLE FRAME DESIGN (G. D. Leechman), XV. 219.
Motor Buses, Items influencing maintenance cost of, XIV. 17.
..... of operating expenses, XIV. 10.

Motor Buses, Proportion of wages to total expenses, XIV. 11.
..... Relation of maximum speed of, to cost of maintenance, XIV. 20.
Motor Car Engines, Various particulars of, XIV. 470.
Motor Cars, Daily Production of, in U. S. A., XIV. 169.
MOTOR CYCLE DESIGN, A SURVEY OF CURRENT (D. S. Heather), XIII. 247.
MOTOR CYCLE, THE ENGINE OF THE SIDE CAR (E. Caudwell), XIII. 345.
Motor Cycle, Basis of Comparison of, with Motor Car, XV. 707, 708, 712, 866, 870.
..... Engine, Balance of, XIII. 348, 384, 385, 520, 537, 539, 541, 543, 554, 555.
..... Engine Performance, XV. 852.
..... Engines, Petrol Consumption of, XV. 857.
..... Frame Nomenclature, XV. 223, 243, 250.
..... Tubes, Formula for Weight of, XV. 245.
MOTOR CYCLE PERFORMANCES, COMPARATIVE (D. S. Heather), XV. 705.
MOTOR CYCLES, SPRINGING OF, THE (G. H. Savage), XV. 35.
Motor Cycles, Belt Transmission for, XII. 261.
..... Braking Effort in, XV. 880.
..... Capt. Burt's Spring Frame, XV. 52, 76.
..... Capt. Francis' Spring Frame, XV. 50.
..... Clutches for, XIII. 258, 283, 290.
..... Comparison of different types of engines for, XIII. 257.
..... Douglas Rear Sprung, XV. 47.
..... Effect of Wind Screen on, XV. 847, 853.
..... Engines, Lubrication of, XIII. 366.
..... Frames for, XIII. 257.
..... Front Forks for, XIII. 269.
..... Gear-Boxes for, XIII. 260, 289.
..... Life of Roller Bearings on, XIII. 379, 390.
..... Matchless Rear Sprung, XV. 63.
..... Permissible Movement of Springs on, XV. 54, 77, 90.
..... Royal Ruby Spring Frame, XV. 48.
..... Single-Cylinder Engine for, Effects of, XIII. 256.
..... Spring Spindle Bearing Design, XV. 66.
..... Springs for, XIII. 269, 274.
..... Stress in Springs, XV. 96.
..... Tractive Effort of Engine, XIII. 347, XV. 862.
..... Transmission for, XIII. 261.
..... Various, Road Performance of, XV. 861.
..... Weight Distribution in, XV. 846.
..... Weight of, XV. 844.
..... Wheel Grip of Spring Frame Machine, XV. 87.
MOTOR CYCLES, TWO-CYCLE ENGINE FOR (E. Tilston), XIII. 519.
Motor Vehicles, Number of, in U. S. A. in 1919, XV. 516.

NAPHTHALENE SOLUTIONS, COMBUSTION OF, IN INTERNAL COMBUSTION ENGINES, THE (L. S. Palmer), XV. 195.
Naphthalene, Commercial Advantages of, XV. 208.
..... Consumption with various percentages of, in Benzol, XV. 205.
..... Practicable percentage of, in Benzol, XV. 203, 207.
..... Solubility of, in Benzol and Petrol, XV. 196.
Naval Engineers, Recognition of, by Admiralty XI. 20.
Nickel, Effect of, on Steel, XII. 498.
Nickel Steel, Effect of varying composition of, at high temperatures, XIV. 50.
Nickel Steels for Case-Hardening, Percentages of Carbon in, XII. 388, 411.
Nomography, Bibliography of, XII. 223.
..... Accuracy of, 12, 229.
Normalising, Effect of, on Steel, XII. 495.
Normalising Temperature, XIII. 46.
Notch, Influence of Radius of, on Impact Tests, XII. 235, 288, 341, 359.
..... Bar Tests, Object of, XII. 336, 340, 352.
..... for Impact Tests, Depth of, XII. 338.
..... Best type of, XII. 362.

Octoid Tooth, XIV. 408.
Oil Groove, Proper Formation of, XIII. 334, 337.
Oilless Bearings, XIII. 323.
Oil Retaining Device, Screw Thread, XIII. 325, 330, 334, 338.
.. Temperature in Crank Case, XIII. 332, 335.

Paraffin Engine, Valve for, XII. 88.
Patent Fees, XIII. 208, 240.
Patents, Extension of, XIII. 214.
Peat, Price of, XIV. 208, 711, 713.
Petrol Engine, Various Sizes of, XI. 6.
..... Mixture, Correct, Heat Value of, XV. 718.
Petrol, Thermal Value of, XIII. 99.
..... Quantity required per revolution, XIII. 533.
Phosphorus, Effect of, on Steel, XI. 322, 345.
Pier's Reciprocating Plunger Type of Hydraulic Transmission, XI. 225.
Pilot Ignition System for Tar Oils, XIII. 480, 488, 499, 512.
"Pinking," XII. 521.
Piston Ring Pressure, Effect of, on Mechanical Efficiency, XIV. 464.
Pistons, Advantages of Aluminium for, XIV. 268.
..... Aluminium, Clearance for, XIV. 269.
Pitches in Screw Gauges, Number of, XI. 215, 220.
Pittler Hydraulic Transmission, XI. 230, 250.
Plant in Works, Layout of, XI. 372, 389, 393, 396, 397.
Pneumatic Tyre Making, Canvas for, XV. 410, 439, 446.
..... Limits in Weight Allowed, XV. 420.
..... Moulding Systems Employed in, XV. 387.
..... Vulcanising Systems Employed in, XV. 389.

Pneumatic Tyre Rims, S.S. and B.E. Types compared, XV. 394.
PNEUMATIC MOTOR TYRES, DESIGN AND MANUFACTURE OF, THE (C. Macbeth), XV. 379.
Pneumatic Tyres, Alarm for, XV. 805, 809.
..... Effect of Standard Tolerances on Fit of Bead, XV. 397.
..... Factors of Safety in, XV. 382.
..... Functions Defined, XV. 379.
..... Giant, XV. 447, 456.
..... Hints on Care of, XV. 448.
..... Internal Pressure in, XV. 382.
..... Necessity for Security Bolts in, XV. 449.
..... Nomenclature of Parts Used in, XV. 416.
..... Non-Skid Tread Designs, XV. 403.
..... Percentage of Stretch Allowed, XV. 402, 439, 446.
..... Requirements of Rubber Used in, XV. 383.
..... Re-Treading of, XV. 457, 458.
..... Rolling Factor, XV. 405, 805.
PRESIDENTIAL ADDRESS (L. A. Legros), XI. 3.
Pressure on Land with various Machines, XII. 80, 118, 438.
PRODUCER GAS FOR MOTOR VEHICLES (Col. J. D. Smith), XIV. 169.
Producer, Consumption per foot of grate area of, XIV. 194—221, 234.
..... Disadvantages of ordinary design of, XIV. 178.
..... Early History of, XV. 813.
..... Stewart, Description of, XV. 820, 835.
..... Weight of, XV. 829, 832.
..... Suction Gas, Origin of, XV. 815.
..... Refractory Liner for, XIV. 191.
..... Weight of, XIV. 193, XV. 829, 832.
Producer Gas, Advantages and Disadvantages of, XIV. 172, 176.
..... Compression Pressure for, XIV. 197, 234, XV. 826.
..... Cost of, compared with Petrol, XIV. 200.
..... Upkeep, XIV. 203.
..... Depth of Fuel Bed for, XIV. 180, 206, 208, 213, 219, 221, 224, 226, 227, 230, 233, 236, 238, XV. 833.
..... Exhaust Gas Analysis of, XIV. 203, 218, 231.
..... First Introduction of, on Motor Vehicles, XIV. 176.
..... Fuels suitable for, XIV. 206—209.
..... Men necessary to run, XIV. 713, XV. 834.
..... Plant for Ford Car, XIV. 212.
..... Sparking Plugs for, XIV. 203.
..... Thermal Efficiency of, XIV. 714.
..... Time taken to start, XIV. 201, 215, 712, XV. 823.
..... Waste Gas Analysis of, XIV. 205.
..... Water Consumption with, XIV. 201, 228.
PRODUCTION, SOME NOTES ON (Sir Hy. Fowler), XV. 6.
Propeller Speeds, XIII. 90.
PROPELLER, TESTS ON MODEL (by J. Lawrence Hodgson), XI. 261.

Propellers, Model, "Burble" Point of, XI. 294, 295.
..... Cavitation in, XI. 302, 303.
..... Characteristic Curves of, XI. 263.
..... Design of, XI. 299, 300.
..... Diameter of Slip Stream, XI. 277, 294, 296.
..... Dynamical Similarity applied to, XI. 293, 298.
..... Effect of Proximity of Impediments on, XI. 267.
..... Experimental Apparatus for, XI. 263.
..... for Helicopters, Experiments on, XI. 285, 291, 295, 302.
..... Method of Constructing, XI. 263.
..... Numbers and Shapes of Blades compared, XI. 280, 294, 296, 299, 300, 301.
Prott Hydraulic Transmission, XI. 226.

Re-Agents for Etching Steel, XIII. 38.
Reinecker Spur Gear Grinding Machine, XIV. 424.
Rejections, Systems for dealing with, XII. 14.
Research, Department of, Industrial and Scientific, XII. 165.
..... Associations, Rules for, XII. 165.
RESEARCH, AUTOMOBILE, SUGGESTED PROGRAMME FOR, A (Geo. Watson), XV. 511.
Road Resistance of Electric Vehicles, XIII. 410.
ROAD TRANSPORT, THE POSSIBILITIES OF FURTHER ECONOMIES IN (Presidential Address: T. Clarkson), XIV. 683.
ROADS AND VEHICLE MAINTENANCE (Col. R. E. Crompton), XV. 167.
oads, Formula for Impact of Wheels on, XV. 177, 180, 182.
..... Kerbs for, XV. 171, 779, 785.
..... Method of Re-Surfacing, XV. 170, 173.
..... Re-inforced Concrete for, XV. 783, 786.
Roller Bearings, Life of, on Motor Cycles, XIII. 379, 390.
Rolling Resistance, XII. 82.
Rotary Engines, Formula for obtaining centrifugal tension in rod, XIV. 476.
..... Inertia Torque of, XIV. 476.
..... Side Pressure in, XIV. 476.

Sandwich System, XII. 450, 460.
Sankey Test, Information given by, XIV. 568, 592, 596.
Scaling of Valves, Cause of, XIV. 38.
Scleroscope, Use of, XII. 417, 421.
SCREW GAUGES, NOTES ON (by Col. R. E. Crompton), XI. 173.
crew Gauges, Angles and Forms of Thread, XI. 155.
..... B. A. System, Introduction of, XI. 175.
..... Clearance on Crest and Root, XI. 190, 191, 194, 208.
..... Formula for, XI. 208.
..... Crompton's Adjustable, XI. 189, 192, 204.
..... Fit of Threads in, XI. 216.
..... High Crested Threads, XI. 183, 192.

Screw Gauges, Method of Measuring Clearances in, XI. 193.
..... Nomenclature for, XI. 173, 213, 219.
..... Number of Pitches in, XI. 215, 220.
..... Principles Involved in Limit Gauging, XI. 188, 194.
..... Rounded Crests *v.* Flat Crests, XI. 179, 192.
..... Sharp's Suggested Crest and Root Clearances, XI. 210.
..... Stromeyer's Suggested Crest and Root Clearances, XI. 211.
..... System of Gauging, XI. 179.
..... Tolerances to be allowed in, XI. 182, 199.
..... Formula for, XI. 201.
..... Value of Hardening, XI. 191, 200.
..... Wear of, XI. 184, 192.
..... Whitworth Thread, Date of Introduction of, XI. 175, 215.
Selective Inspection, XII. 17.
Semi-Diesel Engine, Definition of, XII. 487.
..... Distinction between Diesel Engine and, XIII. 475.
Shop Committees, Duties of, XV. 9.
Slow Bending Tests, XII. 327, 354; XIII. 35.
Soldering for Cycle Tubes, XIII. 286, 288, 289, 293.
SOME PROBABLE EFFECTS OF THE WAR ON THE AUTOMOBILE INDUSTRY (A. A. Remington), XIII. 4.
Spark Gap, XII. 530.
Spark Gap Distributor, Object of, XIV. 324.
Spiral Ball Thrusts for Worm Gear, XI. 105, 144.
..... Bevels, Definition of Right and Left Hand, XIV. 417.
..... Gears, Definition of, XIV. 411.
..... Formula for Strength of Teeth in, XV. 795.
..... Method of Cutting, XIV. 411.
..... and Straight Bevels, Number in Use compared, XIV. 428, 434.
Springs, Helical, Nomogram for design of, XII. 209.
..... Lubrication of, XIII. 334, 335, 338.
..... Means of obviating accident in case of Breakage of, XIII. 55.
..... for Motor Cycles, XIII. 269, 274.
..... Strength of, for Valves, XIII. 369.
..... Steels for, XIV. 521.
Spuds, Length of, XII. 81, 94.
..... Time taken to remove, XII. 99.
Stampings, Causes of Failure in, XI. 323.
..... Drawings for, XI. 340.
..... Flow of Metal in, XI. 324, 341, 343.
..... Heat-Treatment of, XI. 331.
..... Test Pieces for, XI. 325.
Standard Tooth Form, XIV. 419.
..... Steels, B.E.S.A., XIV. 566.
Standardisation, List of Reports of B.E.S.A. affecting the Automobile Industry, XIII. 17.
..... Parts suitable for, XII. 429.
Stanton Test, Information given by, XIV. 568, 575, 581, 589, 590, 596, 599, 739.

Starters, Engine, Epicyclic Gearing for, XV. 910.
Starting Gears, Armature Bearings for, XI. 54.
..... Automatic Switch for, XI. 46.
..... Bendix Pinion for, XI. 51, 52, 64, 66, 71, 73.
..... Bijur System, XI. 44.
..... Contact Breakers for, XI. 57.
..... Atwater-Kent System, XI. 59, 67.
..... Connecticut System, XI. 60.
..... Effect of position of Brushes in, XI. 38.
..... Magneto and Battery compared, XI. 55, 56, 57.
..... Methods of obtaining Constant Voltage in, XI. 41.
..... Speed of Dynamo for, XI. 49.
..... Standardised Flanges for, XI. 54.
..... Third Brush System, XI. 40, 46.
..... Torque required in, XI. 63, 65, 67.
..... Vibrator Control of Voltage of, XI. 42, 45, 64.
..... Electric Single *v.* Two Unit System, XI. 34, 73, 75.
Steam Generators for use on Aircraft, XIV. 694.
Steatite for Plugs, XII. 532.
STEEL RESEARCH COMMITTEE, SOME NOTES ON THE REPORT OF THE (J. H. S. Dickenson), XV. 99.
STEEL, THE USE AND ABUSE OF (by Lieut.-Colonel R. K. Bagnall-Wild), XI. 307.
Steel, Action of Alloys on, XIV. 572, 590.
..... Best, for ease of machining, XIV. 59, 591.
Steel Castings, Effect of Heat-Treatment on, XII. 472.
..... Contraction of, XII. 475.
..... Imports of, XII. 477.
..... Mechanical Properties of, XIV. 524, 595.
Steel, Chromium, Treatment of, XI. 347.
..... Case-Hardening of hollow parts, XI. 321, 339, 359.
..... Circular Impact Test Piece for, XI. 315, 341, 345, 347, 359, 365.
..... used to check Analysis of, XI. 323, 344, 351, 358.
..... Critical Cooling Capacity of, XII. 497.
..... Range of, XII. 491.
..... Danger of Rough Surfaces in Heat-Treatment, XI. 310, 319.
..... Design of six-throw Crankshaft, XI. 353.
..... Difficulty in determining Yield Point of, XI. 317, 344, 351, 358.
..... Effect of Annealing on, XI. 320.
..... Bright-Drawing on, XII. 397, 412, 416.
..... Chromium on, XII. 499, XIV. 45.
..... Cobalt on, XIV. 81, 111.
..... Manganese on, XII. 499.
..... Nickel on, XII. 498.
..... Normalising on, XII. 495.
..... Phosphorus on, XI. 322, 345.
..... Sulphur and Phosphorus on, XI. 322, 345.

Steel, Effect of Variations in Heat-Treatment of, XI. 308.
..... Heating and Cooling Curves for, XII. 491.
..... Elastic Range of, XIV. 733, 740.
..... for Crankshaft, Requirements of, XIV. 472, 508, 569.
..... Gear Wheels, most suitable, XI. 327.
..... Valves, XIV. 521.
..... Best composition of, XIV. 58, 61, 62, 63.
..... Desirable properties of, XIV. 39.
..... Fibre of, XIV. 91, 102.
..... General conclusions, XIV. 54.
..... properties of, XIV. 55, 107.
..... Heat-Treatment of, XIV. 91, 93.
..... Influence of Carbon on, XIV. 81, 111.
..... Relative Cost of, XIV. 106.
..... Types used, XIV. 40, 81.
..... Hair Cracks in, XI. 312, 313, 344, 350, 354.
..... Manufacture of Ingot, XIV. 528.
..... Mass Correction, Notes on, XI. 334.
..... Notes on Heat-Treatment of, XI. 328.
..... Physical Properties of, XII. 503.
..... Parts, Causes of Failure of, XII. 504 *et seq.*
..... Physical Properties of, XII. 503.
..... Reduction Gears, Design of, XI. 340.
..... Specifications, Interpretation of, XI. 316.
..... Stampings, Causes of Failure in, XI. 323.
..... Flow of Metal in, XI. 324, 341, 343.
..... Heat-Treatment of, XI. 331.
..... Test Pieces for, XI. 325.
..... Use of Brinell Testing Machine for, XI. 331.
..... Variation in Specific Gravity of, XII. 387.
Steels, Air Hardening, Heat-Treatment of, XI, 331.
..... Precautions in Design of Parts of, XI. 351, 366, 368.
..... Alloy, Wöhler Tests on, XV. 473.
..... Brinell Test, Information given by, XV. 151.
..... Carbon, Wöhler Tests on, XV. 471.
..... Case-hardening, Wöhler Tests on, XV. 472.
..... Effect of Mass on Tests on, XV. 108, 121.
..... for Motor Car Parts, Fatigue Range of, XV. 467.
..... Impact Speed of Frémont Machine, XV. 131.
..... Izod Machine, XV. 131.
..... Treatment and Mechanical Properties of various Analyses of, XIV. 511, 521.
..... B.E.S.A. Standard, XIV. 566.
STEELS, FURTHER NOTES ON AUTOMOBILE (Dr. W. H. Hatfield), XV. 465.
STEELS, THE MOST SUITABLE, FOR AUTOMOBILE PARTS (Dr. W. H. Hatfield), XIV. 503.
Stepped Piston for Two-Cycle Engines, XIII. 527.

Stewart Producer, Description of, XV. 820, 835.
..... Weight of, XV. 829, 832.
Still Engine, Thickness of Liner, XIV. 719.
Stresses, Light for Revealing, XII. 31.
..... Distribution of, in Crank Web, XII. 36.
..... Instrument for Measuring Change of Thickness due to, XII. 41, 62.
Stub Tooth, Advantages of, XIV. 421.
Sulphur, Effect of, on Steel, XI. 322, 345.
SUPERCHARGING, SOME EXPERIMENTS ON, IN A HIGH SPEED ENGINE (H. R. Ricardo), XV. 717.
Supercharging, Effect of, XV. 735, 743, 744.
..... on Cooling, XIV. 720.
..... Engine for, XV. 730.
..... Inter-Cooler for, XV. 741.
Switch for Starting Gears, Automatic, XI. 46.

Tappet Clearance, XIV 84, 100, 111.
Tar Oil for Diesel Engines, XIII. 480, 485, 488, 499, 512.
Technical Committee of the Motor Industries, XII. 5.
Temper Brittleness, XII. 350.
Temperature Increase Allowable in Hydraulic Transmission, XI. 256.
Temperature, Spontaneous Ignition, for different Oils, XIII. 480, 484, 487, 489, 507, 511, 514.
Tempering, Effect of, on Steel, XII. 493.
Tempering Temperatures, Effect of variation in, on different Steels, XIV. 70, 71, 81.
..... Nickel on, XIV. 72.
Test Pieces, for Wöhler Machine, XV. 491, 505.
..... Hollow, for Wöhler Machine, XV. 488, 504.
..... Round for Impact Tests, XII. 310, 311 *et seq.*, 338, 340, 341, 345, 347, 352, 353.
TEST RECORDS OF SOME PETROL ENGINES (A. E. Berriman), XIII. 83.
Test Records of Petrol Engines, XIII. 121.
Testing Metal under Internal Hydraulic Pressure, XIII. 64, 70.
THERMAL PROPERTIES OF GAS MIXTURE, A DIAGRAM OF THE, AND A DIAGRAM FOR THE CALCULATION OF NOZZLES (W. H. Gibson), XIV. 659.
Thread, Whitworth, Date of Introduction of, XI. 175, 215.
Threads in Screw Gauges, Fit of, XI. 216.
..... High Crested, XI. 183, 192.
Threshing Machine, Weight of, XII. 78.
..... Power required for, XII. 84.
Tilston Two-Cycle Engine, XIII, 529.
..... Criticism of, XIII. 534, 536, 544.
Tolerances to be allowed in Screw Gauges, XI. 182, 199.
..... Formula for, XI. 201.
Tolerances, Method of Dimensioning, XI. 132, 133, 134, 135, 137.

Tooth Angles for Spur and Bevel Gears, XIV. 414.
..... Strength, Formula for, XIV. 697.
Torque Curves, XI. 5.
..... per lb., Formula for, XI. 5.
..... required in Starting Gears, XI. 63, 65, 67.
Tractive Effort of Motor Cycle Engine, XIII. 347, XV. 706.
..... Adhesion, XII. 114.
..... Force of a Horse, XII. 437.
..... Resistance, XII. 74, 78, 106, XV. 583, 705, 783, 839.
TRANSMISSION, SUGGESTED LINES OF DEVELOPMENT IN (Capt. S. Bramley-Moore), XV. 261.
Transmission, Efficiency of, of Tractor, XII. 106.
..... for Motor Cycles, XIII. 261.
Tungsten Steel, Effect of varying composition of, at High Temperatures, XIV. 41.
Tungsten, Influence of, on Valve Steels, XIV. 66.
Turbine, Internal Combustion, XIII. 109.
Turbulence, XIII. 174, 197, 198, XV. 760.
Twisting Moment for Shafts, Nomogram for, XII. 209.
Two-Cycle Engines, Desiderata in Design of, XIII. 525.
TWO-CYCLE ENGINES FOR MOTORCYCLES (E. Thston), XIII. 519.
Two-Cycle and Four-Cycle compared, XIII. 476, 501, 520.
Two-point Ignition, XII. 518.
Tyres, Cause of Reduced Wear on, on Electrical Vehicles, XIII. 415.

Universal Joints, F. N., XIII. 336, 340.
..... Pressure on, XIII. 311.
"Utility Test," XIV. 320.

VALVE FAILURES AND VALVE STEELS IN INTERNAL COMBUSTION ENGINES (Dr. L. Aitchison), XIV. 31.
Valve Failures, Methods of ascertaining causes of, XIV. 89.
..... Steels, Influence of Carbon on, XIV. 66.
..... Cobalt on, XIV. 81, 111.
..... Vanadium on, XIV. 66.
..... Temperatures, Effect of inequality of, on warping, XIV. 90, 105, 109.
..... Petrol Consumption on, XIV. 95, 97, 98, 254, 286.
..... Method of measuring, XIV. 89, 101.
Valves, Case-Hardening for, XIV. 87, 100, 112.
..... Cast Iron for, XIV. 82, 88, 89.
..... Cause of Scaling in, XIV. 38.
..... Causes of Failure of, XIV. 34.
..... Distortion, Best Steels for avoiding, XIV. 58.
..... Effect of Width of Valve Seating on Temperature, XIV. 83, 96.
..... Methods of dissipation of heat from, XIV. 32, 75, 78, 86, 95, 96.
..... Materials for, XIII. 282.
..... Pitting of, XIV. 96, 99.

Valves, Running Temperature of, XIV. 32, 94.
..... Steels for, XIV. 521.
..... Suggested Steels for, for varying conditions, XIV. 63.
Vanadium, Influence of, on Valve Steel, XIV. 66.
Variation in Discharge of Pumps for Hydraulic Gear, XI. 232.
Velocity, Gas, Effect on Consumption of, XIII. 177.
Vena Contracta for Measuring Flow of Liquid, XIII. 465.
Volume Swept, Nomogram for, XII. 215.
Volumetric Efficiency, Factors Contributing to, XIII. 102.

Water and Air Cooled Engines, Weight of, compared, XIII. 94.
Weight per H.P., XI. 3.
..... of Locomotive, XI. 4.
..... Motor Cycle Engine, XIII. 370.
..... Air and Water Cooled Engines compared, XIII. 94.
Whitworth Thread, Date of Introduction of, XI. 175, 215.
WHY PETROL MUST GO HIGHER IN PRICE (Dr. W. R. Ormandy), XIV. 649.
Wilkes Engine, XIII. 542.
Windage, XV. 705, 706, 707, 712.
Windmills, Power of, XII. 115.
Wöhler Machine, Test Pieces for, XV. 488, 491, 504, 505.
..... Tests, Advantages of, XV. 503.
..... Calculation of Stress Imposed in, XV. 477.
..... Effect of Variation of Cycles of Stress in, XV. 497.
..... on Alloy Steels, XV. 473.
..... Carbon Steels, XV. 471.
..... Case-Hardening Steels, XV. 472.
..... Cold-Rolled Brass, XV. 468, 489.
..... Duralumin, XV. 469, 489.
..... Wrought Iron, XV. 466.
Women, Employment of, XII. 13, 460.
..... Labour, XII. 431.
..... Work done by, XI. 26, 401.
Wood, Structure of, XII. 125.
..... Percentage of Moisture in, XII. 127.
..... Case-Hardening of, XII. 129.
..... Amount of Shrinkage of, XII. 131.
..... Effect of Seasoning on, XII. 132.
..... different methods of Heating on, XII. 135.
..... Sturtevant Drying Kiln for, XII. 138.
WORKS ORGANISATION (By A. W. Reeves and Cecil Kimber), XI. 371.
Works Organisation, Alterations during Manufacture, XI. 399.
..... Layout of Plant, XI. 372, 389, 393, 396, 397.
..... Scrap, Treatment of, XI. 393, 398.
WORM GEAR AND WORM GEAR MOUNTING (By F. W. Lanchester), XI. 79.

Worm Gear, Accommodation Piece for, XI. 117, 128.
..... Assemblage, Good and Bad Designs of, XI. 125.
..... Backlash in, XI. 126, 158, 170.
..... Cause of Variation in Efficiency of, XI. 84.
..... Comparison with Bevel Gear, XI. 159.
..... Conditions for Design of, XI. 90.
..... Cutting of, and Worm Wheels, XIV. 409, 425, 430, 437.
..... Daimler-Lanchester Standards for, XI. 118.
..... Design, XV. 288, 318, 334, 354, 366, 794.
..... F.J., Pressure Angle, XV. 800.
..... F.J. and Lanchester compared, XV. 303, 321, 333, 336, 358, 362, 368.
..... Interchangeability in Mounting, XI. 131.
..... Introduction of Cheese Type Roller Bearing for, XI. 110.
..... Location of, Discussed, XI. 91, 148.
..... Lubrication of, XIV. 436.
..... Methods of Mounting, XI. 101.
..... Method of Preparing Sections of, XI. 93.
..... Necessity for Accuracy in, XI. 115.
..... Number in Use in U. S. A., XIV. 428, 434.
..... Objections to Adoption of Hindley Type of, XI. 145.
..... Points necessary for successful design of, XI. 149.
..... Precautions necessary in Mounting, XI. 101, 113.
..... Professor Kennerson's Tests on Efficiency of, XI. 83.
..... Proximate Contact between Teeth of, XI. 95.
..... Radius in Ball Bearings for, XI. 130.
..... Reasons for Slackness of, on Shaft, XI. 143.
..... Spiral Ball Thrusts for, XI. 105, 144.
..... Temperature of Running of, XI. 107.
..... Tolerances to be allowed in Mounting, XI. 139.
..... Types of, XI. 79.

Yield Point of Steel, Difficulty in Determining, XI. 317, 344, 351, 358.

PLATES.

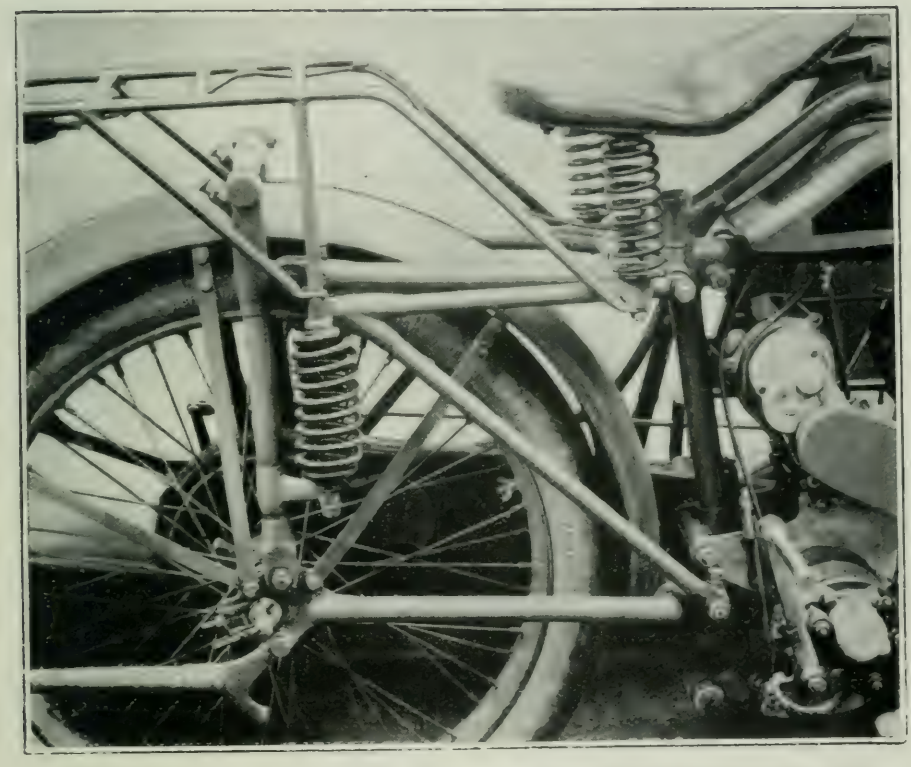


FIG. 13.—Matchless Standard Model.

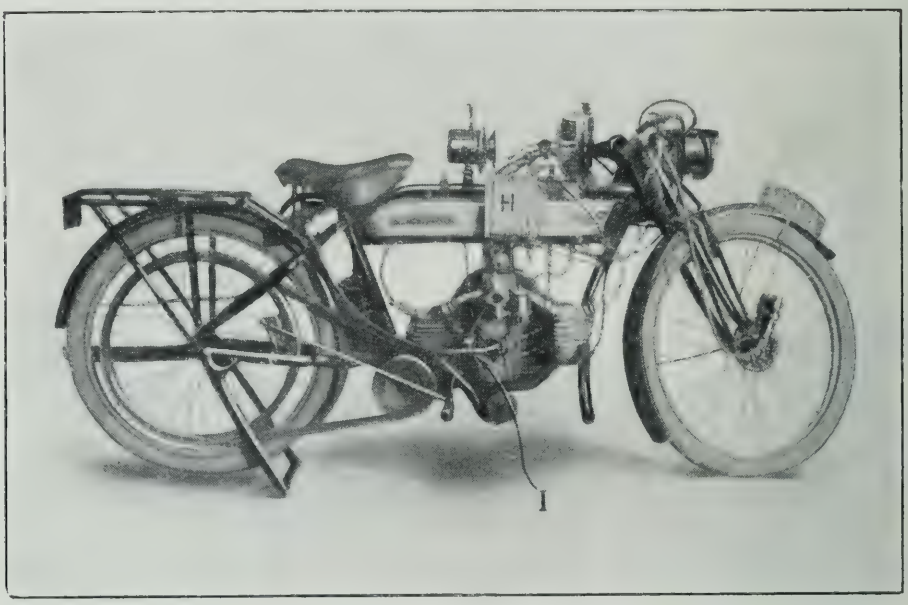


FIG. 2.

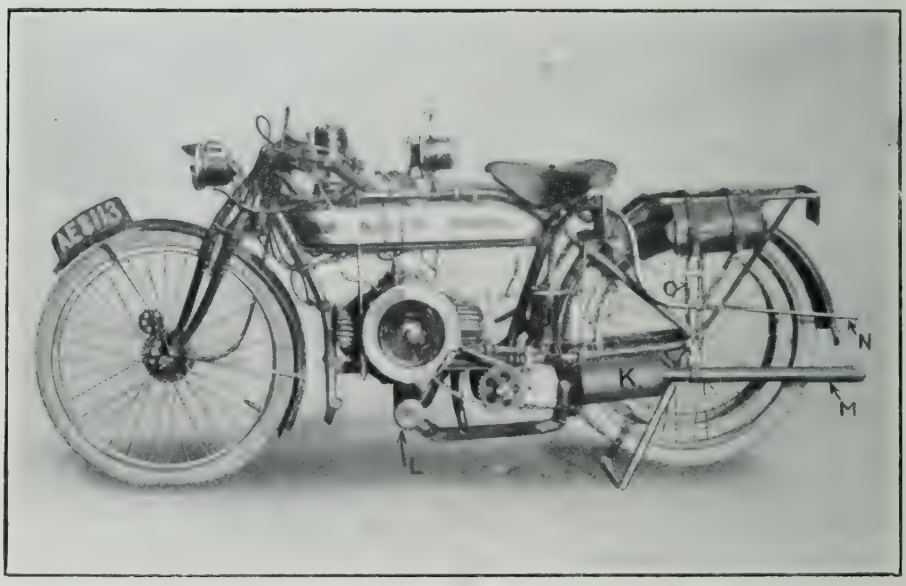


FIG. 3.

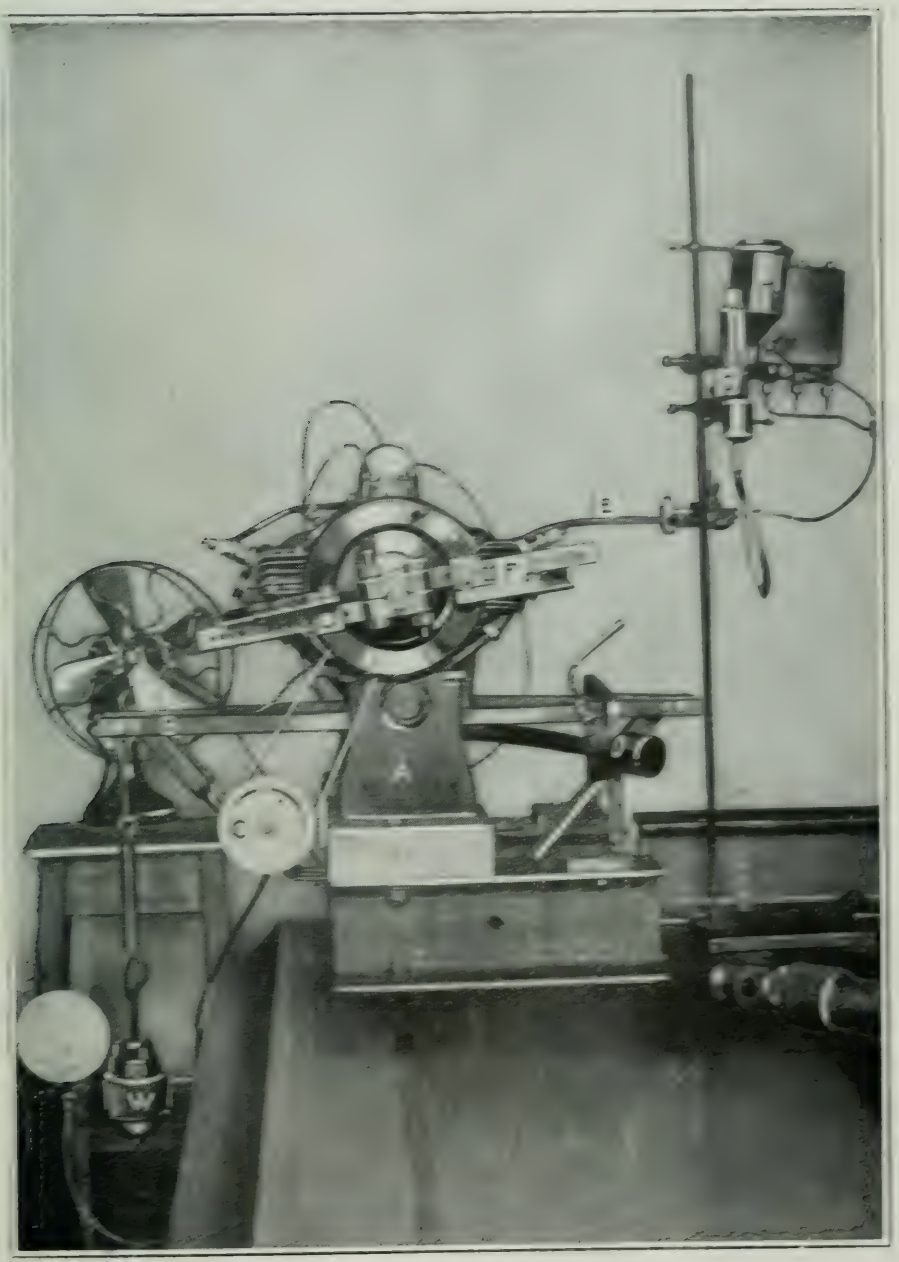


FIG. 4

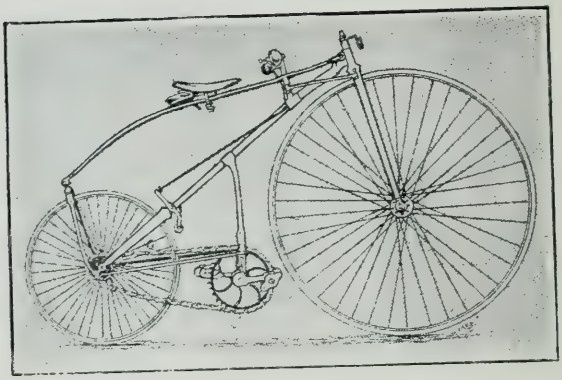


FIG. 1.

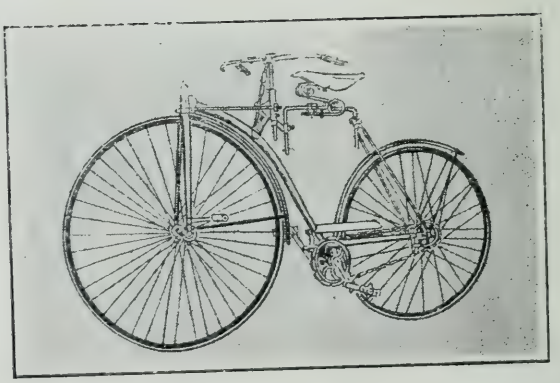


FIG. 2.

FIG. 3.

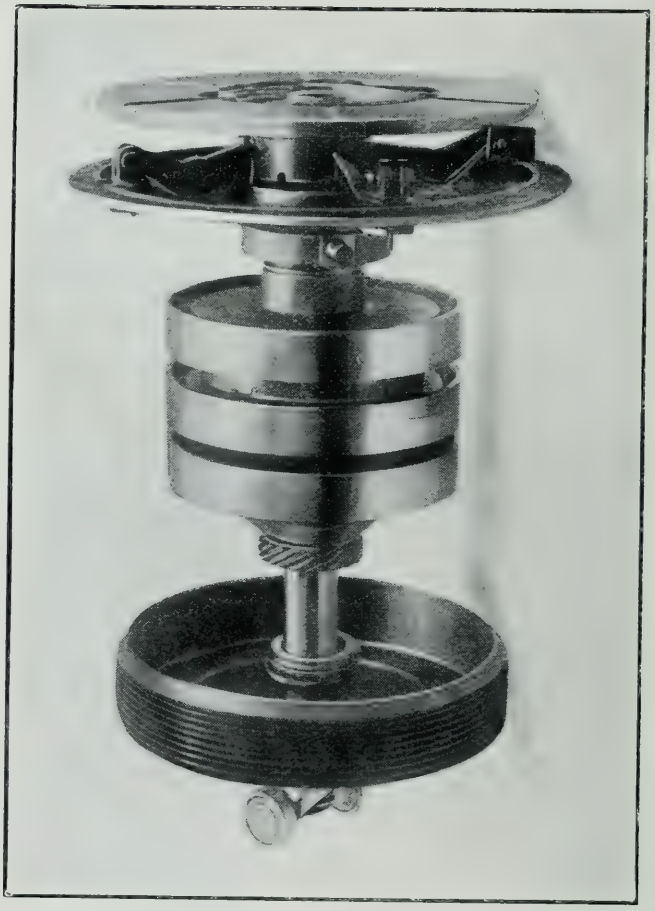


Fig. 54.

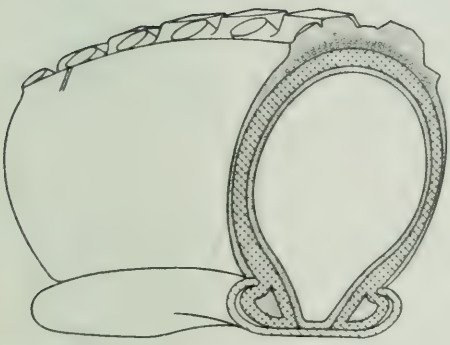


FIG. 12.

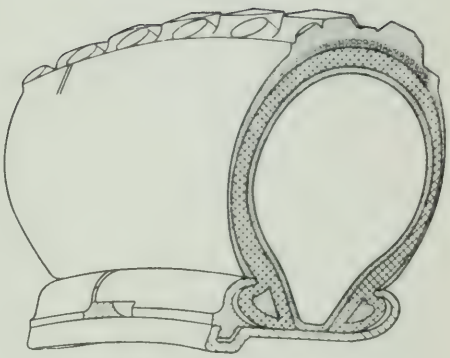


FIG. 12a.

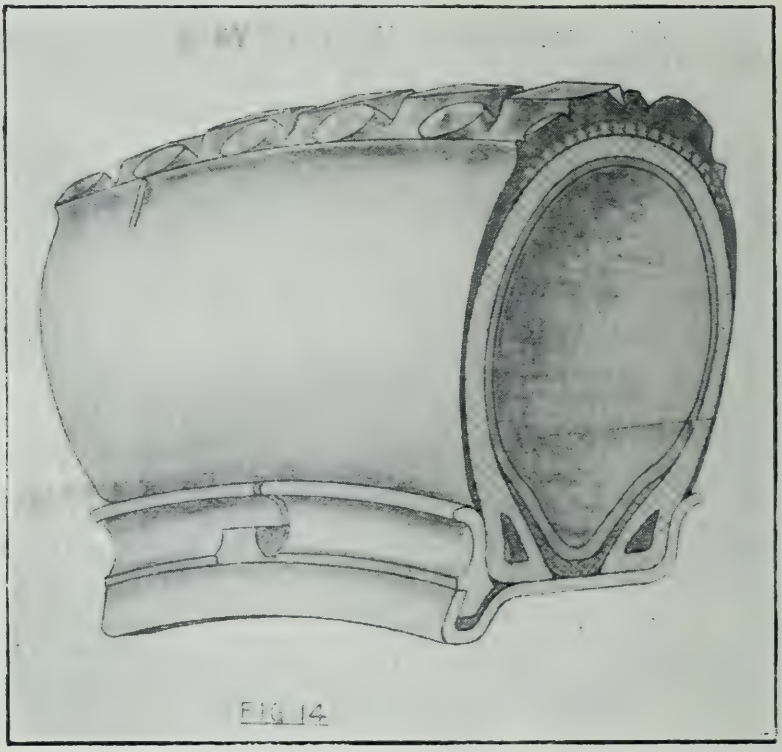
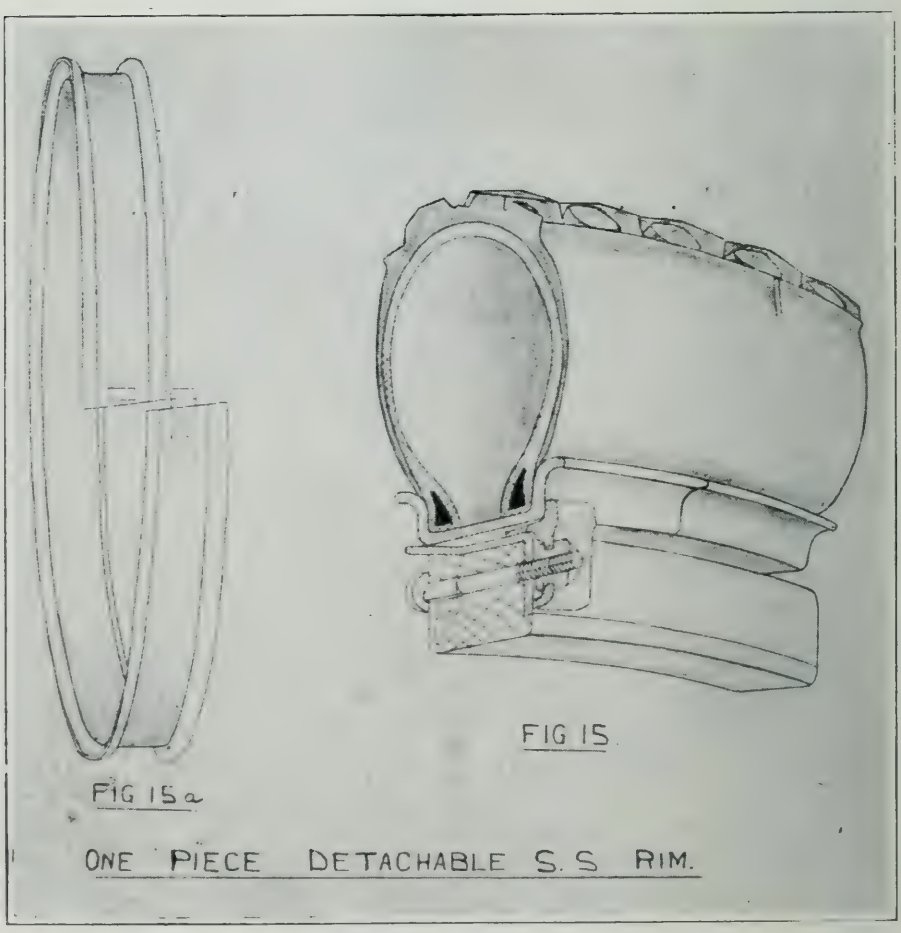


FIG. 14.



FIGS. 15 AND 15A.

Fig. 19.

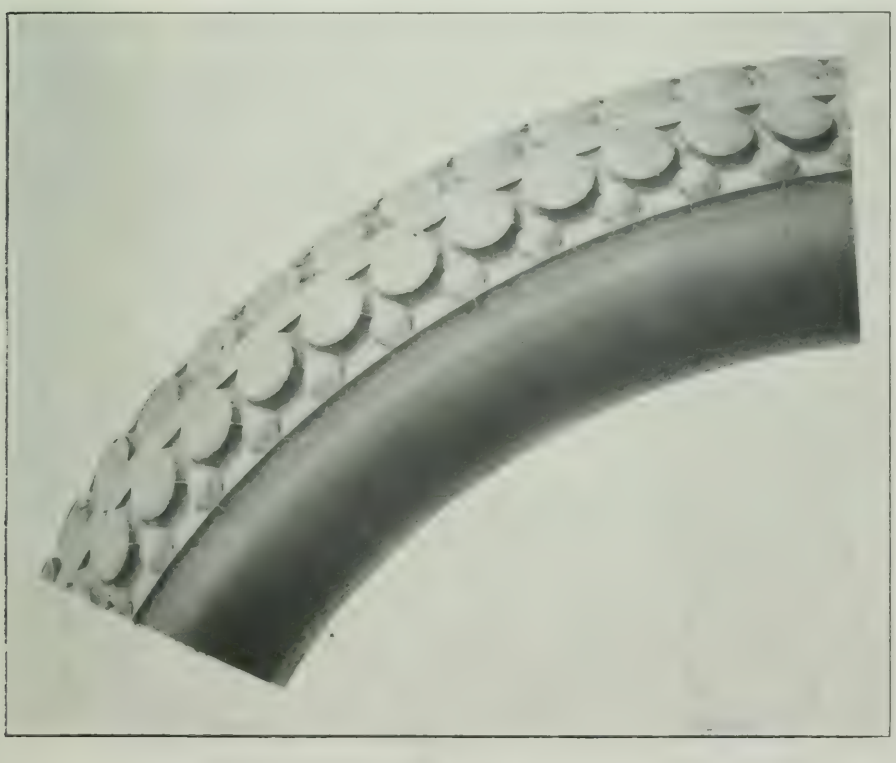


Fig. 21.

FIGS. 25 TO 25D.

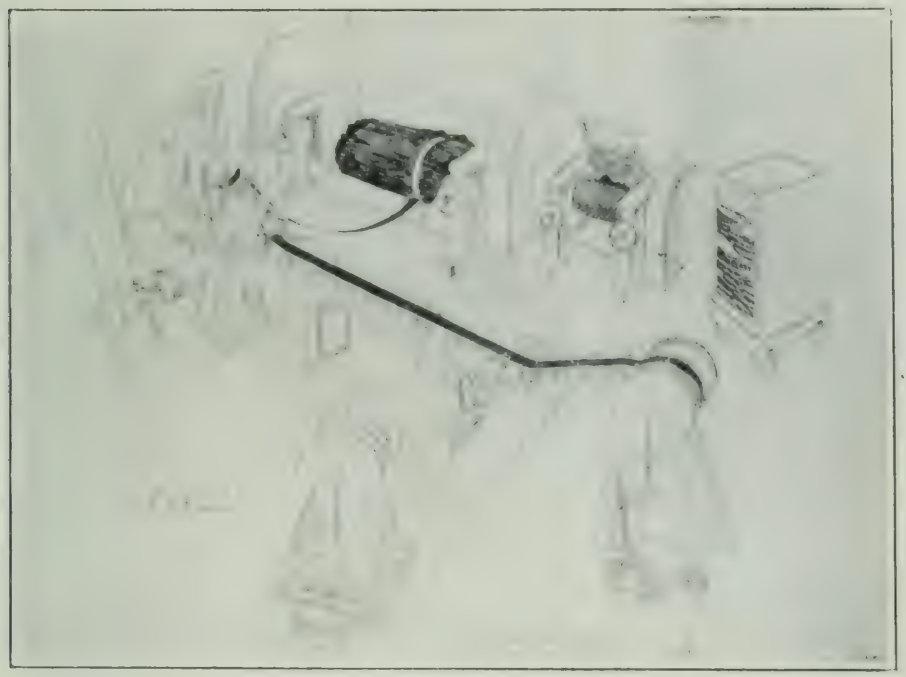


FIG. 33.

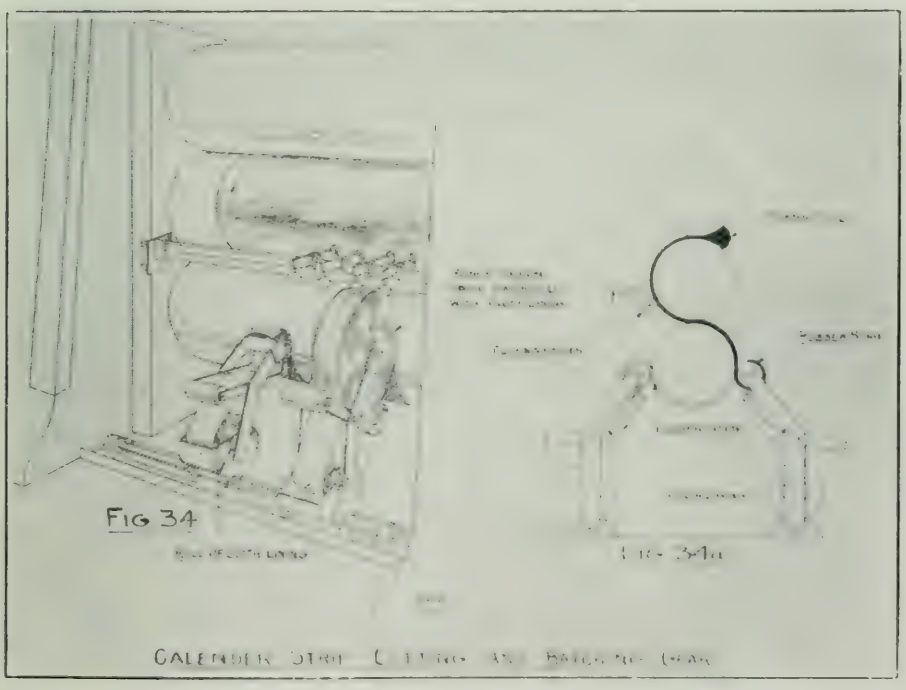


FIG. 34.

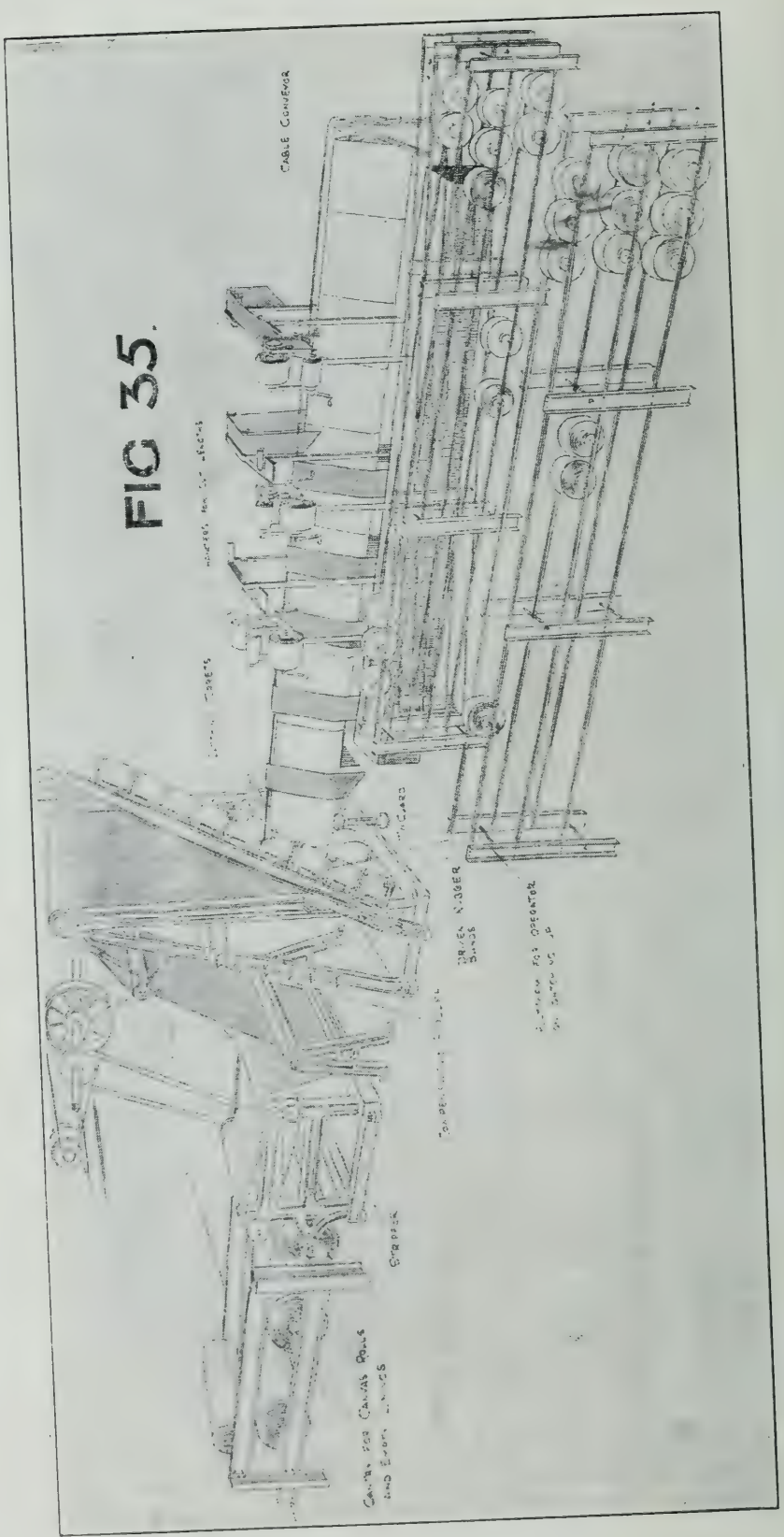


FIG. 35.

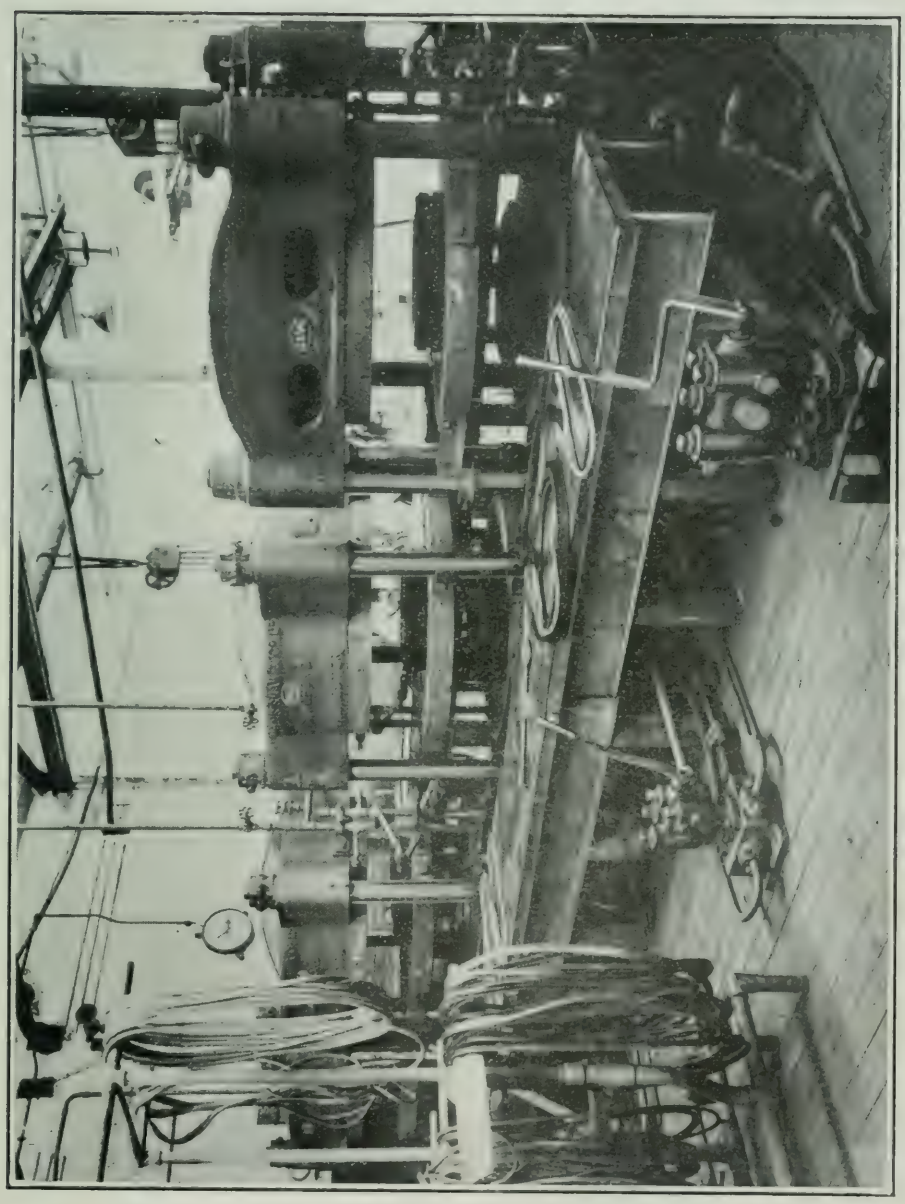
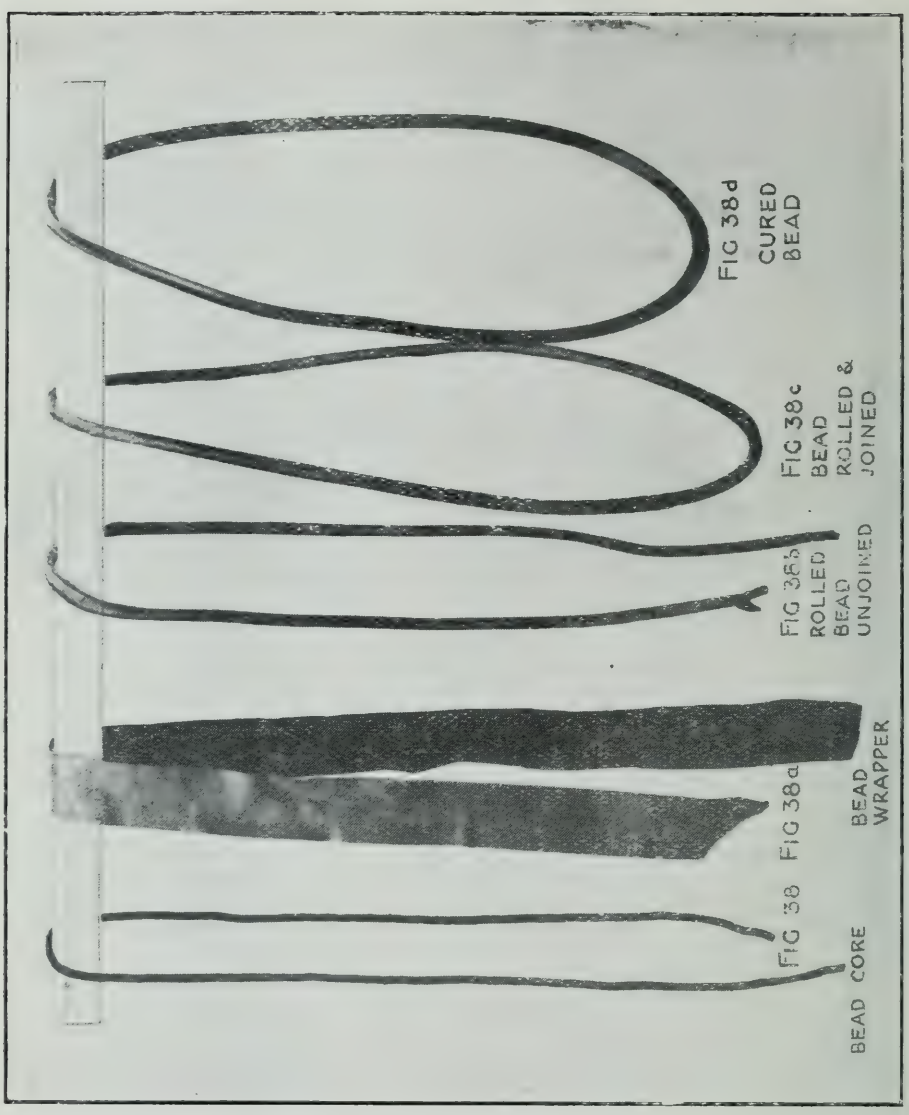
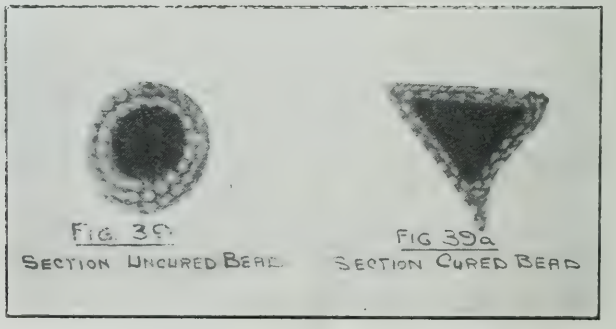


FIG. 37.



FIGS. 38 TO 38D.



FIGS. 39 AND 39A.

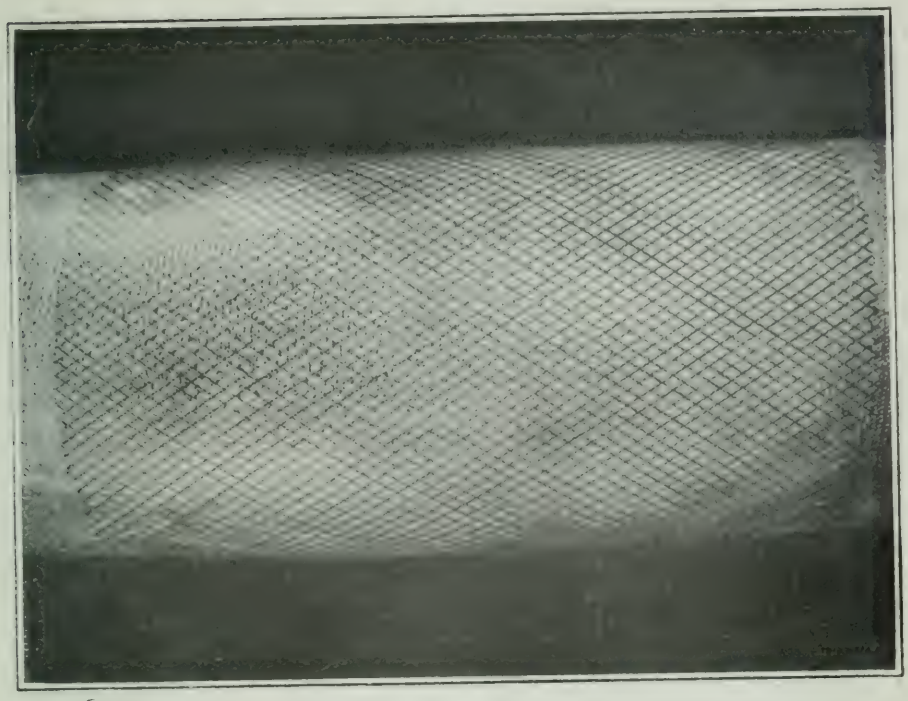


FIG. 43.

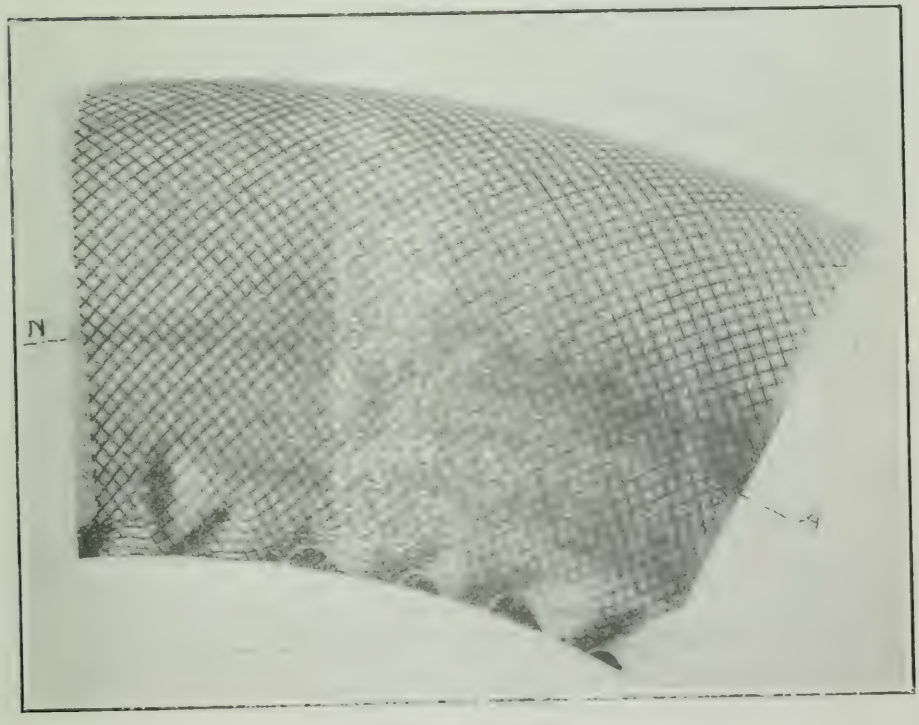
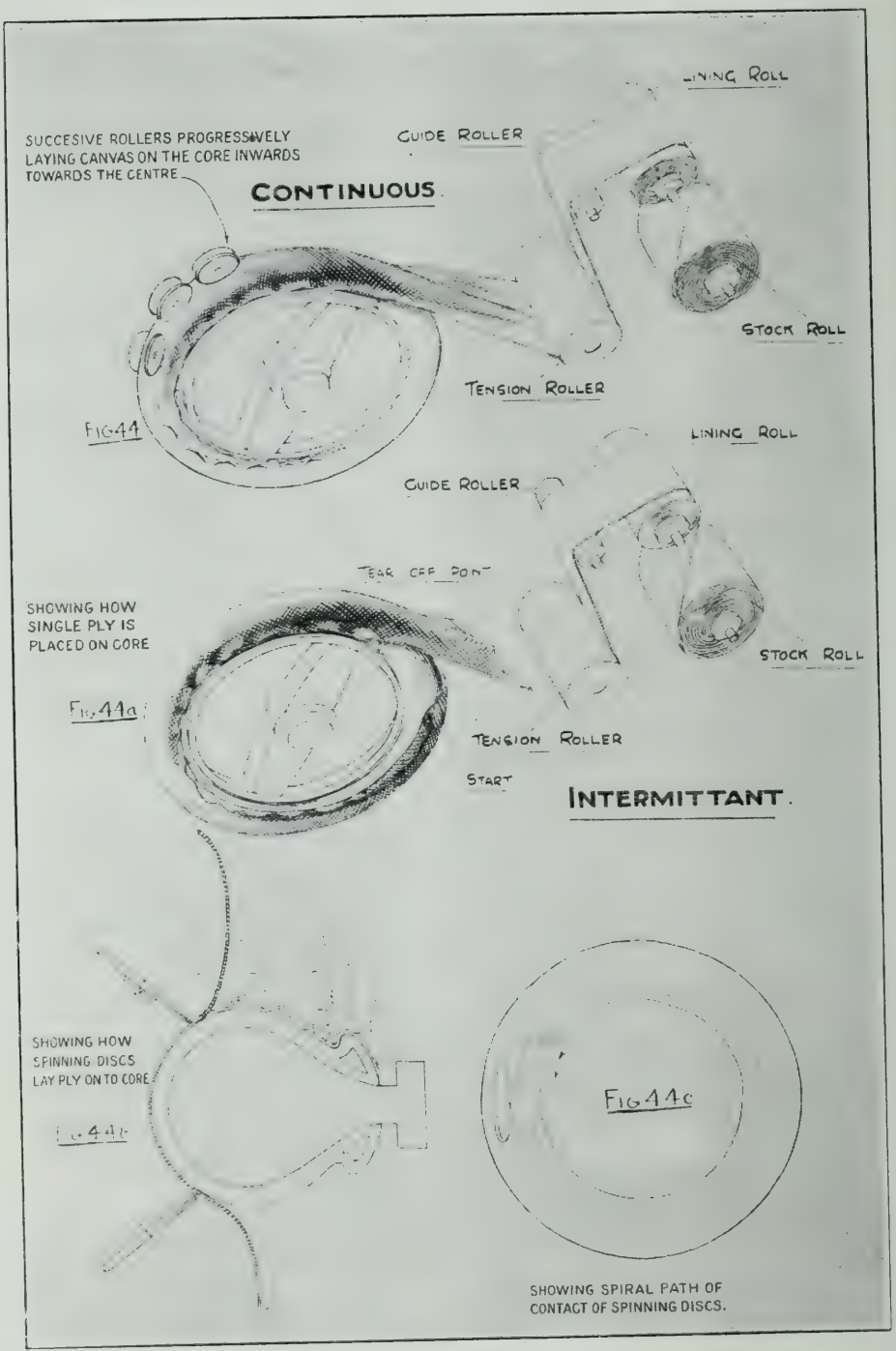
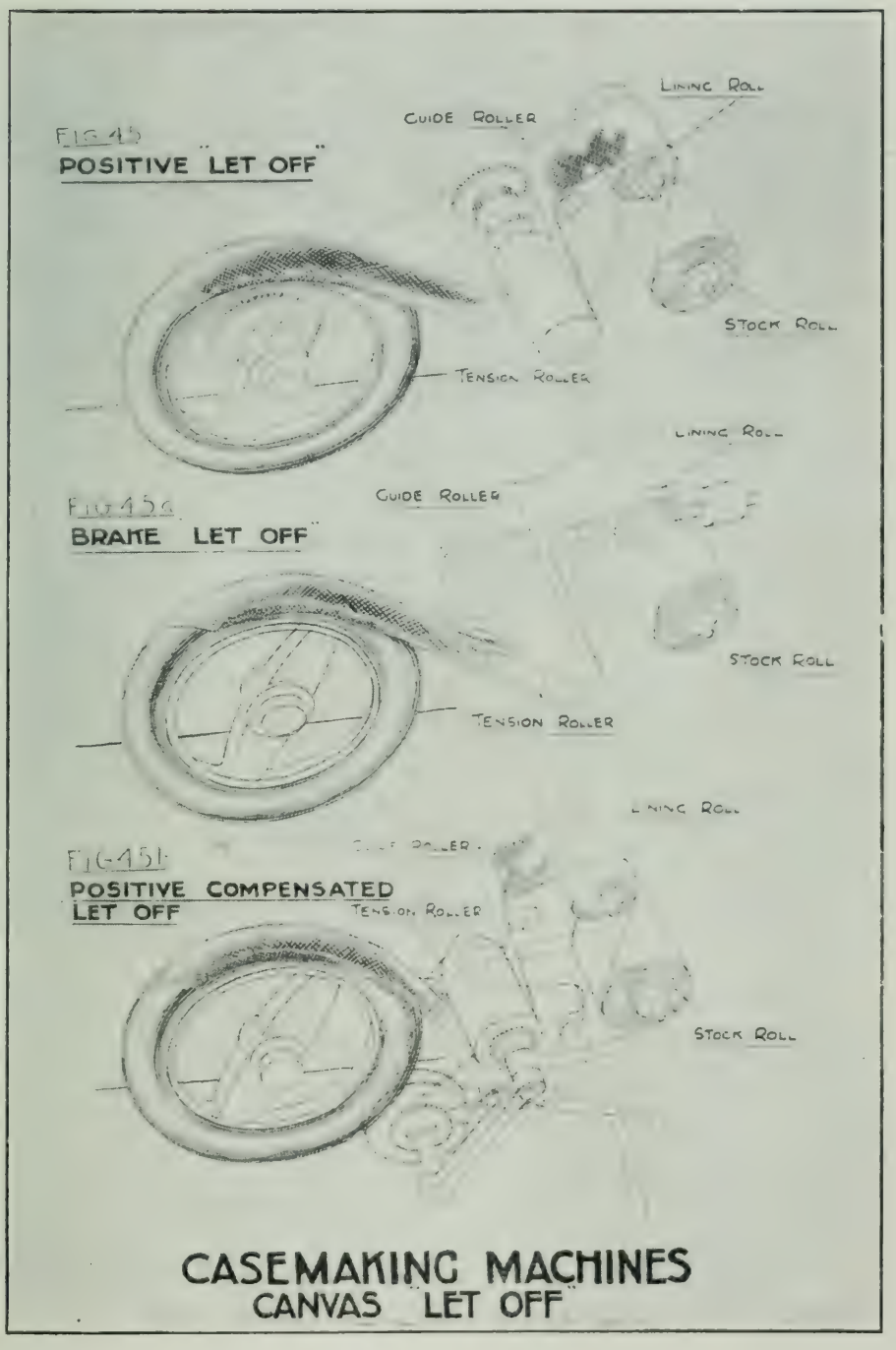


FIG. 43A.



FIGS. 44 TO 44C.



FIGS. 45 TO 45B.

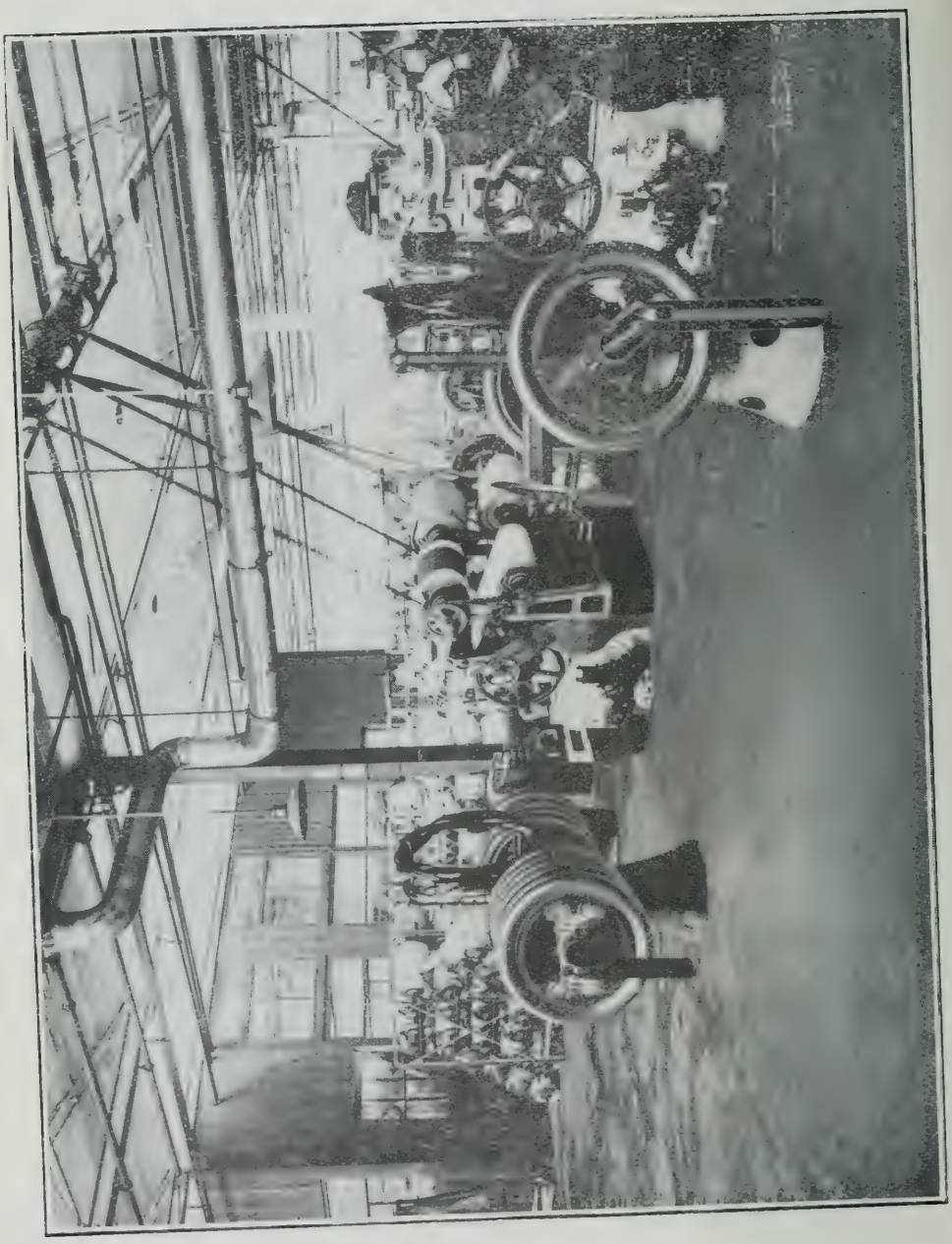


Fig. 46.

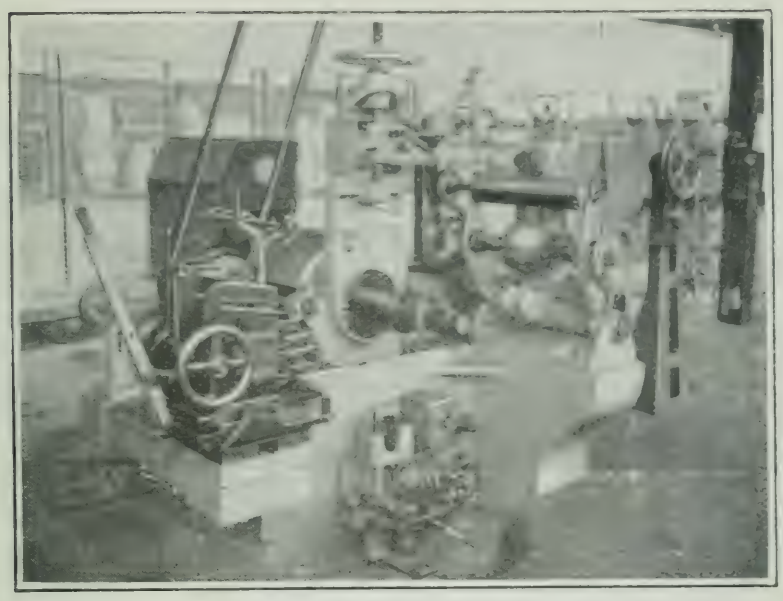


FIG. 46A.

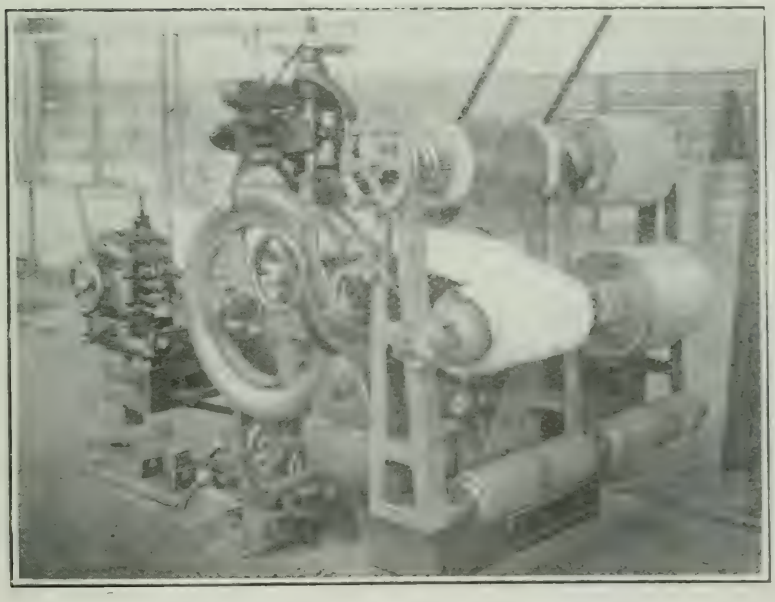


FIG. 46B.

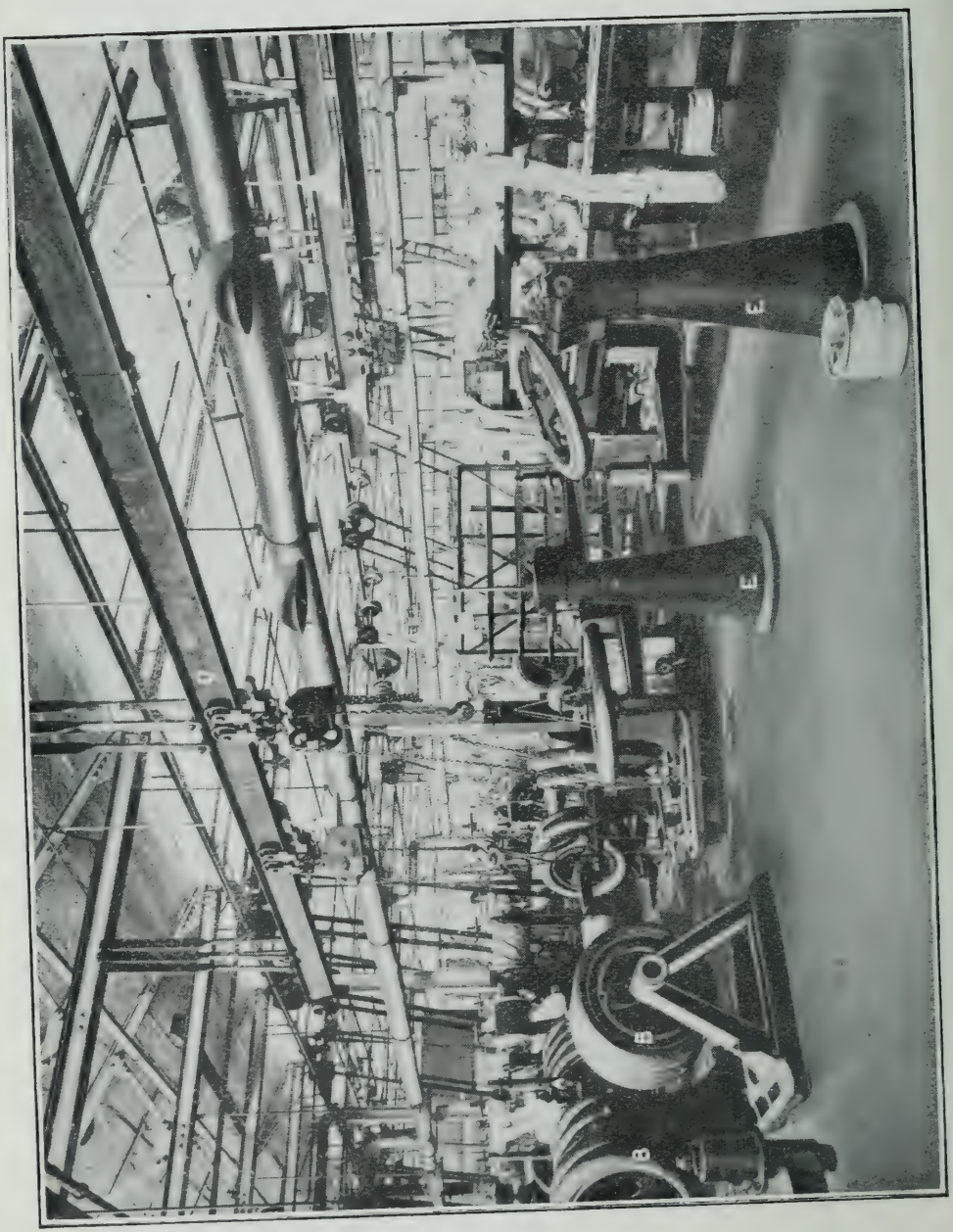


FIG. 47.

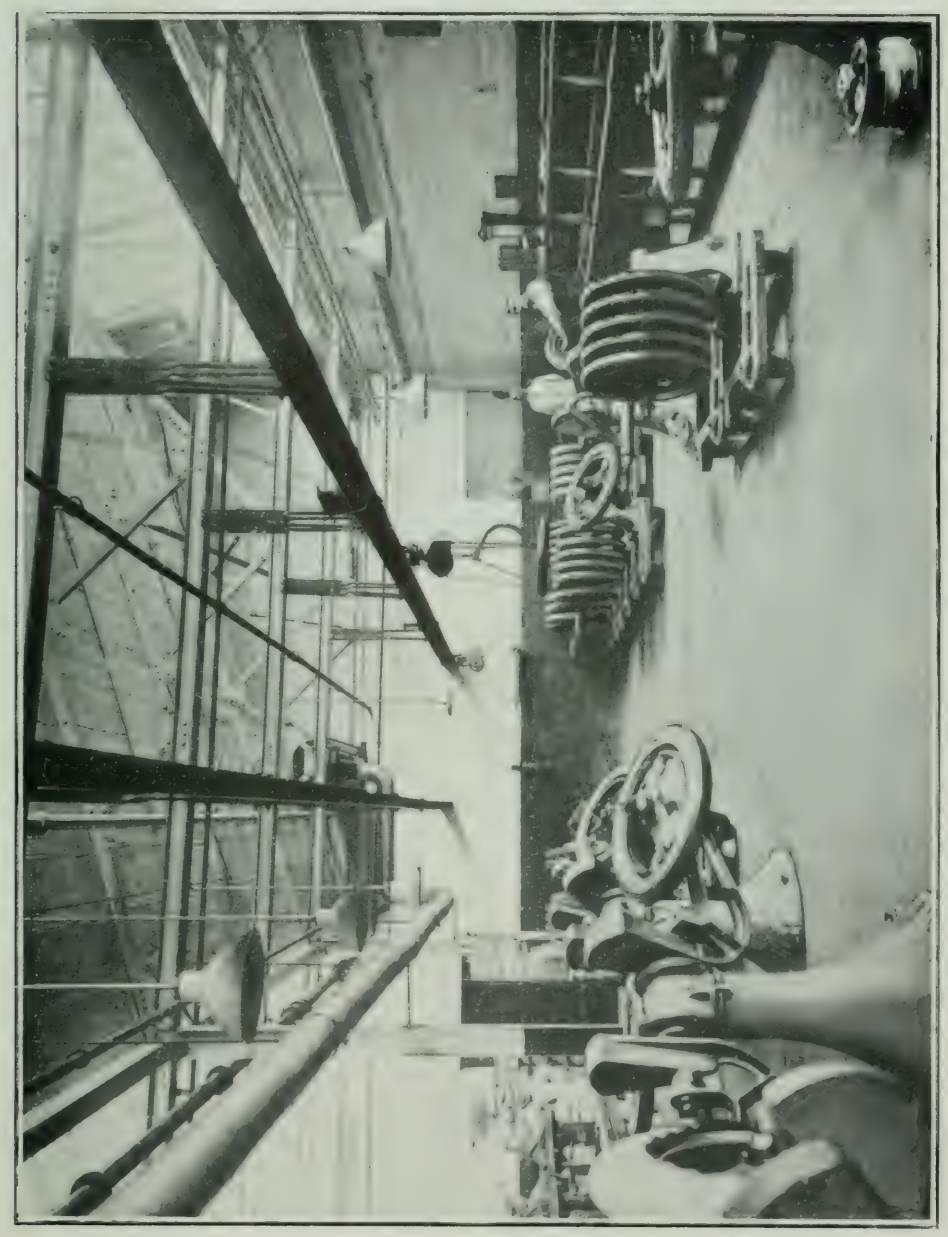


Fig. 48.

Fig. 49.

FIGS. 50 AND 50A.

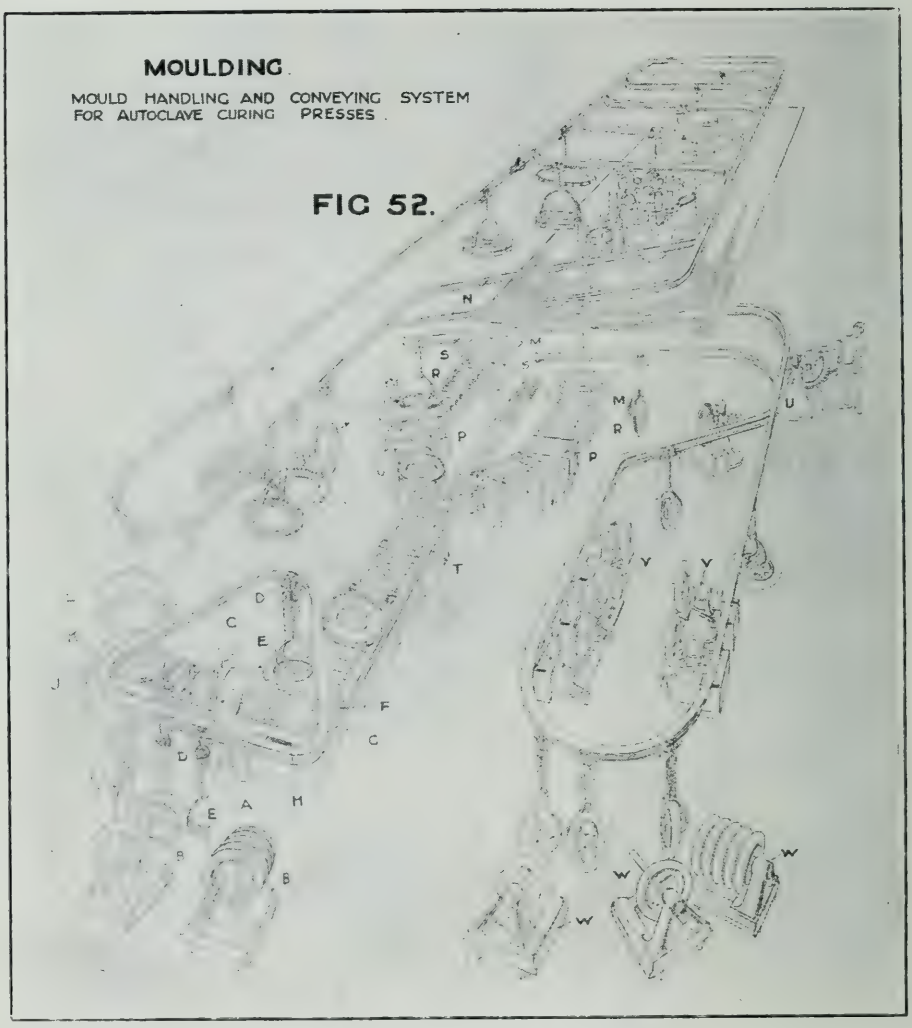

MOULDING.
MOULD HANDLING AND CONVEYING SYSTEM FOR AUTOCLAVE CURING PRESSES.
FIG 52.

Fig. 52.

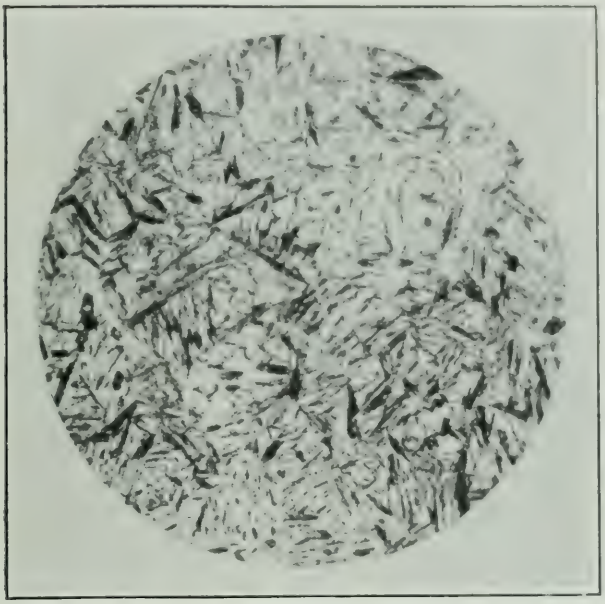


FIG. 9.

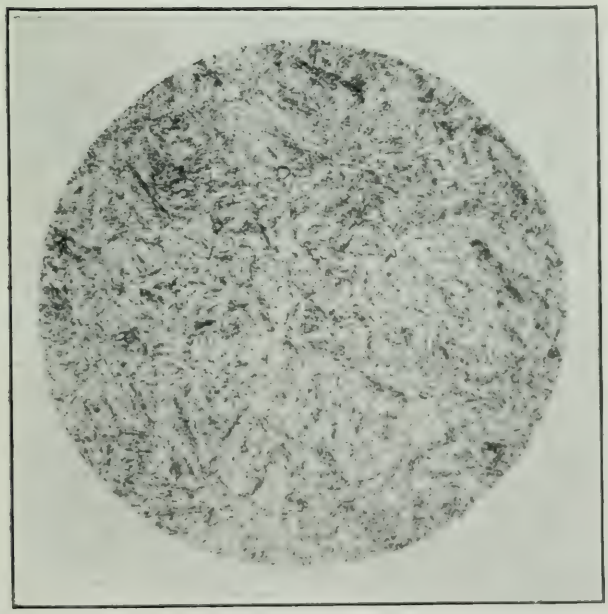


FIG. 10.

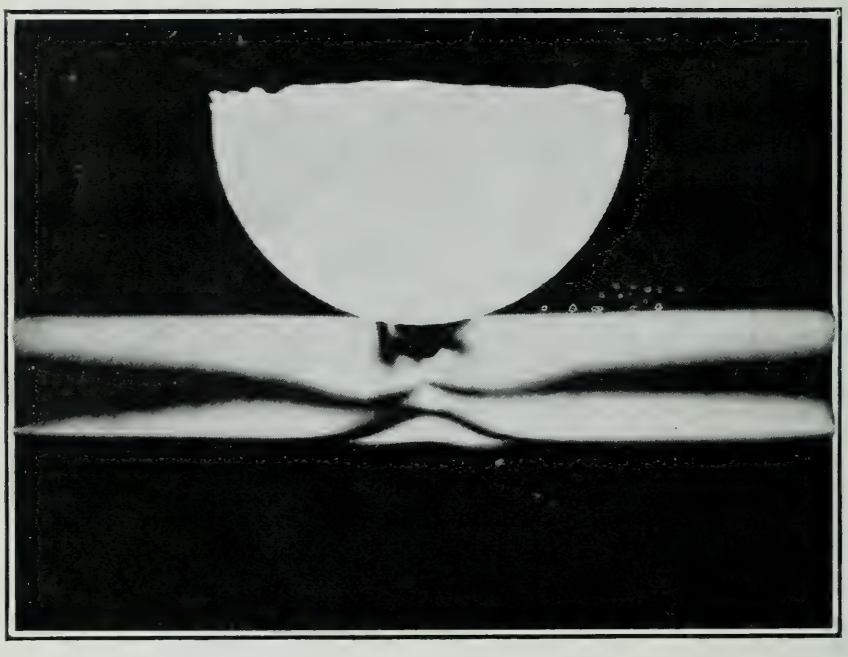


Fig. 11.

The stress is applied by means of a wheel over the area at which the V notch occurs. The dark continuous line is the neutral axis which runs up towards the notch. The other dark areas are the colours which represent the strains, and in a black and white photograph are no guide to the amount of stress at these points. The shape of the neutral axis does, however, show a high concentration of stress at the V notch.

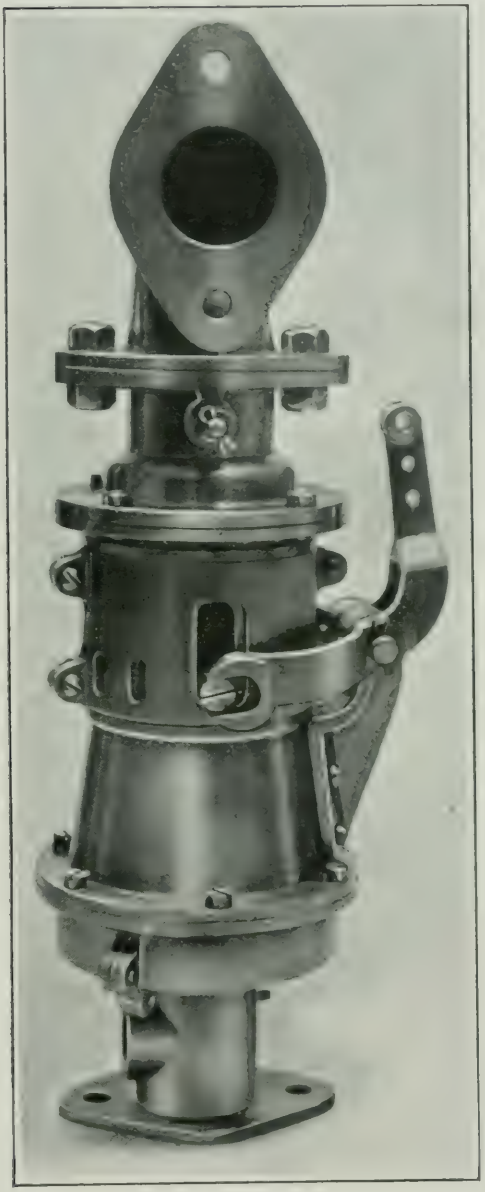


Fig. 2.

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